CORRESPONDENCE

Environmental Management: the Precautionary Principle and Null Hypotheses

A RECENT paper (Calver *et al.* 1999) exemplifies an approach to environmental problems which, though common, is often inappropriate, and may indeed be counterproductive in confrontational situations. An examination seems called for.

Let us look first at the way in which the Precautionary Principle is presented in a number of official international documents:

"... a precautionary approach ... may require action to control inputs ... even before a causal link has been established by absolutely clear scientific evidence." (Second Conference on the North Sea, 1987)

"... the principle ... applies especially when there is reason to assume that harmful effects, are likely to be caused ... even where there is no scientific evidence to prove a causal link" (Paris Convention for the Prevention of Marine Pollution, 1989)

"... to take action to avoid potentially damaging impacts ... even where there is not scientific evidence to prove a causal link ..." (Third Conference on the North Sea, 1990)

"Where there are threats ... lack of full scientific certainty should not be used as a reason for postponing measures to prevent environmental degradation" (Bergen Ministerial Declaration, 1990; UNCED, Rio de Janeiro, 1992 and Convention on Climate Change, 1992, are almost identical)

"... preventive measures are taken when there are reasonable grounds for concern ... even when there is no conclusive evidence of a causal relationship ..." (Convention on Protection of the Environment of the North Atlantic, 1992)

These statements at the international level all indicate that scientific proof, as usually understood, is irrelevant when the Precautionary Principle is applied. These formulations ask at most for "reasonable grounds for concern". "Full scientific certainty" may indeed stick in the throat of scientists. There is no such thing — at least in environmental matters. One can hope at best for high probability.

A possible rewording of the principle, taking into account the lack of certainty and the role of probability in all environmental sciences, would be:

"Any proposed action involving disturbance of the environment should not be undertaken without consideration of its probable effects. Only if those probable effects can be shown to be within acceptable limits should the disturbance be allowed to proceed."

Essentially this formulation (like many others, including all those above) puts the onus of proof on those proposing the disturbance. This is a most important principle.

In situations addressed by the Precautionary Principle, the question at issue is not usually whether a proposed disturbance has no effect on the system. Indeed only a bold — even foolhardy — ecologist would make such a claim. It is rather a question of the magnitude (and perhaps the sign) of such an effect — is it large enough to be worth bothering about? In other words, the question is one of estimation.

In the research situation, the environmental scientist (along with most of his biological colleagues) is often concerned to prove whether a particular effect occurs. But the alternative supposition — that the effect does *not* occur — is unprovable. One must be content to set up, as an Aunt Sally, a null hypothesis — that there is no effect — and then aim to show that, *if* this hypothesis were true, the observed results would have been very improbable. This conclusion is usually regarded as a disproof of the null hypothesis, one is advancing the boundaries of scientific knowledge.

But if one sets up a null hypothesis which is incompatible with the existing corpus of scientific knowledge, and attempts to disprove that, one is no further advanced whether one fails or succeeds. If the hypothesis is that Father Christmas lives at the North Pole, disproving it may be worth while if one lives within a culture which believes in Father Christmas. Otherwise, it is wasted effort. The null hypothesis is worth disproving only if it is credible. If, as is usually the case, it is part of the existing set of scientific beliefs, it is, from this very fact, credible. In the scientific world, the existence of Father Christmas is incredible: hence a hypothesis about his residence is not worth disproof.

In general, if a null hypothesis is *prima facie* incredible, the traditional approach is inappropriate. Why should one take any trouble to disprove an incredible hypothesis? Such an exercise is not called for in applying the Precautionary Principle. There is no need to set up a null hypothesis or to disprove it.

Calver and his colleagues (Calver *et al.* 1999) discuss the risks of Type 1 error (rejecting a true hypothesis) and Type 2 error (accepting a false hypothesis), but do not consider what has been called Type 3 error (asking the wrong questions) — which is often involved when a null hypothesis is set up and tested in environmental science.

It has been said that an insect's footfall echoes in the farthest corner of the Universe. But let us look only at an ecosystem. Any ecosystem is connected internally by an all-pervasive web of causation. Like a spider's web, every remotest part of which responds to a vibration in one corner, so all parts of an ecosystem are changed by a modification anywhere in the system. The changes may be small — may even be strictly negligible — but they exist. To deny this is to deny the whole corpus of background knowledge; yet this is what one does when one entertains a null hypothesis that a disturbance has no effect. The effect will be there, large or small. Any value within the system is affected by any event elsewhere in the system.

Any particular quantity that one might measure within an ecosystem can be regarded as a point in continuum which is constantly in flux, as events elsewhere in the ecosystem pull it this way and that. Its value at a particular point in time has no special interest; rather than measure this particular value, one would wish to describe the characteristics of the continuum of values of which it happens to be the present representative.

Likewise with changes within the system. Any disturbance of the system will result in changes throughout it. The change in a particular quantity may be characterized by a central value and a penumbra of uncertainty around it. A null hypothesis of no change pricks out a single point in this continuum of possible changes, and gives it a special status that it does not deserve. It may conceivably happen that, for a particular component, subject to tugs and thrusts from all directions, these tugs and thrusts happen at a particular point in time to be exactly in balance. But this zero shift is one point in the continuum of possible shifts, and chance of its happening exactly so — that there is no net effect — is strictly infinitesimal. Shifts of varying magnitude, in various directions, are the order of the day.

The approach of attempting to disprove a null hypothesis that a disturbance has no effect — a hypothesis which is *prima facie* untenable and implausible, indeed strictly incredible — is counter productive from the point of view of those questioning the acceptability of disturbance. By framing the question in terms of null hypothesis of no effect, the defender of the status quo hostages to those whose interests lie in causing and justifying the disturbance. If the test of the null hypothesis fails to show a "significant" effect, the disturbers can then say "The scientists have not been able to show any effect of the disturbance", and proceed as if there were in fact none. Of course, those who use it know that a hull hypothesis is essentially unprovable — it can only be **disproved**. But a failure to disprove it is too easily misrepresented as a positive proof.

Calver and his colleagues (Calver *et al.* 1999) may modify changing the significance level and hence the power of the test; but these efforts are pointless because the null hypothesis is itself incredible — even if it is not disproved, one will not believe it. Marsupial populations, as well as all other variable within the system, will inevitably be influenced by logging, even though the effect may be very small. Disproof of this null hypothesis, as of the hypothesis that Father Christmas lives at the North Pole, is irrelevant because one would not believe them even if not disproved. To attempt to disprove the "no effect" hypothesis is as much a waste of effort as would be an attempt to disprove the Father Christmas hypothesis.

One should take one's cue from those who framed the Precautionary Principle. This principle puts the onus on the person or group proposing a disturbance to demonstrate that it will have no effect on the system, or that the effect will remain within acceptable limits. The supposition that the disturbance will have *no* effect is *ex hypothesi* unprovable. To ask for such a proof of the proponents of disturbance would, then, be inappropriate. But to ask them for a demonstration that the expected effect is within specified and acceptable limits is reasonable, and this should be the policy followed by those questioning whether a proposed disturbance should be allowed.

Environmental scientists should not sell themselves short. By confining themselves to testing a null hypothesis (which they do not believe anyway), they are ignoring the fact that their science already contains an abundance of relevant information bearing on the problems in question. They should muster and present this knowledge, rather than pursue the futile goal of testing an incredibile hypothesis.

Action should always be guided by the best information available at the time of decision. In applying the precautionary principle, complete ignorance of the results of a proposed disturbance implies that disturbance should be postponed until some information is obtained. But this is rarely the position. Background information will almost always enable one to make *some* forecast, however vague and uncertain, about the effect. And any such information makes possible a more informed approach than complete ignorance. The environmental scientist should not be backward in offering this information. That it is sketchy and subject to error should not lead him/her to hold back; but (s)he should always be prepared to make clear the uncertainties surrounding the information available, and to quantify the uncertainty as far as possible.

Despite the irrelevance of a null hypothesis in ecosystem management, an experiment performed nominally to test whether an effect differs from zero can usually be used for the more appropriate purpose of estimating the magnitude of the effect on the (much more realistic) assumption that it **does** exists. As an example, one may cite the work of Abbott and Van Heurck (1985) on bird populations in jarrah forest. They pursued the usual course of testing a null hypothesis, and wrote that they found ". . . no significant decrease in relative abundance of jarrah forest birds soon after logging . . .". Yet their figures showed a decrease in the number of records per hundred metres of transect per hour from 37.5 in 1982 before logging to 26.0 in 1983 after logging. Thus, the best estimate of the change following logging was a decrease of 31% irrespective of its "significance". Such an estimate is the most important contribution that an ecologist can make to discussion of the desirability or otherwise of a proposed disturbance. The maximum-likelihood estimate of the magnitude of a change is exactly the value which would be compared with zero under the irrelevant null hypothesis, irrespective of how good or bad the estimate may be; for many purposes the cloud of uncertainty surrounding this best estimate is equally important, and needs to be quantified. That is, the distribution of likelihood in the continuum of which the best estimate is the centre needs to be ascertained. This distribution can often be represented by the error estimate for the central value. Once estimates are available of the magnitude of the ecosystem changes which would result from the proposed disturbance, together with their uncertainty, their importance can be assessed in the light of whatever value system is adopted, and their values and probability can be balance against the values and probability claimed for the other effects of disturbance — those for the sake of which it is proposed. The value systems used will reflect considerations outside the scientific realm — economics, aesthetics, ethics, sociology — as well as questions like irreversibility which are strictly scientific. The advantages claimed for the disturbance can then be weighed against the adverse effects expected, and an informed decision can be made. Indeed, if the disturbance itself is an continuing process, continuous monitoring of the effects can enable the original forecasts to be updated, and any limits placed on the disturbance can be relaxed or tightened as forecasts are improved.

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RESPONSE

In a recent paper we suggested means for quantifying the precautionary principle and aiding resolution of disagreement in cases calling for its application. In his commentary on this paper, Goodall (1999) makes four important points: (i) that it is reasonable to ask proponents of a disturbance to demonstrate that any possible consequence will lie within specified and acceptable limits, (ii) that a null hypothesis is worth disproving only if it is credible, (iii) that one should beware of asking the wrong question when framing environmental studies, and (iv) that scientific proof as generally understood does not apply to the precautionary principle. We are in substantial agreement with all these arguments. However, as explained below, we differ in our assessment of their implications.

SPECIFIED AND ACCEPTABLE LIMITS TO BE MET BY PROPONENTS OF A DISTURBANCE

Many commentators on the precautionary principle support a shifting of the balance of proof, requiring proponents of a disturbance to demonstrate that the consequences will lie within specified limits (e.g., Peterman 1990a,b; Peterman and M'Gonigle 1992; Underwood 1997). We endorsed this position in our original paper, using logging in the jarrah forest of Western Australia and its putative impacts on jarrah forest mammals as our primary example. We noted the various measures in place to ameliorate possible impacts and argued that the value of these measures could not be assessed without measuring the response variable of changes in the population trends of selected species of jarrah forest mammals. We proposed three indicator mammal species on the basis of features of their biology and an assessment of demonstrated deleterious impacts of logging on mammal populations elsewhere in Australia and in the northern hemisphere. To address the question of logging impacts on the selected species, we suggested monitoring their population trends with a view to detecting declines of 40% over two years in the Common Brushtail Possum *Trichosurus vulpecula* and 20% over two years for the Western Ringtail Possum *Pseudocheirus occidentalis*. In deciding if these rates of decline have been exceeded, one may err by concluding that a decline of the specified magnitude has taken place when it has not (Type I error), or by concluding that such a decline has not taken place when it has (Type II error). We acknowledged these possibilities and assigned probabilities of 0.20 to each as standards that should be met in assessing if a decline of the chosen magnitude occurred. As stressed in Calver *et al.* (1999), these figures were proposed for illustration only, because choice of final values in these matters should be reached by discussions involving all stakeholders.

We believe that this approach is consistent with asking proponents of a disturbance to place possible consequences of the disturbance within specified limits. Furthermore, if the consequences of the errors can be costed, with, perhaps, a special monetary value assigned for non-economic costs such as aesthetic values, economic consequences of the error rates proposed can be assessed. We find this philosophy very similar to that presented in Goodall's final paragraph.

SCIENTIFICALLY CREDIBLE NULL HYPOTHESES

Few would argue with the contention that every action has consequences and that therefore every disturbance will have an environmental impact. The critical question, as Goodall (1999) highlights, is to assess the size and direction of the impact. Thus the "no impact" hypothesis is a rhetorical device suitable for generalization, but in a given situation it is replaced by a more specific null hypothesis with scientific credibility. Taking an example from Calver *et al.* (1999), the null hypothesis for the case of the Western Ringtail Possum, based on the figures given, would be: "Over two years the population of the Western Ringtail Possum has not declined by more than 20%". We contend that this null hypothesis is plausible, since it is scientifically valid to postulate that any mortality caused by logging is not