SCIENCE for the EARTH :
Can Science Make the World a Better Place?

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JOHN WILEY & SONS ***Chichester – New York – Brisbane – Toronto - Singapore***

Galloping Gertie and the Precautionary Principle – How is Environmental Impact Assessed?

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pp.197-236.

Introduction

There should be a law which states that every scientist intending to carry out an environmental impact assessment (EIA) must first be presented with a statue of a dog. Not just any animal, of course; the statue, or more properly a memorial, would be of a dog which once belonged to the daughter of Leonard Coatsworth from Washington State, in the USA. The dog's name was Tubby and, alas, poor Tubby was killed by Galloping Gertie on a November morning in 1940. The death of Tubby, and indeed that of Galloping Gertie, who died at the same time, was a direct result of something which has always been a feature of scientific investigation, but whose effects are now increasingly becoming the focus of many environmental issues. The problem can be summed up thus:

> *Q.:* *What's the difference between a correct and an incorrect theory?*
> *A.:* *About ten years*.

This dictum, for which I am indebted to Dr Hans Joosten of Utrecht, neatly expresses the fact that any existing scientific model is almost certain to be modified or even completely overturned by subsequent discoveries. Predictions for much more than 10-20 years hence, based on present-day models, have a significant probability of ultimately proving to be, if not wrong, then at least not quite right. The greater the complexity of the system being modelled, the greater the probability that this will prove to be so. It is a fact which environmental consultants would do well to consider before making unduly optimistic predictions about the likely impact of a particular development. They should also reflect on the fate of poor Tubby.

Bridge building, oscillations and vortices

The Tacoma Narrows Bridge was designed by one of the leading bridge designers of his day, Leon Moitseief. At the time, it was considered to be a daring, innovative example of engineering, but ultimately its intended purpose was simply to bridge the Narrows. It was expected to do this within the limitations of known engineering safety tolerances because no-one was going to pay for a bridge which was unsafe to use; but that was exactly what Moitseief eventually provided for the people of Washington State.

Within weeks of opening, it was observed that, in any sort of wind, the bridge would oscillate up and down, very slowly. The Tacoma Narrows Bridge, rapidly renamed 'Galloping Gertie', was proving to possess aeroelastic properties which had never before been encountered or even dreamed of within bridge-building circles. While sightseers came simply to gaze in amazement, engineers came to measure the process. They considered the various stresses on the fabric of the bridge, and declared that, although the motion was undoubtedly unusual, the bridge was nevertheless quite safe and could remain open even when 'galloping'.

This view prevailed until one November morning in 1940. On that day, a 40 mile/hr wind blew steadily down the Narrows and people began observing a new phenomenon. Whereas, on all previous occasions, Gertie had simply oscillated up and down, now she was going up and down and beginning to twist from side to side. By the time Leonard Coatsworth and Tubby reached the bridge in Leonard's car, the combined oscillations were just getting into their stride. Once on the bridge, further progress was clearly impossible. Mr Coatsworth, Tubby and several other motorists found themselves stranded on a bucking leviathan. Police closed the bridge and managed to persuade all of the occupants in stranded vehicles to run to safety - all, that is, except Tubby, who absolutely refused to leave the vehicle. Newsreels of the time show Mr Coatsworth, minus Tubby, staggering from the bridge like a drunken man as steel and concrete heave and roll around him in the manner of a confused sea. A short while later the newsreels also show the entire structure finally tearing apart and collapsing into the Narrows, carrying poor Tubby and Gertie to a watery grave. [see video: https://www.youtube.com/watch?v=XggxeuFDaDU]. When asked later about the cause of the phenomenon, Moitseief simply said 'I have no idea.' He died not long after the disaster, a mystified and broken man.

With the Tacoma Narrows Bridge, Moitseief had employed engineering techniques which were well tested and thoroughly researched. He had taken the design just a little way into new territory by making the ratio of width to length slightly greater than had been attempted before, but, because there was no existing evidence to suggest otherwise, no problems were anticipated with this. It was only 52 years later, in 1992, that science finally caught up with Nature and was able to show that Gertie's self-destructive change from vertical to lateral oscillations was a result of von Karmen vortices. The effect of such vortices on a structure such as Gertie were an entirely unknown phenomenon in the 1930s.

The Ferrybridge Disaster

Despite the name, Ferrybridge is not another bridge disaster, nor was it a project intended to push back the frontiers of engineering. The construction of cooling towers for power stations was, by the early 1960s, a well-established and somewhat prosaic engineering process. The several towers of the Ferrybridge Power Station in Yorkshire were all constructed using standard techniques. Nothing novel here.

Except that, on the afternoon of a windy November day in the early 1960s (perhaps the patron saint of engineers takes a break every November), one of the concrete and steel towers in the complex began to behave in a distinctly novel way. It started to oscillate, wobbling like a badly made pot on a potter's wheel. The wobbles became increasingly violent until, as anyone who has attempted pottery will recognise, the instability moved rapidly and inexorably towards an unavoidable and catastrophic climax. Millions of fascinated TV viewers watched the evening news to see the violent wobbles cause the flailing upper parts of the tower finally to shear off. The whole 375-foot construction then collapsed in a heap of rubble and twisted steel.

What happened next was almost as incredible. Even before the dust had settled on the TV screens, an expert was wheeled on explain how the disaster had happened. With the day's gale gusting at up to 85 mile/hr, huge vortices caused by adjoining cooling towers had created such powerful forces of suction that the fabric of one particular tower had been quite unable to cope with the resultant stresses and had simply disintegrated. I well remember the sense of wonder which this revelation provoked. 'Good Heavens!', we exclaimed as we listened to this expert, 'then why were the towers not designed either to avoid, or to cope with, such vortices? After all, gales of 85 m.p.h. are not so unusual in Britain.' The answer, of course, lay in the undisclosed fact that the possible development of these vortices, and their subsequent impact, had been unsuspected by engineers *until* they were observed to occur in the real world, at Ferrybridge.

Engineering and the real world

In effect, to carry out an experiment and then develop a model which can provide useful predictions, a scientist will try as far as possible to provide an environment which is entirely controlled or measured. Even bridge builders, however, in their relatively rigidly-defined world of concrete and steel, recognise the limitations of their craft when faced with the unpredictable real world. Rod Rhys Jones, an engineering consultant, observes:

> *How much can engineers possibly know about the future? Bridges are there a long time. Engineers have to know the unknowable, predict the unpredictable. They have to be able to account for Acts of God*.[1]

Bridges are certainly there a long time, some even pre-date the origins of some valued wildlife habitats, but although the common objective of both bridge-building and conservation is to maintain the working fabric of a bridge or habitat as far into the foreseeable future as possible, the bridge-builder always knows that, should the need arise, another bridge can be built to replace the existing structure. For the majority of valued wildlife habitats, such artificial reconstruction is simply not possible - existing attempts at habitat recreation or regeneration have yet to show any significant level of success. They currently represent no more than the first stumbling steps of ecological engineers into a vast and largely uncharted territory. Over-confidence at such an early stage in the exploration of this relatively new field could all too easily lead entire species groups into blind alleys from which there is no return - unlike bridge design, the luxury of two bites at the cherry is not one of the characteristic features of extinction.

Klaus Ostenfield, Bridge Director of COWI Consultants, recognises that, in relation to projects where the bridge builder is also going into uncharted territory:

> *[The designer] runs the risk of having overseen something, so every time you make a long extrapolation in technology, you have to watch out and be very careful in order to make sure you see what you cannot see.*[1]

Such cautionary thoughts come from engineers who deal for the most part quite literally in concrete facts. The majority of bridge design is utterly predictable. The designer's biggest worry is that the real world will intrude on this ordered, measured and precisely defined exercise with some previously unrecognised mischief. For a bridge-designer the majority of facts are solid, and only a few elements of un-predictability are expected to seep into the final process.

The one major change in circumstances, when the engineer finally takes a design or a model from the laboratory out into the real world and begins construction, is that the design is then subject to the much more complex environment of the real world, and this may throw up a few surprises. Indeed, the common causative factor in the collapse of both Galloping Gertie and Ferrybridge was nothing to do with engineering *per se*. Both resulted from a series of atmospheric tricks which were, until that point, completely unknown to the engineer, and which only became apparent when the design was subjected to the the unpredictability of the real world.

It is important to understand that 'unpredictability' doesn't mean some Luddite view of a world which is hopelessly chaotic (though we will come back to that word), a world beyond any possibility of measurement and thus whose future behaviour is subject only to, in footballer Diego Maradonna's famous phrase, 'the Hand of God.' The word is used here very deliberately to contrast conditions in the outside world with the 'controlled conditions' of a laboratory environment. The very idea of experiments carried out in 'controlled conditions' logically indicates that everything outside these conditions must be 'uncontrolled' - *i.e.* unpredictable, or at least *not measured*.

The furore over 'cold fusion' centres, in part, over accusations that physicists Pons and Fleischmann failed to control their experimental conditions sufficiently closely, and that perhaps some of the products they observed were therefore derived from entirely unrelated and unrecorded external processes. This accusation may or may not be true, but it does illustrate the very rigorous application by the general scientific community of the concept of 'controlled conditions'. It also highlights the common problem which faces scientists when carrying out any experiment - namely the ease with which unpredictable factors can slip into what is a supposedly controlled environment. One of the biggest problems in maintaining this set of conditions is ensuring that the real world is not inadvertently allowed to intrude on the experiment. All scientists know the problem, even within the closed world of the laboratory - low readings from a dirty electrode, a fault in the deionised water supply, stray *Penicillium* mold on an agar plate...

In an engineering project, the actual process of design and construction rests largely on the use of materials and techniques which have already undergone exhaustive tests and trials under laboratory conditions. Othmar H. Ammann, chief designer of the Brooklyn Bridge, produced 'a design so precise that engineers considered the job half-finished when workmen appeared on the site, in 1959'. He knew that the 693 ft twin towers would need to support 150,000 tons of roadway and cable, and that the towers themselves would weigh 48,000 tons. He calculated, and was subsequently proved right, that the weight of the completed road deck would cause the suspension cables to sag by exactly 28 feet below their un-laden arc. He was able, because of laboratory tests which showed the behaviour of metals under differing conditions, to allow for summer expansions and winter contractions which would alter the height of the span by as much as 6 ft, and when the wind blew, enable the whole span to distort up to 14 ft sideways. Ammann was able to do this because the vast majority of steps in the design process were already robustly proven techniques, some of which went back to Roman times or even earlier. The construction process was almost entirely calculable, predictable and, in effect, simply represented a scaled-up version of controlled laboratory conditions.

Compared with this relative bedrock of certainty and arsenal of precisely defined measurements, the environmental consultant embarks on an EIA (while clutching a little statue of Tubby) to be confronted with a maelstrom of uncertainty, in which the majority of elements are uncertain or unknown and are anyway constantly fluctuating. Often, the only concrete facts are those which relate to the actual fabric of the proposal - the engineering details of the dam, factory, barrage, wind turbine, road, quarry or whatever. Virtually *every* other factor which must be measured or modelled is instead something which lies outside the predictable world of the controlled environment. Few elements of the environment have given up their secrets to definitive laboratory analysis. Yet the job of the consultant is not merely to understand how the ecosystem works initially, but then to determine the impact of a potentially profound change on this real and 'unpredictable' environment. The EIA consultant, in other words, is very much at the opposite end of the certainty-spectrum from the bridge-designer.

The unregulated environment

Another important difference between the bridge-designer and the EIA consultant is that bridge-design is heavily regulated. A whole range of safety codes must be followed, otherwise the designer faces prosecution, particularly if disaster strikes. The world of EIA is, in contrast, regulated in only the most general way. For example, under the Commission for the European Communities (CEC) Directive 85/337/EEC, member states of the European Union are required to ensure that EIAs are carried out under particular circumstances, but the regulations provide little in the way of practical guidelines relating to either content or quality. Within the UK, relevant regulations were generally enacted in the late 1980s and early 1990s – *e.g. The Environmental Assessment (Scotland) Regulations 1988* – and the official country conservation agencies have together arranged for the production of joint guidance relating to their functions within the EIA process, this guidance being in the form of the *Environmental Assessment Handbook*. Then again, though providing a usefully detailed analysis of the steps required to carrying out and evaluate an EIA, the *Handbook* is not part of the legislation, and is thus not legally-binding.

As far as the regulations themselves go, in the UK at least, the greater bulk of the legislation is concerned with determining whether an environmental assessment is required in the first place. In describing the practical (as opposed to the broad) nature of any Environmental Assessment (EA) and the Environmental Statement (ES) which emerges from that, the legislation is remarkably coy.

The intention of the CEC Directive is that developers should have considered the possible impacts of any proposed development at the earliest possible stage in their plans. Encouraging the developer to carry out the EA studies is thought to be one means by which a direct interest in environmental issues can be engendered more widely. Unfortunately, the translation of this philosophy into UK legislation embraces the concept to a greater degree than is perhaps helpful.

A developer is required to provide to the planning authority a statement which includes (1) a description of the physical nature of the development, (2) data with which to assess the *main* environmental effects, (3) an analysis of likely *significant* effects (both direct and indirect) on: humans, flora, fauna, soil, water, air, climate, the landscape, any interactions of the foregoing, material assets, the cultural heritage, (4) measures envisaged for avoiding, reducing or remedying effects and (5) a non-technical summary.

An ES may, but is not legally obliged to, include amplification and explanation about: (1) land-use requirements during construction & operation, (2) the main environmental characteristics of the production process, (3) the expected type and quantity of residues, (4) alternative approaches examined, (5) direct & indirect effects of using natural resources and of the creation of wastes, (6) the forecasting methods used for (5), and (7) difficulties caused by technical deficiencies and information gaps. It is recognised that the concept of 'effect' can include those of a secondary, cumulative, short/long term, permanent/temporary, or positive/negative nature.

This list of items for investigation is undoubtedly comprehensive, but it's rather like saying to a randomly picked person, a dog-owner, perhaps, 'Here's some fish-plates, girders and tarmac - now build me a bridge'. No statutory guidance is given as to the form which an ES must take; how the developer goes about answering these questions is left entirely open. The developer is simply told to provide sufficient information on all the listed issues for the planning authority to be able to make a judgement. If the information is not considered adequate by the planning authority the developer can be requested to provide more data until the authority is satisfied. In the absence of a set framework by which an authority can be expected to judge an EA, this represents something of an open-ended commitment on the part of the developer because planning, scheduling and budgeting is very difficult for something which may, ultimately, and at the authority's request, turn into a major ecosystem analysis. Even the consistently informative *EA Handbook* is sketchy in its treatment of the subject, stating blandly that no standard methods can usefully be provided.

This lack of formal provision to ensure that common methods are used between different EAs also means that planning authorities, faced with two successive applications which are concerned with largely similar proposals, may not be able to transfer the knowledge and experience gained from one study over into the process of judging the second proposal. Methods adopted in the two EAs may be so different that comparison of the results between the two may not be valid. Nor will the planning authorities be able to benefit from what should be an accumulating knowledge pool as more EAs are carried out. Similarly, developers are not easily able to draw on the experience and results of previous studies when carrying out their own investigations.

The other major, and possibly more important, problem with the EA philosophy, as practiced, is that the entire process is concerned only with the stages *prior* to the granting of planning consent. This arises from the laudable desire to ensure that environmental considerations are included at the earliest planning stages of a proposed development. This is translated into UK legislation in the form of, as we have seen, a potentially exhaustive review of *likely* effects *prior* to a development being given approval, but nothing in the legislation provides for *post-hoc* monitoring and alterations. The *Handbook* notes that monitoring after planning consent has been granted is possible through the provisions of planning conditions under, for example in England & Wales, Section 106 of the Town & Country Planning Act (1990). However, the *Handbook* also observes that such actions are 'seldom undertaken in the British context', nor indeed is it common practice elsewhere in the world.

The present system therefore has three crucial shortcomings:

1. the system fails to encourage any consistency of approach between developers and is thus a recipe for anarchy;

2. consequently neither developer nor planning authority can easily draw on the results of previous studies, nor is there any provision for the collation of such accumulating knowledge - each EA is a study unto itself;

3. the system is entirely predictive, concerned only with *possible* events in the future and, in practice at least, fails to continue impact assessment into the future to note *actual* effects - not being responsible for the subsequent outcome, EA teams thus have no direct incentive to make accurate predictions.

These flaws in the EIA system itself are then compounded by another, more potentially serious problem, more serious in that, with a simple change in the legislation all three failings listed above can be overcome, whereas this additional complication arises from something more deeply ingrained. It is an aspect of scientific culture which has given rise to many remarkable examples of scientific conduct in relation to EIA work and Public Inquiry advocacy in Britain. At its very worst, it can be described as scientific *hubris*, an arrogant assertion that science has all the answers. Its more normal expression is found either in the belief that, as Robert Earll of the Marine Conservation Society describes it, 'absence of evidence' is synonymous with 'evidence of absence', or as a scientific caution which demands that proof of harm be obtained before action be taken, whether or not that harm can be reversed.

The list of superficial, slap-dash, negligent, and in some cases frankly laughable EIA studies or statements which arise from this dangerous cocktail is long and not very edifying. Far too many EIA studies are in reality no more than a cosmetic treatment of the issue, merely providing an environmental gloss to a proposal, a smooth and politically soothing lustre whose foundation is more worthy of Helena Rubenstein or Yves Saint Laurent than of the Royal Society or the National Academy of Sciences.

Such studies claim to be professionally executed scientific assessments of the proposed development, yet many would be viewed with horror by the engineering profession if applied to the relatively predictable construction of bridges. Applying them instead to the much more complex problem of the environment requires both greater caution and broader vision - caution to avoid over-confidence and vision to see possible connections, the evidence for which may not, as yet, be available.

Finally, EIA scientists must also be prepared to overcome a particular philosophy which now seems to dominate much of the scientific community, as a result of which too many are reluctant to admit knowledge-gaps in their data and uncertainties in their predictions. Oddly enough, although portrayed in popular myth as an evil ogre plotting to push a scheme through by guile or subterfuge, the developer is often largely blameless for the poor quality of an inadequate EIA. Nor does the problem necessarily lie with the planning authority or the Secretary of State, because many of these poorly-executed EIAs are subsequently rejected. These particular problems arise instead as the result of a particular scientific culture which has been allowed to grow up and in some circumstances almost stifle the spirit of scientific enquiry. This culture could be called 'The Fear of Ignorance' and is discussed in Tom Wakeford's chapter.

EIA and Scientific Peer Pressure

The normal pre-requisite for publication in a respected scientific journal is proof that an experiment has been carried out in such a way as to ensure that all possible variables have been accounted for. Such an approach is designed for science carried out under laboratory conditions. It is a philosophy unsuited to investigations, no matter how objectively rigorous, in an open 'uncontrolled' environment. Once an experiment moves outside the laboratory environment it becomes impossible to control all variables. Rather than expending useless effort on trying to do so, it therefore becomes more important to identify those elements which can be measured, and those elements which cannot. In general, those which cannot be measured or controlled will outnumber those which can, given the time and cost constraints under which a developer is usually operating.

Many of these unregulated and unmeasured factors may have only a minor impact on the results obtained; such is the assumption of many studies. However, it is quite possible that hitherto unsuspected influences are also affecting the data obtained. For example, it is only with the development of the 'groundwater mound theory' by Hugh Ingram of Dundee University that it is now possible to explain changes in the vegetation of a peat bog some considerable distance from an afforestation scheme on that same bog. These observed changes are now understood to occur as a result of alterations in the overall shape of the bog water-table caused by water removal beneath the trees. Previously, such changes were attributed solely to the effects of grazing or lack of regular burning.

It is impossible to measure every aspect within, and connection to, an ecosystem. Papers which are concerned with environmental issues and which also aim to provide scientific illumination to a problem should in principle provide a list of *all* those factors which might possibly have a bearing on the results, but which were not investigated, together with an assessment of the possible implications of these omissions. This represents a type of scientific writing which is very different in kind from those papers which describe the more traditional type of controlled condition experiment. Such disarming honesty would make interesting reading and would probably provide a rich vein for further scientific discussion and research. Unfortunately such documents are currently rare animals.

The reason for this rarity is, at last in part, that scientists are reluctant to admit that they might have missed something, that their experimental method is not as comprehensive as they would like, or that cost and time constraints have simply rendered a proper analysis impossible. Indeed the scientific community at large and funding bodies in particular, do not welcome such admissions of omission. Papers have less chance of finding their way into print if they clearly state that certain aspects remain unstudied even though these may have a material bearing on the issue at hand, than those which simply do not acknowledge such gaps in the experimental method. Even academic reviewers will often be unaware of the full suite of features and connections which might have a significant bearing on a particular study. Ironically, it appears that being 'economical with the truth' is a better strategy than 'the truth, the *whole* truth, and nothing but the truth'. The derision with which scientists treat their colleagues in the 'social sciences' seems to intensify with the passing years. At least most social scientists *admit* who complex their subject matter is. This may seem harsh, but look at a range of scientific papers which describe an investigation into the natural world and consider what *other* factors may also have contributed to the observed effects – it is by no means unusual to come up with several which are not mentioned by the authors. Although not quite the level of scientific fraud described in Broad and Wade's classic book *The Betrayers of the Truth*, some papers come pretty close.

The pressures facing environmental scientists over the issue of experimental uncertainty and incompleteness, especially when attempting to carry out scientific investigations which resolve real-life dilemmas, can be judged from some of the recent and occasionally quite heated discussions which have surrounded the concept of the 'precautionary principle'.

The precautionary principle

The unpredictability of many natural phenomena, and the undoubtedly limited level of understanding which ecological science is able to apply to most natural systems, has increasingly obliged politicians and administrators to recognise that even the very best advice provided by the scientific community must be leavened with a significant degree of caution. Such acceptance of uncertainty has increasingly become formalised through intergovernmental action. In particular, according to the UK Biodiversity Action Plan, the principle of 'precaution' will now form one of the primary mechanisms by which the UK Government intends to meet its obligations under the United Nations Conference on Environment and Development Convention on Biological Diversity, signed in June 1992.

Such international commitments explicitly recognise the need to apply precaution when coming to decisions which may affect the natural biodiversity of a region or influence the natural sustainability of an area. The precautionary principle is described by the government thus: 'interactions [of natural systems] are complex [and] where there is a significant chance of damage to our diversity occurring, conservation measures are appropriate even in the absence of <u>conclusive</u> [my emphasis] scientific evidence that the damage will occur' (HMSO 1994).

This concept has attracted a lively response from certain scientific quarters. A particularly illuminating set of papers published between 1990 and 1992 in the *Marine Pollution Bulletin* explores the various implications of such an approach, while Alex Milne has launched a vigorous attack on the 'precautionary principle' in *New Scientist* (June 1993), in response to previous articles from Mary Midgely (August 1992) and Bryan Wynne and Sue Mayer (June 1993).

Milne argues that the burden of proof which the precautionary principle places on industry is not merely an excessively onerous obligation, but one with which it is impossible to comply. He argues that the precautionary principle is unsound and unscientific because:

1. the definition, derived originally from a German concept, states that action should be taken even before the definitive scientific proof of causal links between emission and effect has been obtained;

2. the principle recommends that action be taken *prior* to complete determination of 'harmful limits';

3. that the proof of 'no-harm' is impossible to demonstrate;

4. that the precautionary principle is a form of moral philosophy, rather than any rigorous scientific principle.

The precautionary principle as moral philosophy

Taking Milne's last point first, most conservationists and scientists would agree with this. The precautionary principle is indeed a philosophical approach, but this is because mankind's relationship with the outside world is generally a 'subjective', moral issue, rather than something which can be defined by science. Science, of course, has no value judgements, and therefore the concept of 'harm', and the desire to avoid such harm, can have no place in a world ordered strictly by scientific principles. In Milne's world, scientists who have devoted their entire working lives to a single species ought not to regard the extinction of their chosen species with anything but cool, scientific detachment. From an objectively scientific platform, extinction provides an opportunity to investigate the impact which the loss of this species would have on the surrounding ecosystem. It also provides an empty niche for exploitation by other species or even for the evolution of new species and thus the scientist is provided with potentially as rich a vein of research as was provided by the now-extinct species. Few scientists would, however, propose that a species be driven to the brink of extinction in order to study ecosystem interactions.

The anthropocentric view of the environmental care states that we should maintain a stable set of conditions, minimizing such extinctions and marked perturbations, to ensure that we, and succeeding generations, can continue to survive and harvest the Earth's resources sustainably. This is not justifiable through any scientific principle, but is the widespread view, even amongst scientists, because we as a species value our continued survival. The world of Lovelock's Gaia hypothesis might easily take a rather different view.

Milne suggests that the kind of moral philosophy enshrined in the precautionary principle is somehow unusual, but scientists are subject to just such moral restraint all the time. Quite simply, some things are acceptable to society and others are not. The recent past contains chilling examples of the extreme and grotesque scientific practices which can result when such moral restraint is lifted. The memory of experiments carried out in the name of science by Dr Joseph Mengele on his concentration camp victims blows like a bleak wind through the halls of 'objective science'.

The simple fact of the matter is that society puts subjective constraints on scientists and their work because society is based largely on values and value judgements. In environmental matters, society has declared that scientists should be *sure* before they do something, just as they expect engineers to be sure when they build a bridge, because a proposal to alter the environment is not the same as some controlled condition laboratory experiment. A bridge which turns into a Galloping Gertie is dangerous or inconvenient only for those wishing to cross the river, but we all have to live with the consequences when an 'objective' scientist inadvertently opens an environmental Pandora's box. Winston Churchill expressed this mistrust when he observed 'Scientists should be on tap, not on top.'

The precautionary principle and the 'proof of harm'

It is probably true that it is not possible to prove conclusively whether or not a substance or action is 'harmless'. This is because virtually everything ultimately has some impact. Obviously, chemical tests which prove the extremely insoluble nature of a substance are, nevertheless, irrelevant if such large quantities of the material are to be dumped that it will smother existing marine ecosystems. Alternatively, construction of a roadway across a hydrologically sensitive peatland site may involve the use of an inert substance. If it requires deep marginal drains to prevent it being washed away, the presence of the inert material may not be a problem *per se* (other than for the immediate area of habitat buried beneath the material); the necessary drainage infrastructure to keep it in place certainly would be.

The real-world context in which a substance or action may ultimately feature is thus a vital part of judging potential harm, but anticipating all possible contexts and evaluating their effects will often prove to be beyond even the most sophisticated environmental model. In his book *The Dammed*, writer Fred Pearce provides a catalogue of consequences which have followed from the construction of the Aswan Dam in Egypt. These relate to such diverse aspects as loss of the Mediterranean sardine which once bred in the Nile delta, the spread of bilharzia and schistosomiasis, loss of annual silt deposits on farmland in the Nile valley, the lack of fresh clay for the brick-making industry, widespread erosion of the Nile delta and its farmlands, the overwhelming of coastal defences and the accumulation of salt in agricultural land, this last problem to be tackled by a scheme which will cost more than the original construction of the dam. The complications imposed here by the real-life context are clearly quite overwhelming in their complexity.

What is 'harm'?

Milne states that the concept of 'harm' is scientifically worthless because it cannot be defined by any rigorous scientific method. Despite this, the brick-makers of the Nile valley, or the inhabitants of Borg-el-Borellos, now submerged two kilometres out to sea because coastal defences on the Nile delta have collapsed, would doubtless have a very clear opinion of what constitutes 'harm'. Their views may, nevertheless, be disputed by fishermen harvesting fish in these new shallow waters, because 'harm' is indeed a subjective, rather than objective, concept. The degree of change regarded as a threshold, beyond which something is regarded as 'harmful' must ultimately be a subjective decision because science puts no values on one sequence of events compared to another.

Just because something is scientifically 'worthless' does not mean it is worthless in the real world, however. If society defines its values, as it has done so in, for example, the UK Biodiversity Action Plan, it is then possible to use the best science available to provide data which can be set against these values, and balancing judgements made accordingly. The precautionary principle may be based on the value judgements of society, and not, as Milne would like, on 'objective' scientific proof, but judgements about 'harm' can be informed, rather than defined, by rigorously objective, scientific evidence. The principles of biodiversity and sustainable use, as outlined in the UK Biodiversity Action Plan, make it possible to begin the development of rational, pragmatic guidelines for the definition of 'harm'.

To act before 'harmful' limits have been identified is not scientific

It may be true that, in the strict sense of the terms, such actions are not 'scientific' according to Milne, but to what 'action' does he refer? It is actually a decision *preventing* change from existing conditions. More accurately, it represents the imposition of *no* action until such time as a new proposal has proved itself free of 'harm' within the pragmatic guidelines provided by society.

Milne's approach virtually endorses the instigation and continuation of an activity until it has been proven, beyond any doubt, to be 'harmful'. It is difficult to believe that Milne seriously believes this to apply to all other scientific disciplines. The pharmaceutical world is prevented by society from releasing new drugs until such time as they have been exhaustively and independently tested for evidence of 'harm'. The 'action' of preventing release of a drug even before the testing for 'harm' begins is standard practice in pharmacology, but is this unscientific? Logically, one approaches with more caution a problem for which one has little data than one does another problem for which the data are largely complete.

Without imperfect knowledge there would be no science

Those, like Milne, opposed to the precautionary principle say that uncertainty is unacceptable and unscientific. Yet if everything in the universe were revealed to us there would be little need for science, because all research feeds only on the unknown. Today, the practice and application of science appears to be so anxious to deny this intimate link, this dependence on the unknown, that a popular perception has developed of the scientist as someone concerned only with concrete facts and absolute certainties. The philosophy of scientific certainty and the fear of ignorance have brought about a general belief, not least in the minds of developers and politicians, that the scientist merely has to wave a magic wand over the problem for all the answers to pop out with the absolute precision of Ammann's Brooklyn Bridge project. Of course, this is rarely the case with the environment. What society gets, all too often, is a Galloping Gertie.

Nevertheless, people continue to believe in the science of certainty and the certainty of science. Consequently a developer, having paid a seemingly considerable sum for an EIA, does not want a consultant's report which lists page upon page of topics not investigated, followed by further pages of cautionary caveats in the impact assessment. Nor does a politician or planning officer, faced with a difficult and potentially costly decision, welcome such uncertainty and lack of definitive guidance.

This belief in the essential certainty of science is encouraged by scientists who make confident statements to the effect that no evidence of harm has been detected using the available methods and therefore it is inconceivable that any harm could occur. This represents a fine example of scientific *hubris*, especially if such comments are based on limited survey and are thus a clear case of 'being economical with the truth'.

More worrying still, perhaps, are confident predictions that Nature is robust enough to bounce back unchanged, or better than ever, after such events. In the Marine Pollution Bulletin debate, John Gray, for example, confidently predicts that, if ilmenite dumping off the Norwegian coast is halted at some point in the future, the marine ecosystem 'will be restored within 10 years'. Restored? Ten years? Has Gray studied *all* aspects of the marine ecosystem in the area currently affected by such dumping, or is he talking only of the more evident components of the ecosystem? Such confident statements about recovery and restoration (and such statements are much less rare in EIAs than are frank and detailed admissions of information gaps) should always be very carefully and critically examined.

What is required is a subtle shift in the present scientific ethos, one which welcomes open acknowledgement of ignorance and encourages the limitations of any particular study to be fully explored. If scientists were more up-front, enthusiastic, even, about the true limits to our understanding on an issue, this could, in time, engender a recognition within society as a whole just how enormous are the environmental uncertainties which currently face us. It would also make EIAs more honest in terms

Building bridges between developers and the environment

The current state of affairs could at last be improved if those involved with EIAs, and perhaps the community of ecological scientists at large, were to adopt the philosophy of the bridge-designer. The key factor in a bridge-designer's mind during the whole process is a constant searching for the unexpected, the unplanned-for discovery, which can then receive immediate attention before disaster sets in. The philosophy, in other words, positively encourages the identification of knowledge gaps because the alternative of hiding such gaps under the carpet, pretending they are not relevant, could lead to collapse, catastrophe and perhaps appalling loss of life. Other chapters in this book suggest that this may already have happened.

The sequence of events involved in building a bridge is not so very different from carrying out an EIA, at least during the early stages, although, the bridge designer is committed to a much longer relationship with the final outcome than is the EIA scientist. Before exploring this comparison further, however, it is worth turning aside briefly to consider four difficulties which are a chronic problem for EIA studies, and which add significant complexity to any such programme of work.

The small extent of existing knowledge

Unlike many of the traditional sciences, which have a formal pedigree going back many centuries, ecology is a very young science. Indeed, the term itself was not coined until 1869 by Ernst Haeckel, and university courses have only been turning out graduates formally trained in the subject since the 1960s. A discipline which has only received significant attention for the last 40-50 years, when some of the natural cycles already identified span 20-30 years, is obviously only going to be able to offer a limited base line of existing data.

Funding bodies prefer 'pure' and 'big' science to practical dilemmas

Unfortunately, the difficulties of being a recent arrival on the scientific scene has been compounded by the problem that research into the everyday, practical problems of environmental impact has been starved of research funding for decades. Even today, with the upsurge of green awareness and intergovernmental concern about environmental issues, the main projects funded by the Natural Environment Research Council (NERC) are the global issues such as greenhouse gas emissions, or oceanic current movements, or broad pollution mapping of the North Sea. Ask any professional nature conservationist or planning authority what environmental issues require urgent research for the day to day operation of environmental legislation and the topics will rarely feature prominently in the NERC research budget. The only other significant source of environmental research funding comes through the statutory conservation agencies, but these research budgets have been steadily reduced in recent years to such an extent that only fraction fraction of the priority projects can now be funded.

The short time-scales available

The lack of existing data will often pose significant problems for the EIA team, but such difficulties will generally be seriously compounded by the short time-scales generally provided for such a study. The team is rarely given the time required to gather sufficiently meaningful sets of measurements. A five-year lead-time for the construction of a factory, from initial concept to final completion, is not unreasonable in construction terms. For many environmental problems five years is too short a time in which to measure, understand and model the existing ecosystem, never mind to judge the impact of disrupting this system. A hydrological budget alone requires the accumulation of data over several years in order to balance out the variability introduced by wet or dry years, and such a budget represents only a small part of what generally requires investigation. Such environmental measures must normally, however, be carried out well within the period of preparation and construction for the proposal. It is not unusual to have an EIA based on only one season's data, and in some cases on only one or two site visits, and even the normally-excellent *Environmental Assessment Handbook* suggests that sampling might need to be carried out for as much as a whole year.

The balance of time-constraints, cost to the developer and the need to obtain meaningful data is one of the central dilemmas of EIA work. Millions of pounds have been spent on the underground deep storage caverns at Sellafield nuclear reprocessing plant in Cumbria, UK, yet only now has it been established that the water-table of the underground caverns is connected to the regional groundwater system. Such aquifer links are a common and expensive feature of regional groundwater regimes, yet how often will a developer be prepared to carry out extensive borehole tests to develop a working model of the relevant area of the groundwater and its behaviour? Experience suggests that few can even afford to do so. And groundwater behaviour is just one aspect. Unravelling the causes behind the steady loss of Norfolk's spring-fen sites poses a multifactorial problem of labyrinthine proportions. Any development proposed for the environs of these fens has a stark choice - become embroiled in an in-depth multi-disciplinary study, or ride roughshod over fears of further environmental impact.

Natural systems are unpredictable or 'chaotic'

Scientific investigation and modelling has been based for centuries on the assumption that all things are deterministic Newtonian systems, on the assumption that, provided everything is measured carefully enough, it is possible to predict precisely and consistently the behaviour of any system under any conditions. Increasingly, this fundamental assumption is being revealed as incorrect, particularly for natural processes. The general rule seems to be that non-linear processes – 'chaotic systems' – predominate in Nature.

Weather patterns are coming to be recognised as unpredictably chaotic even by the general public, who accept that it is impossible to predict that a specific shower will fall on a specific place at a particular time, but the weather is just one example of many. This non-linear behaviour has profound implications for EIA studies. Measurements taken from such systems provide only one possible set of initial states. In a chaotic system even a fractionally different set of initial conditions will cause a model to spiral away from reality within an alarmingly short space of time. This is why reasonably accurate weather forecasts are only possible for a few days hence, although even then the detail remains unpredictable.

The worlds of the bridge-building engineer and the EIA ecologist should thus be very different in outlook. Bridge building is largely a deterministic exercise in the Newtonian sense, with only a few chaotic elements to cause the odd surprise; ecosystem analysis is quite the reverse, involving a turbulent sea of chaotic systems which swirl around a few isolated rocks of Newtonian certainty. Oddly enough, welly-booted ecologists stomping round in sometimes uncomfortably close proximity to Nature appear to have been slow to recognize the significance of this new understanding of natural systems; it is perhaps ironic that such insight has come from mathematicians and physicists sitting in darkened rooms, bathed only by the glow of computer screens. Meanwhile, the ecologist still struggles to fit observed behaviour into appropriate deterministic patterns. The result is a dangerously simplistic set of EIA predictions.

The burden of proof

Although many failures of EIA analysis are not the fault of the developer, but rather the desire of the scientific community to paper over gaps in the scientific basis of predictions, the developer is not entirely innocent, however. Until the introduction of the precautionary principle as part of government policy, the burden of proof on environmental issues had been laid firmly on the shoulders of the environmental scientist and the conservationist. Indeed, the desire to prove, scientifically, that damage occurs before taking preventative action is very attractive to certain types of scientific philosophy. In the *Marine Pollution Bulletin* debate, John Gray argues forcefully for this approach in order to avoid accusations of 'crying wolf' about environmental issues. The counter argument, presented by Robert Earll, points out that the 'dump-monitor-act' approach is all very well but assumes that monitoring will tell us when to act and that we will know how to cure the problem once we have let it develop to the point of being recognisable. In support of this, Alf Josefson argues that it is not wrong to provide cautionary warnings, even if the scientific evidence is not statistically significant, provided that a clear statement accompanies the warning to the effect that the issue is not yet proven.

As regards industry's demand that the environmental scientist, rather than industry, should bear the burden of proof, Alex Milne makes the following revealing observation:

One of the practical costs of these unscientific and impossible requirements is time and effort wasted by scientists in industry and government. I was not reading the House of Lords Report HI 23 or the European Commission's Marketing and Use Directive for fun, but because they were trying to put manufacturing industry out of business.

Here we have the rub. Neither of the two reports he was reading, nor the sentiments behind them, sought to 'put [Milne's anti-fouling paint] manufacturing industry out of business', any more than the Montreal CFC Protocol sought to send the refrigeration industry to the wall. They are instead examples of national or international efforts designed to encourage industry to adopt new methods which, for whatever reasons, are considered to be less harmful.

Milne's attitude reflects the impatience of industry reluctantly facing the burden of considering its impact on the 'downstream' environment. Such a sentiment is in stark contrast with the demands of industry for the 'upstream' part of the manufacturing process. Materials coming into most industries must be of a consistently high and predictable quality. Thus particular water supplies, or rock types, or gravel deposits, or peat types are described as 'essential' for the continued survival of the industry. Industry demands predictability in its raw materials and is quite happy to apply the precautionary principle with some vigour at the supply end, therefore, but is often keen to avoid being bound by this same principle in its outputs – or at least in certain outputs.

A manufacturing or production industry – take the anti-fouling paint industry as an example – will already have carried out exhaustive tests on the paint product for the effect on a boat's underside (no point in producing a paint which dissolves the gel of a glass-reinforced plastic hull), as well as tests to ensure that the paint will not eat its way through the storage tins while on the retailers' shelves and presumably also exhaustively tested the paint's toxicity to humans. A manufacturer's nightmare is the release of a product which must subsequently be withdrawn because of a defect in these, or any number of other aspects. We can therefore be reasonably confident that testing for such features is rigorous, thorough and reliable; the same approach, indeed, as a bridge designer would employ when preparing a new design. The difference with these outputs is that the customer can sue. The environment cannot sue anyone, it can only suffer in silence.

Even this high level of caution is, as we have already seen, still not sufficient to ensure that even the basic functions of the finished product can be guaranteed. Galloping Gerties will always be with us, popping up unpredictably in even the most rigorously-controlled environments. Yet compared to the care lavished by industry on the relatively straightforward processes of raw materials acquisition and subsequent product manufacturing, the level of attention usually devoted to the environmental 'downstream' aspects of production is extraordinarily meagre. Milne seems to express a lofty sense of world-weary contempt for the idea that the manufacturer should devote any significant effort to evaluating such 'downstream' aspects of production, even if it is only to identify those areas where uncertainty exists - this is time and effort wasted by scientists in industry and government.

Improving Galloping Gertie's current form

There are a number of ways in which the present unsatisfactory and haphazard situation could be improved. Achieving these improvements would require a willingness on the part of industry and developers generally, and perhaps most importantly, environmental consultants, to embrace the philosophy of long-tern 'downstream' monitoring, together with all its implications and responsibilities. Like a puppy, an EIA is not just for Christmas, it is for life. Both developer and consultant must be prepared to adopt the sense of responsibility and long-term commitment to their EIA that has been felt by designers of bridges probably since the first tree-trunk was felled across a river.

Establish a single authority responsible for overseeing EIAs

An improvement on the current EIA free-for-all would be to have a recognised authority to whom both developer and planning authority can turn when an EIA is required. The EIA Authority would then be able to define for the developer those environmental parameters which should be investigated, and in what way, in order to provide the best evaluation possible. If the developer's resources are limited, this authority would be capable of guiding the developer to the most critical issues. It can then assist the developer in drawing on the accumulated archive of similar studies and scientific publications to assist the ES make best-possible predictions. It can subsequently guide the planning authority in assessing the degree of certainty which can be placed on what has been achieved within the developer's available resources.

One of the advantages of such an authority, for scientists involved in EIA work is that the authority can take responsibility for identifying those aspects which have not been or cannot be investigated. It can endorse the practical decisions not to investigate certain aspects, while at the same time explaining to the developer and politician the implications of this for the certainty or otherwise of any final predictions. The EIA scientist is thus not left in what is often perceived as the exposed position of having to choose which aspects not to investigate. Such choices can be shown instead to have been made in consultation with the EIA Authority.

The Authority would also be responsible for accumulating the data archive from EIA studies, as well as pulling together information from published scientific data. In this way it can establish best-practice methods for investigating particular aspects. It can also enable EIA studies to maximise the use of resources available by avoiding work which merely repeats what is already know and instead identify the key areas which cannot yet be resolved using existing knowledge.

Finally, the Authority would provide the long-term link required to ensure that, even if individuals in the company or consultancy change, *post-hoc* monitoring is carried out in a manner which is consistent with the requirements for monitoring change, that the results are collated and reviewed on a regular basis, and that this information is conveyed to the planning authority.

The EA legislation needs to be changed to incorporate after-the-event (post-hoc) monitoring.
It would seem sensible to incorporate such a system into the existing planning legislation, with perhaps a three- or five-year review period for any scheme. On the review date, the need for additional safeguards in the light of either evidence of change, or of improved scientific understanding, can then be assessed. The EIA Authority would be responsible for co-ordinating the efforts of developer and EIA team to ensure that the best possible advice is provided for this review period, but the decision would remain with the planning authority.

Ecological science needs to become more chaotic
Despite the increasing enthusiasm of the mathematical world for the non-linear behaviour of Nature, the number of ecological and environmental scientists making the effort to link up with non-linear mathematicians is very small. There is undoubtedly a rich seam of understanding to be gained from such a cross-disciplinary collaboration, but it so far lies almost completely untapped.

Edward Lorenz's paper about chaotic weather patterns lay un-noticed in an obscure meteorological journal for almost 15 years before it was finally re-discovered, to much amazement and excitement, by mathematicians and physicists. The same is now in danger of happening in reverse; ideas from this new mathematics are filling entire mathematical journals, but few of these ideas are filtering through into ecological and environmental journals.

Funding is needed for research to protect the environment
There is no question that funding is available for environmental research, but only a tiny fraction of this is used to address practical environmental questions. The major part of environmental funding goes towards big fashionable topics which have little practical relevance to everyday conservation. The global picture is thus being studied intensively, but meanwhile things in our own back yard are quietly going to blazes.

The UK's Natural Environment Research Council, ironically, regards the giving of research grants to study practical conservation issues as too applied, by which it presumably means 'too close to the environment'? It is a revealing reflection on the scientific community's view of the environment that funding for large particle accelerators runs into the multimillions of pounds, but most practical conservation research survives on sums of a few thousand, if indeed *any* funding is available. This is despite the fact that, in many ways, resolving some of the practical environmental problems in our own back yard can involve working on a system as complex as, and probably more unpredictable than, some of the largest atomic physics programmes. Funding for environmental protection science is still stuck in the days of 'bucket ecology', although today some of the answers being demanded are beyond the long-term predictive ability of even the most powerful computers.

Scientists need to be more explicit about information-gaps and uncertainties
Precisely because much of our understanding of, and modelling capability about, the natural world is currently so limited, it is important that those reading assessments and predictions fully understand the degree of uncertainty which exists.

Greater willingness to identify such limitations can produce two benefits. Firstly, decisions can be made about development proposals on the basis of a full understanding of the risks and benefits. Secondly, regular and consistent identification of gaps will generate a rich seam of potential research projects, as well as revealing and emphasising those areas of environmental research which urgently need to be addressed by environmental scientists.

Closing thoughts

At one time, the engineer faced a world which was filled with mystery and uncertainty. History is rich in examples of edifices such as the Tower of Babylon, many early cathedrals, the first aeroplanes and 'unsinkable' ocean liners, which produced rude and bewildering shocks because they all ended in catastrophe. After 7,000 - 8,000 years of experimentation and trial and error, however, the engineer can now expect at least the majority of projects to behave as planned. As Furnas & McCarthy observe in Time/Life's celebration of *The Engineer*:

> *...the engineer has changed over the years from an ingenious improviser who worked by trial and error, to a skilled and systematic specialist who brings a wealth of scientific knowledge to bear on the increasingly complex problems of today and tomorrow.*

Scientists, wrestling with environmental problems, are still at the trial and error stage.

In adopting the strategies outlined above, we have the potential to bring the work of EIA analysis from its current state of error and anarchy closer to the structured approach of the engineer. We should aim to be able to say, if decline or disaster should occur –'Well, yes, we knew that was a gap in our knowledge-base. Let's learn from this to fill the gap and add to the knowledge-pool.' At the moment the system doesn't even help us learn from our mistakes.

Volcanologists can sympathise with the present plight of the environmental scientist and the EIA consultant. A sobering observation made after the catastrophic eruption of Mount St. Helens in Washington State, USA, (thereby ending in the state where we began), and which could apply to all who struggle to understand the natural world, expresses it thus[2]:

> *...Perhaps the most important lesson of the past 30 years has been the virtue of scientific humility: Who knows which ideas that now appear untenable will turn out to be right, and which of those ideas that we currently accept on the basis of available evidence will seem hopelessly naive 2000 years from now?* (p.15) Decker, R. & Decker, B. 1989. *Volcanoes*. New York, W H Freeman & Co.

Galloping Gertie was born because Leon Moitseief, in attempting to bridge the Tacoma Narrows, accidentally strayed from the relatively calm and safe waters of established bridge engineering back into the uncharted reefs of untested predictions. Those reefs cost Moitseief his reputation and Tubby Coatsworth his life.

There are currently no clear open seas for the environmental consultant embarking on an EIA; the study begins amidst reefs, beyond which are only bigger reefs. The prudent professional acknowledges every reef and makes allowance accordingly. When things become difficult, the little statue of Tubby should act as a reminder that insufficient attention to detail can destroy part of the natural world as surely as the final design of Galloping Gertie sealed the fate of the Coatsworth's dog. A little caution and a ready, unabashed acknowledgement of scientific limitations would, under such circumstances, seem very little to ask.

Notes

1. Quotes from the 1993 Channel 4 TV programme *Bridging the Future*, written and produced by Chris Hawes.

2. Quote from *Volcanoes*, R. Decker and B. Decker, New York : W. Freeman and Co., 1989.

Annotated List of Further Reading

Betrayers of the Truth, William Broad and Nicholas Wade, London : Century, 1983.

Sociologist Deena Weinstein argued in a classic study in the 1970s that there is no reason to expect any less fraud in science than in any other area of society. Broad and Wade graphically describe some of the most famous cases, but most scientists would find the fuzzy distinction between truth and invention all too familiar.

Environmental Assessment Handbook, London : Environmental Resources Ltd., 1992 (versions for England, Scotland and Wales).

The bible of the official GB conservation agencies when guiding and evaluating Environmental Assessments. A lucid handbook which gives much helpful advice, though rather glossing over some of the more fundamental problems of EIA work as it is necessarily practiced.

Biodiversity : The UK Action Plan. Cm2428, London : HMSO, 1994.

The official Government response to the Biodiversity Convention signed in Rio. There is much that is very useful in this document for all aspects of nature conservation and environmental protection. It contains some very clear statements indicating the need to recognise uncertainty and act with caution when carrying out activities which may affect the environment.

Can science save its soul?, Mary Midgely, *New Scientist*, 1 August, 1992. *How Science Fails the Environment*, Brian Wynne and Sue Mayer, *New Scientist*, 5 June, 1993. *The Perils of Green Pessimism*, Alex Milne, *New Scientist,* 12 June, 1993.

An entertaining and sometimes illuminating set of articles about environmental precaution and responsibility. Midgely is a philosopher, Wynne a sociologist, Mayer an environmentalist and Milne an industrial scientist.

Marine Pollution Bulletin, 1990 to 1992.

Contains a series of articles and letters about the precautionary principle, sparked off by Prof. John Gray's short paper in the 1990 volume, *Statistics and the Precautionary Principle*. Various means of approaching the precautionary principle are discussed, and although the issue is not resolved, a number of useful points are brought out.