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CERTAINTY, COMPLEXITY AND CURIOSITY

A Note on Studying the Hydrological Effects of Land Management Changes

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ABSTRACT

An examination is made of the reasons for carrying out research on the hydrological effects of land management changes; the reasons are grouped under the headings of 'certainty', 'complexity' and 'curiosity'. The research techniques used for this type of research are listed and an indication given as to which techniques could be used for given problems. Some ideas are presented as to future research on experimental basins.

INTRODUCTION

Research activities involving determination of the hydrological effects of land management changes have been considerable in many parts of the world. Many projects have begun with the laudable aims of keeping the research simple, or of completing it in a relatively short time. However, the history of this type of research seems littered with incomplete and abandoned projects, and with experiments which appear to increase in scope continuously and never seem to end. In addition, some of the research results appear conflicting or indeterminate because insufficient facts have been given in the reporting of these results, and the land management expert is consequently not always sure how to use them.

What should we do about this — should we give up this type of research, or should we take stock of ourselves and examine whether we are approaching the research in the right way? There is widespread agreement (Hewlett *et al*., 1969; Ward, 1971) that this type of research on a catchment basis has been most beneficial, at least for the greater understanding of hydrological processes.

Nevertheless, it is certainly worthwhile to examine the research periodically, and much argument has been evident over the last ten years or so (Ackerman, 1966; Boughton, 1968; Toebes and Ouryvaev, 1970). These arguments, however, centre more on methods of

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research, such as experimental design techniques, and an examination of what the results are to be used for may well be much more profitable than a critical examination of techniques.

Luna B. Leopold (in press), has said: "The successful research man is the one who asks himself the right question". We must all agree with the truth of this statement. However, should we not go further, and simply say: "The successful man . . . . . . ?" It applies, for instance, to all who are dealing with land management problems, since the research we are talking about involves not only the scientist but also the land management expert. Let us, therefore, have a good look at what questions we should ask ourselves, to assist us in achieving the best possible research programme.

The subject topic can be stated simply — we are interested in knowing what are the hydrological (and perhaps other) effects of a change in land management. However, stating the topic is not posing the question. The question is really associated with the reason for seeking this knowledge, and if we can answer this, we can then examine methods of approach and fit them to the question asked.

In examining the reasons, we have to generalize in order to encompass the entire field — although this generalization should not provide a licence for a general, imperfect statement for any particular problem.

The object here is to bring some clarity to the reasons for this research which have been so scantily treated in the literature, and it can be approached in three ways, which could be called certainty, complexity and curiosity.

CERTAINTY

Here we have a simple reason; a simple answer to our question. It is really a matter of wanting to convince people: our customers, ourselves, and those in the administrative and political spheres.

For instance, an expert soil conservator may know how to cure a particular erosion problem, but he has to convince the farmers and he has to demonstrate that he knows what he is doing. Or this same soil conservator, although he may know a given answer because of his ability and his experience, does not dare to promote his techniques without some scientific proof. Again, he may have to convince the powers that be to approve and subsidize a particular erosion control scheme and the system considers it needs some facts before it can approve the proposal.

The problem with the wish to have certainty is frequently that the distinction between demonstration and research becomes blurred. It is hard to say how often elaborate experiments have been established on areas, or on plots, or on entire catchments for the above reasons, but it is probably quite large, and certainly in New Zealand the pressure for this is continuous.

For these reasons, the wish to have certainty, to prove what we know already, to prove that a technique is sound when the land management expert knows by experience and intuition that it is sound, makes this question the most exasperating and difficult to deal with. We know the reason — we want to convince somebody — but it will be much more difficult to judge whether a particular research effort, or any at all, is warranted in any case under this heading. The research effort, if any at all, should be minimal and if possible other avenues should be used to obtain the certainty required.

COMPLEXITY

In this case we do not want to convince ourselves or others; in this case we genuinely want to carry out research because the problem is too complex to handle without some scientific facts. We may have no previous experience, or no idea on how to handle a particular problem, or we could have a number of alternatives — the best one we cannot judge because too many factors are involved. Such cases arise in particular at present when we are thinking about integrated catchment control schemes. We might have to deal with relatively large areas, which makes it more difficult because downstream effects may be quite different from on-site effects and because the need for optimization of the solution arises.

Then again, it might be necessary at the same time to consider water resources planning, i.e. the location and allocation of water in the same catchment in which we are to carry out our catchment control scheme. As is well known, it is not possible to have both optimum erosion control and an optimum water yield, and it may therefore be necessary to carry out difficult research which includes many considerations. Or, going even further, we might wish to consider complete land use planning which involves research into the choice of the conflicting objectives of optimum production, optimum water yield, minimal erosion and minimal (non-industrial) pollution.

However, the difficulty of complexity is that it is frequently hard to state the question in detail, because we must carefully consider whether or not the question we pose is trivial. This means that before we declare a problem complex and demand or carry out research, we should critically examine it — and this examination should include a consideration of how the results will be used.

For this reason it is most important that land management expert and scientist collectively state the problem when it appears a complex one.

CURIOSITY

This is the question most often criticized by the administrator and the practical man, because here we are concerned with *apparent* scientific curiosity.

A land management expert may be genuinely curious about what is happening in nature and may stimulate research, but more often than not it is the scientist who promotes this aspect. And more often than not he appears more concerned with the elucidation of a given process than with the immediate practical effect of a cultural change on this process. Of course, the scientist is really only asking for freedom to go his own way, and he knows that in this way general solutions will be found rather than specific ones, saving the taxpayer, ultimately, a considerable amount of money.

Bright Wilson (1952) has said that a certain proportion of scientific staff should be given freedom to do fundamental research, and he states "the purpose of this freedom would not be philanthropy but a hardheaded realization that any basic knowledge pertaining to [the given subject] would almost surely be used later to solve practical and urgent problems in a much more rapid and satisfactory way than the usual empirical cut-and-try procedures which must be employed when understanding is lacking".

We should realize that most of the so-called fundamental research in our field of interest has a very practical objective, and for this reason such research should be stimulated.

TECHNIQUES

Having posed a question, the researcher should subsequently match the question with the most suitable technique. Because we have mixed the question (and the technical concepts) in the past too much amongst aspects of certainty, complexity and curiosity, there have never been clear statements as to which is the most suitable technique for a given problem. Perhaps it is impossible to define this completely, although at least a listing of various techniques with some indication of their applicability could improve the situation.

The known techniques are:

Experience

This is considered experience on the part of the land management expert. It is listed for completeness only, since it does not require research.

Literature Review

This is a logical corollary to experience. By reading the scientific literature which deals with the subject, the land management expert will increase his experience.

Scientific Review

This is the next step. In this case the land management expert calls in a hydrological scientist for his opinion. This is valuable because much of the research experience of any scientist is never published and the land management expert might find it difficult to keep up with the literature. It also brings the land management expert and the scientist together. These are obvious reasons for strongly promoting this technique.

Before-and-After Experiment

In this case observations of some hydrological aspects are made before and after the change, on the actual area or catchment where a land management change is carried out.

This has been a common practice in many countries and has generally been a failure since the research has often been casual, the observations have frequently been started when the change is already in progress, and the experimental area often includes areas outside the control of the investigator — giving rise to interference with the experimental results.

These reasons have made analyses of the observed data very difficult if not impossible in most cases, and when results have been published an examination of the errors involved could well lead to a modification of the conclusions.

*Local Experimental Basin**

To overcome the difficulties associated with the before-and-after experiment, an experimental basin is sometimes established within or near the problem area. Such a basin allows for control of the land and gives scope for a better experimental design.

This approach, from a scientific point of view, is superior to the before-and-after experiment. The drawback, however, is that the problem is frequently *now*, while research results are usually too late to affect the solution. Also, there are obviously too many problems to allow such an experiment to be carried out everywhere.

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* Included in the term experimental basin are runoff plots, experimental areas, reserves or whatever they may be called. The difference between these is purely one of experimental design.

Representative Experimentation

This method attempts to overcome the disadvantages of the 'local experimental basin' by sampling the problems and by establishing some sort of network of experiments, or at least by carrying out an experiment on a piece of land which is representative of a large area. This approach is more conducive to providing general solutions rather than specific ones, provided of course that the question has been properly stated.

A typical example is the network of IHD experimental basins in New Zealand, which attempts to sample the typical soil/vegetation complexes of the country together with typical land management changes (Toebes, 1965). This approach, because of its concentration of scientific effort, is bound to be superior to other methods, and should provide some answers to any question raised under the headings of certainty, complexity and curiosity.

Mathematical Models

Modelling, whether in a physical or abstract form, has always been associated with experimentation. In recent years, because of the development of electronic computers, abstract mathematical modelling of hydrological processes has proceeded very rapidly. Simultaneous development of systems analysis has opened the way for the modelling of hydrological systems, and mathematical simulation of the behaviour of entire river basins is now a practical reality.

Representative experimentation is now rarely done without associated mathematical modelling, in fact a mathematical model is designed at the same time experimentation is begun so that both the experiment and the model can be modified, as time progresses, to give optimum results.

A model is being developed in New Zealand which uses data from representative and experimental basins, and which will be capable of forecasting the effects of land management changes on water yield (Ibbitt, 1971). Models such as this are capable of giving general solutions. Methods are now also available to develop mathematical models for specific problems, such as catchment control schemes, and allowance can be made in these for the inclusion of economic and social decision variables in addition to the hydrological parameters.

New Techniques

A technique that is not necessarily new, but certainly not sufficiently explored, is the scientific observation of existing conditions rather than carrying out the usual before-and-after experiment. As an example, consider the planting of poplars and willows to stabilize hillslopes; before elaborate experimentation is resorted to, much useful knowledge could be obtained by observing stable and unstable hill slopes in relation to selected geomorphological and hydrological characteristics such as rainfall, aspect, slope and soil moisture. Similarly, observations could be made of, for instance, the effect of poplar planting on stability characteristics, considering in addition factors such as species, growth rates, water usage, rooting characteristics and planting patterns.

Another new technique involves a move away from the traditional 'calibrate, cut and publish' approach on experimental basins, since, as explained previously, the before-and-after experimentation may in some cases prove what is already known, especially if the question asked comes under the heading of certainty.

Problems are really deeper, and considering the conflicting aims in land management of a high production, a high water yield, minimal erosion (and flooding) and minimal (non-industrial) pollution, research on some experimental basins should be geared to what could be called the 'synenergetic balance'. This approach combines the radiation balance, water balance and nutrient balance (including erosion and sediment transport) not only in terms of total mass and energy transfer, but also in terms of the most efficient use of energy.

It may be argued that economics is important and should be taken into account, and this can be done, but the optimum way of utilizing the given available energy may in the long run be also the most economic. Considering economics alone, production (of food and water) is always the overriding factor, because the damage done to catchments in terms of erosion, pollution, biological degradation and aesthetic depletion cannot be adequately accounted for.

Such research is very difficult; for instance, the percentage of incident solar radiation used in photosynthesis is extremely small (Slayter, 1967), causing problems in defining its value accurately. It is especially here that the question of a more efficient use of energy can be considered by selecting the right land management technique to reduce the limiting factors such as water or availability of nutrients, or by fitting the right pattern of land use to the given condition.

CONCLUSIONS

Anyone who has a problem in the field of land management should consider very carefully whether the problem is one that really requires research. It may be trivial, or it may be a very localized problem, or the question stated may fall under the heading of 'certainty'. In such a case a thorough literature and/or scientific review is a much more realistic approach than carrying out research. It must be realized that certainty in science is impossible to achieve anyway; Bertrand Russell (1950) has stated that "the demand for certainty in our lives is natural to man, but is nevertheless an intellectual vice".

If it is decided that the problem is real and that it can be placed under the heading of 'complexity', careful consideration should be given to whether a solution could be found by using existing experimental areas, or by using techniques which have been developed for general application, or whether the scientific observation of existing conditions could provide the answer. Only if none of these alternatives is possible should specific experimentation be resorted to.

The problems which may lead to questions under 'curiosity' should be considered in great depth. Basic research is of long duration and can be expensive, and we are getting past the stage where we can aim at researching simple effects of land management practices. It requires careful thought on the type of experimentation for experimental basins, and this should move in the direction of specific research such as the development of the synenergetic balance. This does not free the scientist from the responsibility of considering how the results are going to be applied; to obtain this requires continuous close co-operation between the land management expert and the scientist.

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