Is Our Approach to Forestry Research Adequate?[1]

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Summary

The original motivating forces in European and American research are outlined. The concepts of basic and applied research are defined and discussed. The methods of handling research and research problems are discussed with particular reference to points of difference between the Irish and American approaches and the significance of the fundamental approach. Aspects of communication are discussed, with pointers on standardization of technique in scientific writing, and the need for better communication with the public at large.

INTRODUCTION

As has been the case with most fields of scientific endeavour, forestry research evolved from the practical need to increase forest productivity to supply a growing market. A resumé of development in the field of sustained forestry yield shows how management in European forests progressed through the centuries. Evolution of thought and development of the scientific approach are illustrated in the following stages of advance in management techniques.

As early as the ninth century impositions were placed locally on land clearance. In 1305 Petrus de Crescentiis made the first suggestions regarding artificial regeneration, but it was not until 1368 that, at Nurnberg, the first forest seeding took place. This trend of human interference was slow to develop in the beginning as the resources were large and the need to conserve and replace was not apparent. An early example of a projected view of forestry can be seen in some of our oak woods established and fostered to provide timber for the English navy.

Yield control as a legal measure preceded silvicultural techniques, but with the development of silvicultural systems the foundations for scientific investigation of management problems were established.

To quote a few early examples we note that in 1669 the area division method was prescribed for all French forests, and that in 1791 Hennert produced the first yield tables. Again we can see a slow early progression due to limited need to organise and rationalise.

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1. Text of a paper read at a symposium on "Aspects of American Forestry of Interest in Ireland" at Pomeroy Forest School, Northern Ireland, April 22nd, 1965.

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From this the pertinent fact that research in European forestry developed out of traditional approaches to management becomes evident.

In the United States of America forestry, even at the turn of the century, was hardly a science, hardly an art, but mainly a matter of exploitation of natural resources. To quote Fernow (1891):

> "Of all the natural resources reserved for our use it is the most directly useful, for in the forest we find ready to hand, without further exertion than the mere harvesting, the greatest variety of material applicable to the needs of man, the means to satisfy every direct want of life".

I do not wish to quote Fernow out of context, for he continues by defining the goal of management as being the production of the largest amount of the most useful wood in the smallest area possible and with the least expenditure of energy or money. To achieve this obviously requires considerable thought and planning and cannot be conceived of as purely an exploitation approach. With reference to the previous paper, one can see that the above quote appeared six years before there was a definite government policy for timber lands. However, much of American forestry, particularly that undertaken by the timber industries, was mere exploitation, and it has only been in recent times that the realisation has come home to them that the future of the American forests may well be in jeopardy unless steps are taken to conserve them by proper management practices and to improve productivity by conducting research over a wide area of silvics and silviculture.

As has been stressed in the first paper, a growing realisation that forestry properly must be thought of in terms of multiple land use has led American workers to investigate the forest in relation to watershed management, soil stabilisation and recreation, and other facets. In this the lack of tradition has been of considerable value. The enquiring mind is not set, is not biased in favour of one scheme or another, and is without preconceived ideas on how a job should be tackled.

RESEARCH IN GENERAL

To develop a discussion on science in general is out of the question here, but certain aspects of research in its broadest meaning bear thinking about in relation to our approach to forestry research.

Research is generally classified under two main headings — basic and applied research.

Basic Research

Although Stone (1957) tends to classify basic research as that which is oriented towards determining why, others who have attempted to define it consider it more as a quest for knowledge irrespective of its applicability. Frequently this envisages the "why" of a phenomenon, but not necessarily so. By elaborating on Polanyi's definition of pure science (Polanyi 1939) we can say that:

> Basic research is essentially the study of the fundamentals of our universe and all that pertains thereto for the sake of knowledge and truth, regardless of its implications in the world of man.

Applied Research

Applied research may broadly be defined as the application of scientific knowledge and techniques to solving practical problems.

Examples of these concepts I have seen at work in the State of Tennessee where the radio-isotope Cesium^{137} was used to study the cycle of elements in a tree crop as a factor of growth and metabolism (basic research) and could also be used as a substitute for potassium to study the effects of fertilisation on the cycling of this element—a guide to fertiliser requirements (applied research).

Unfortunately, a strong degree of antagonism exists between protagonists for the two spheres of research and our enlightenment has not yet arrived at the point where the need for basic research is universally accepted and it is often considered by those involved in basic research that theirs is the only true research, the work in the applied field being hardly more than manual labour. But this contention that exists between basic and applied research is meaningless. The snobbery existing between the two fieldse as illustrated by Polanyi (1939) and Beveridge (1957) is ludicrous. I prefer the idea portrayed by Julian Huxley (1936) who said:

> "People have realised that practical problems can be solved by handing them over to the pure scientist, even if for a time his work seems to have no relevance to practice, and basic research has been interposed as a link in the chain between question and answer".

Applied to our own immediate concern with research in forestry E. L. Stone (1958) makes the plea:

> "Perhaps our test of good forest research should be not whether it is basic or non-basic, but rather is it relevant; is it well done; will it reduce the degree of empiricism in its area?"

To present the approach by which the above questions may be answered with a "yes!" is my intention in this paper.

THE PROBLEM

The basic problem in all science is the question. To quote Synge (1951):

> "Contrary to popular belief it is harder to ask than to answer".

This idea has a special bearing on forestry research because of the nature of the material we work with. With a simple subject one can formulate comprehensible questions and, more than likely, one can set about answering them without undue difficulty.

But forestry is not simple and the difficulties lie in:

(1) The length of time trees take to grow. This means there must be a considerable time-lag between the initiation of an experiment and the analysis of the results. It is one of the primary reasons why forestry research, not only in Ireland but throughout the world, is still in its infancy. Although much work has been done a great deal of it is inconclusive as yet as the experiments are not terminated — and many remarkable experiments will still be running in 20 years time!

(2) The complexity of the environment rarely allows of clear-cut answers. This means that experiments are either extremely complex or else must be repeated a number of times, or both, before we can be emphatic in our deductions.

(3) The nature of a forest is such that any field experiments are of necessity cumbersome — large tracts of land, large trees etc.

These three factors lead to a certain hesitancy to become "too involved" in research projects. But worse, they may also lead to a hit-and-miss approach to applied forestry research.

Although much of the basic research that has been done has been of the nature of following hunches, or the cut-and-dry type referred to above and as such has led to remarkable discoveries (Conant, 1961), it is not an approach that should always be recommended. This is a suitable basis for the genius, but not for the ordinary research worker working in the applied field.

Thus we are faced with the necessity of planning research. To this idea of planning there are many protagonistic and antagonistic arguments. There is validity in both views, but the main distinction is more a matter of degree. Public resistance to discovery is a useful buffer against a too hasty acceptance of new ideas until they are well proved and tried (Beveridge, 1957). The fear of being swamped by new-fangled notions may also be the clue to planning research. By this I do not mean that freedom of expression in research should be denied. This should never be. But I do suggest that general objectives should be aimed at in research establishments concerned with applied research and that these objectives should be stated in a written programme. This idea is very neatly brought into practice in the Forest Research Division Manual (1956) of the B.C. Forest Service. There the Research Division is mainly concerned with applied research, and they have devised their programme to suit their needs. However, on close analysis of this programme, one can see that the terms of reference are quite broad. It is designed in such a way that the work of the research officers is controlled without being really curtailed. If a man in the field has a good idea, or any idea, he will be heard and, on consideration, will be given either the red or green light. The main thing is that a programme is devised so that there is coherence within any one area of research, that there is association between areas to make the work as relevant and complete as possible and that there is an overall pattern which can be followed and is so recorded and filed that it is readily accessible for observation leading to completion, expansion or rejection of the work.

The field of basic research presents another picture. In applied research it is the project which is given support, whereas in pure research it is the man (Beveridge, 1957). In the latter case we are talking of a man of exceptional merit whose work should not be interfered with, and should not be controlled by set objectives. He should, rather, be given scope to fully express himself and, sooner or later, he will contribute material of lasting value, and most likely, of eventual applicability in the applied field. Essentially he is feeding information into the fund of knowledge, which is of itself a worthwhile objective.

We have digressed somewhat from the three-fold problem facing the forestry research worker. On reflection of the points (time-lag, complexity and cumbersome nature) we find that, should we have a programme we are in a much better position to take the individual parts of this programme and elaborate to see how far we can work on them. A problem examined a piece at a time is far less frightening than trying to encompass the whole. But these three fates are likely to be present in any item on the programme and, logically, can be interpreted in one of two ways.

(a) We accept the limitations imposed by the time-lag, the complexities, and the general awkwardness of field experiments and we act accordingly, or

(b) We try to find a short-cut. *Caution*—should we find a short cut we must realise that this in itself will have limitations—relevance of extrapolation which in many cases may be quite an unjustifiable procedure, and so forth.

The development of a reasonably well-organised programme will help to assign experiments to (a) or (b) and will help also in projecting the value of either or both of these approaches.

The main thing is that, in forestry, we have these problems. We cannot stick our heads in the sand and forget about them. We are, in fact, so surrounded by pressing needs in the whole sphere of forestry research that we have had to put priority on certain subjects. While it is axiomatic that first things must be treated first, a great deal of relevant material tends to be pushed aside, often without realising what is being lost and without seeing where a knowledge of the fundamentals can give great assistance in promoting the development of directly applied research and its interpretations. This has been brought about by a feeling that neither do we have the time to delve into the background, nor is it in the mandate for forestry research work at the State level in this country. As long as State-sponsored forestry research is the only major research in the field, then these notions must be rejected or else a situation of "the more haste the less speed" will develop. The time-lag factor may also lead to erroneous conclusions and an effect observed after a few years of treatment may have little bearing on the long-term result. In this case unthought-of interactions may occur which would later nullify the initial achievement.

A lot of the uncertainty can be eliminated by using the right approach to the problem, by adopting a method which is sound.

THE METHOD

The greater portion of research in Ireland (and all of forestry research), as indeed in most countries, is applied research — the solution of a specific problem of economic importance by deductive methods, generally evolved from the inductive methods of basic research.

Let us take a hypothetical case which, though it superficially appears to be sound, has hidden dangers for the unwary. By this means I can illustrate further the fundamental approach of the Americans in applying basic principles to the most practical question — how to go about a job that must be done.

1. Problem arises or is appreciated as being of relevance.
2. Consultation shows that it should be investigated.
3. Material is gathered, or plots are laid down.
4. Material is examined, or plots are treated.
5. After examination of material, or measurement of plots, on termination of experiment, a report is presented with conclusions.

Satisfactory? Perhaps — but there are possible pitfalls which are frequently either overlooked or not realised at all.

These are:—

1. The problem, being of some importance, may be treated as an entity in itself and other important considerations may be missed.

2. Consultation may not be enough. A background to the problem must be studied as far as possible — even to rejection of the idea.

3. Gathering of material, or laying out of plots without consideration of all the involvements may lead to wasted effort through inadequate preparation, work on an erroneous assumption or experimentation without design.

4. All aspects of the material or plots may not be recorded, and relevant data may be missed that cannot be recalled due to

being obliterated in the experiment or confounded my superimposed treatment.

5. Should the above be the case, how can one present a report?

An approach of a rather different nature has been evolved in America. This has been, for them, an easier job by far because of the nature of the American people and their background which is unhampered by tradition. In the American environment utility and hard reasoning have evolved from the pioneer spirit. This is considered by Europeans as one of their shortcomings, and perhaps in the arts the results leave much to be desired. But in research this has some considerable advantages in that a fundamental approach has developed. They do not have the inhibitions frequently generated by traditional "schools of thought". Matters are pared down to the essentials and from there on they can, if necessary, be built up. If analysis shows that background information is lacking the first step is likely to be an attack on fundamental problems before approaching the immediate question and, assuming that there is a need for this, they consider the money well spent. Do we?

We can again take the problem and re-assess a new sequence of events.

1. The problem arises, and in the case of applied research is generally noted to be important.

2. Consultation shows that it should be looked into.

3. The observed facts are recorded as well as possible and assessed. From the assessment a statement of the problem is made.

4. All reference to this problem is sought and on analysis should yield information of the following type:—

   (a) The nature and extent of earlier work in this field and the conclusions arrived at.

   (b) Possible pitfalls.

   (c) Details of possible methods of approach.

   (d) Interactions which should be noted or possibly eliminated.

5. Further discussion decides whether the project should be accepted or rejected.

6. A final concise statement of the problem is made in the light of new knowledge. This is formulated in terms of a *Working Hypothesis.*

7. On the basis of the working hypothesis a method by which the investigation is to be carried out is set up and is strictly adhered to, unless there is a strong and valid reason for changing technique.

8. The experiment is set up, great care being taken to eliminate heterogeneity and human bias. Where the former cannot be eliminated sufficient samples are taken, or plots established, to cover the possible variation. In other words, the experiment is set up according

to a statistical design that will stand up to rigorous examination for validity.

9. In recording the experiment sufficient data are collected in an objective way to eliminate human bias and allow an assessment to be made that will ensure that the results of the experiment are an expression of the truth.

10. Any report presented must be such as to be a statement of fact. Any conclusions made must relate specifically to the evidence, and where this is not strong no positive conclusion can be made, although subjective interpretation may be made if it is stated as such.

The mehod of handling the problem outlined above is obviously more complex — there are 10 steps where before there were five. What of these five added difficulties? Unless they serve a purpose they are worthless. As I trust I am not pouring out worthless suggestions let us see what new contributions they make.

In essence their purpose is to reduce the degree of empiricism in handling research. The first two points do not vary — they are merely an appreciation that there *is* a problem.

The third point presents a radical departure. This is that a full preliminary investigation is made and that consequent on that one makes a statement, i.e., one commits oneself (though not irrevocably) to a line of thought. The value of this is that it forces the observer to coherent thought, it points out problems and reveals complexities and it tends to make the observer, or research officer, aware of what he has to face.

Point 4 — reference — has much to recommend it. However reading must be tempered with reason. Beveridge (1957) warns against believing everything one reads and also allowing reading what others have written on the subject to condition the mind to see the problem in the same way and make it more difficult to find a new and fruitful approach. Many workers have achieved great things with little or no scientific background to their work (e.g. Bessemer). But I would like to quote him further:

> "The best way of meeting this dilemma is to read critically, striving to maintain independence of mind and avoid becoming conventionalised. Too much reading is a handicap mainly to people who have the wrong attitude of mind. Freshness of outlook and originality need not suffer greatly if reading is used as a stimulus to thinking and if the scientist is at the same time engaged in active research. In any case, most scientists consider that it is a more serious handicap to investigate a problem in ignorance of what is already known about it".

It is very important to read critically, and a frequent mistake is to believe too much, not to distinguish between the results of experiments reported and the author's interpretation of them. Therefore, in assessing the conclusions presented in scientific articles, we must weigh up what is objective and what is subjective in them. But there can be no doubt that if one is embarking on a project which is somewhat outside one's normal experience a large amount of reading *must* be done before any work is attempted—to follow clues, to avoid pitfalls, to assess methods and to evaluate the effects of interactions.

Then comes the final discussion. The case comes up for trial, or to carry the analogy further, it is rather like the taking of depositions. All witnesses are called — in the form of observations and literature references — and from an evaluation of the statements made the accused goes on for trial or is discharged. Here, of course, a lot of modification may be introduced, and a weak case may be so modified that it then becomes a strong one. Discussion is invaluable as fresh minds are brought to bear and may reveal striking weaknesses not noticed by the researcher because he was too close to the problem and too bound up in it.

Assuming, then, that the project is accepted the formulation of the working hypothesis gives the research worker a framework in which to operate and also lets everyone know, both up and down the line, just what is going on. If these objectives are to be achieved the working hypothesis must be clearly and concisely stated and it must also be a full statement, not of every detail but of the nature of the problem to be solved.

Obviously the method employed in the execution of the experiment is of paramount importance. All the previous research and discussion will be to no avail if the set-up of the experiment is faulty. Likewise meticulous observation and recording will be useless unless the factors observed are those required, and unless the methods by which they are recorded are above reproach. How many publications have been issued that on crticism have been as watertight as a bottomless bucket? Much early experimentation, particularly in the basic field, was of a beautiful black-and-white nature. Boyle's work in the 17th century had this element, to quote but one (Conant, 1961). Both this writer and Beveridge (1957) show how a great deal of immeasurable value arose from a flash of intuition in which black-and-white hypotheses were stated. Where they do occur the main methods of attacking them may be as follows:

1. Method of description and classification. This has limited application and is mainly confined to the discovery of new organisms and materials.

2. Evolutionary method. In this comparative or genetic method common origins and relations are assessed.

3. Method of determining causal connections. This entails simple inductive methods in which one variable in an experiment produces a certain result. This factor may be determined by noting the absence of the said result when the variable is not employed (factors a.b.c.d give results w.x.y.z and a.b.c give w.x.y, therefore d gives z); by noting results of varying strength when the value of the variable is altered (a.b.c.d_1 give w.x.y.z_1, a.b.c.d_2 give w.x.y.z_2, etc., therefore

d gives z); and by observing the said result appear consistantly when the factor is employed with other variables (factors a.b.c.d give w.x.y.z, e.f.g.d give t.u.v.z, h.i.j.d. give q.r.s.z, etc., therefore d gives z), to illustrate a few of the techniques.

In many of the above the cause and effect are clear cut. If it is mere cause and effect where cause "d" gives effect "z" then the system is a "stop-go" one (Riker and Riker, 1936).

There are still areas where this virtual utopia of science exists, but they are few and far between. Mostly we measure things by degrees of difference — greys of varying tone are introduced. In forestry research we can say, almost emphatically, that it is among the greys that we work. So the question emerges: when is a grey not a grey? This is no facetious question, though it may appear so. Response to fertilisation, the effects of breeding, the results of provenance trials, the response to thinning — these are all measured in degree, the answers are shades of grey. As trees generally grow without fertilisers, or controlled breeding and so on the problem is not a "stop-go" one. Add to this the confusion caused by variability among trees, between sites and even within sites and the variability of climate from year to year and you find that even the refinement between shades of grey becomes important. Now, you cannot say that, because the complexities are so great, you will ignore them. Do this and you will find yourself out of your depth in no time. The answer lies in the design of the experiment. Patterns of design have been evolved — randomised block, latin squares, etc., as also have proper sampling techniques. This evolution is the result of applying statistics to research.

In this paper it is impossible to deal with statistical design and analysis, but some pointers may not be out of place. Firstly, statistics must be recognised as a tool to be used by the research worker. The function of the tool is to aid in the resolution of phenomena which are too complicated for treatment by the methods so far considered. The statistical method deals with complex and difficult problems in a scientific manner, It takes into account the laws of chance, eliminates heterogeneity and draws conclusions that fit all the measurable variations. But a word of warning may further be quoted from Riker and Riker:

> "Because it solves problems that can be handled in no other way, it has been misused and its importance has sometimes been overemphasised in the popular mind".

Another word of caution by these authors can best be expressed by using their own words:

> "No amount of statistical technique can serve as an adequate substitute (1) for a direct knowledge of the phenomena under investigation (2) for familiarity with them, and (3) for accuracy in taking records. Careless spots do not come out in the statistical wash".

The above discourse covers points 7 and 8 in the approach to the problem. Point 9, the taking of records, has also been hinted at but may be elaborated on. One cannot change horses in mid-stream without incurring serious consequences. This sweeping statement refers us back to the experimental plan, and very often it is only at the time of taking records that faults in the plan emerge. The temptation to change the nature of our records becomes very strong when we see that all is not going too well. Though a trite statement, the obvious way out is not to make the mistake in the first place; make sure that the plan is complete and faultless. This is a tall order, especially with forestry research, but every effort should be made to approach perfection — hence the prolonged preliminaries. With good fortune it may be possible to modify the experiment half way through — or even at the end — but the fear exists that, in so doing, objectivety may be lost. Modification generally follows personal bias — either to make the work simpler or to contort the experiment to fit our own requirements of it. If we have a mess on our hands it is unlikely that we can pull the fat out of the fire without scorching our hands, and it is generally wiser to admit to ourselves that we have a mess and do as should be done with a mess — dump it! Naturally this statement does not cover all cases and it should be employed with discretion, but the sense behind it should not be lost.

The analysis of the results is best done by the employment of statistical means. Here again time does not allow for a discussion of these means, but where an experiment has been established according to an acceptable design and where data have been collected in such a way as to eliminate bias it would be shameful waste not to subject these data to the final test of an unbiased analysis. It is amazing how one can get a preconceived idea of the result by glancing at undigested figures, though in most cases, if there are enough figures, one can get no idea at all. Beveridge (1957) warns strongly against the "should-ought mechanism" which has no place whatsoever in science. He also cautions the use of interpolation and extrapolation. In the main the former may be employed with reasonable impunity, especially where there are sufficient data. The latter can be dangerous — e.g. extrapolating growth response after fertilising during the first few years to predict growth rate in 20 years' time or trying to make results of fertiliser greenhouse trials fit field experiments. But often extrapolation may serve as a useful basis for further experiments after a preliminary study.

In the above discourse on the method of approach to experimentation I may have sounded as if these thoughts were completely my own. Would that they were, but they are, in the main, a synthesis of the American approach to scientific investigation. They show, in essence, that the Americans advance cautiously. They do not jump in at the deep end. If there is a knotty problem of check in plantations they do not dose the trees with N. P. K. but go back to the fundamentals, to basic physiology. To quote E. C. Stone (1957):

> "In view of the few people engaged in forestry research, *non-basic* research appears to be a luxury we can ill afford. Most of the low apples on the tree have already been picked; from now on we have to reach".

This quote reiterates my earlier comment regarding the need for fundamental research. It implies that applied research has almost arrived at the point where no further useful results will emerge without a background of basic research. In many cases we can say that such-and-such a treatment produces a satisfactory response, so what more is needed? But, do we know why? Without the "why" to support our efforts we work in the dark with the possibility that only half the problem is solved, unaware that so much more could be achieved.

The 10th point, presenting a report, is also a most important matter. In fact I wish to devote a special section to this topic, and its wider implications of getting the message across. Even though the previous paper dealt with publicity to quite a degree, there are aspects peculiar to research which may be stressed.

COMMUNICATION

When a worthwhile experiment has been done it is the researcher's duty, not only to report to his chief what has been accomplished but to make this knowledge available to others in the field, and even to the public. Practical advantages for the researcher lie in the publishing of reports in that they help to synthesise what has been achieved, maintain liaison with other workers, and introduce him to more people in the field which could lead to useful co-operation; to these there may be added the increase in stature of the man involved. Basically, of course, publications of any sort show that work is being done and, as will be pointed out later, the advantages to be gained from such a demonstration should be more far-reaching than the confines of the area of research involved. Although scientific communications on forestry research have been presented, we have not done enough, we should be publishing more.

Scientific Publications

The first requirement of a publication is to present the information received. Secondly, it must give this information in a logical and accessible manner. To achieve these aims there is an almost standard method employed in most American scientific journals, i.e.

Abstract.
History and literature review.
Methods.
Results.
Discussion.
References.

We in Ireland have a tendency to balk at the imposition of standards. There is no need to emphasise how detrimental this has been in the commercial field, and it is also something to be appreciated in the field of science. If we lack coherency in our publications then, as these are our means of communicating our scientific achievements to the outside world, we suffer in our reputation as scientists. By use of the headings listed above we present a logical sequence of events and any aspect of the work may be appreciated at a glance.

The abstract shows, in a brief summary, the nature of our work and, to the scientist who has a large amount of reading to do, it shows, at a glance, whether the material is relevant or not to a particular investigation.

The history and review of the literature reveals the background to the work which often points out analogies and shows as well the extent and nature of similiar investigations.

The methods should be concisely stated, quoting references for established techniques, and more detail for new ones as these can be very pertinent to one's assessment of the experiment and also helpful to other workers.

The results are a statement of fact without comment. They can be examined without bias, and should be presented in just that light. Tables and graphs are a great advantage.

In the discussion the scientist presents his own views on the significance of the data. It is here that the relevance of the statistical evidence, if any, can be discussed, even challenged. Very often application of the results may be suggested, e.g. effective levels of fertilisation, remedial treatment for disease, etc.

To have the references compiled at the end of the text is far more serviceable than to have them inserted as footnotes. They are also more meaningful if they are referred to in the text by the author and date rather than by numbers. This is becoming the accepted practice in many American journals now.

Perhaps I may be accused of triteness in the above comments, but frequently a statement of the obvious is desirable as such things may be so obvious that we are not aware of them at all, and so tend to forget them when we are put to the test. A pertinent elaboration on the above can be found in Duffield (1965), written by a man who has spent many years in the editorial field.

Naturally all articles will not lend themselves to this treatment but virtually all reports of experiments will, and it is a positive case of showing advantages in standardisation.

Communication to the Public

Arousing public awareness of forestry and, more than that, stimulating a sympathetic interest in it and its problems can, and should, be fostered by people engaged in research. This is achieved by publications, reports in the press, lectures and films. All of these techniques are used in the United States whereas we in Ireland have used but one, the lecture, and that to a very limited extent. In this age of promotion almost every conceivable venture can benefit from well conceived and designed public display and to display the wares of forestry in terms of reporting, in digestible form, the progress in research helps to focus attention to this sphere of activity and in so doing draws the public into closer contact with the work and aims of the Forestry Division. I have seen the work of the Soils Department of the University of Washington appear not only in many of the leading scientific journals but also in the local Sunday newspaper, in tastefully prepared brochures issued to the public and also in an advertisement in a nation-wide magazine (which, incidentally, showed the extent of envolvement of private industry in forestry research).

The public is now becoming science-conscious. People can be told what research is doing. The form of the literature would obviously differ from the scientific report. A literature review would not be approrpriate and just a passing reference to the methods used would be employed. More emphasis would be placed on why the work was done, which is normally obvious enough to the scientist. Results and discussion should be presented, but briefly and in layman's terms with a strong emphasis on the possible implication and application of these results. Needless to say, the appearance of such a publication should be eye catching and have an appeal that forces the reader to open the cover and read it through.

Good relations with the press should be fostered and coverage of aspects of research should be encouraged. There is not much point in allowing neophytes to write articles for the press on forestry matters when more harm than good may result through lack of sufficient knowledge of the topic. The Forestry Division should develop better public relations so that either it can provide digested information to the press or else assist outside writers to present articles that are more realistic and conform with the known facts.

Is there any reason why contact with the press and the publication of suitable literature should not be used to promote the idea that private industry can also play its part in financing research in the universities?

Encouragement of Research

> "Curiosity and love of science are the most important mental requirements of research. Perhaps the main incentive is the desire to win the esteem of one's associates, and the chief reward is the thrill of discovery, which is widely acclaimed as one of the greatest pleasures life has to offer.
> There is real gratification to be had from the pursuit of science, for its ideals can give purpose to life". (Beveridge, 1957).

To what extent are the above fostered here? I suggest that the "thrill of discovery" is one thing that is rather poorly nourished in our environment where the scientist and research worker is often thought of as an essentially useless type who has little relevance in the material age of commerce. The contrary is the case and this attitude must be dispelled. Given the right men with the right tools research will pay dividends, that is assuming that those in management and administration will listen to what he has to say. Unless he is heard and heeded the scientist would be better off not to exist. This is even more relevant in the case of applied research than in basic. Much of the latter is an end unto itself, but applied research is a service to the community, and to have the results of his work ignored because they do not fit the preconceived pattern, or because he has unpleasant things to say is, apart from an ostrich-like attitude, a horribly frustrating and degrading experience for the man involved.

Even in the United States, where research is on a far sounder footing and where it commands far greater respect than here there have been pleas for improvement. E. C. Stone (1958) in the concluding article of the dialogue on basic and applied research recommends the following:

1. Each Forestry School should re-examine the Ph.D. programme related to basic research.

2. Students should be encouraged to do research.

3. The U.S. Forest Service should limit the number of research stations and have each well equipped rather than a great number of poorly equipped ones.

4. The men trained for basic research and who will use the facilities should be allowed to plan the facilities and not the administrator who, for all his proven ability to deal with people and papers, may never have been an active participant in basic research.

5. Forestry Schools should improve their research facilities.

In the 4th point above the comments are most applicable to basic research, it is true, but they are also relevant to applied research. Perhaps a certain control, or rather guidance, should be given by the administrative section, but obviously the man who is doing the work knows best what he needs, and in this I say strongly that not only the chief research officer but also his colleagues, assistants and technicians should have a say in the development of facilities. Remember, without the technician not much would be accomplished.

It is not my place to talk on education, but for the development of a good research worker it is axiomatic that he receive his training in a place where research is fostered and respected.

Even for the man who is not inclined to research such an environment creates in him a respect for research. One cannot divorce education from the development of an active enquiring mind, and an active mind is best nurtured in an environment where scientific endeavour charges the atmosphere with inquisitiveness. The mind must be taught

> ". . . to realise that the concepts of to-day are not unchangeable but rather, are merely the best we have to-day; that all truth is progressive, not static". (Fletcher and McDermott, 1959).

I would like to transpose the suggestions of the above authors from their applicability to teachers to an applicability to research workers, i.e. that they be

(a) encouraged to conduct research or advanced study;
(b) provided with time and facilities for these activities;
(c) rewarded according to their competence and performance in either endeavour.

The encouragement, the facilities and the rewards are not enough in this country. In sustaining the research student (or worker!) Nearn (1959) also states these ideas by suggesting that he needs the following:

(a) Challenging and significant areas of work.
(b) The field populated with people whom he can respect.
(c) That if he becomes competent in his field he will be accepted by workers in other scientific fields as an equal,
(d) and that his material rewards will be at least sufficient to enable him to live in a manner which he is almost certain to desire because of his intellectual level.

The glorification of the salesman or business manager, etc., by the timber press, while belittling the researcher either turns him towards a more remunerative business side or else completely away and to a field where he will be respected.

Job interest and the need to feel wanted are essentials to any productive effort. Incorporated in these facets are respect and appreciation of the researcher's efforts by the administration, by the timber industry and by the public at large. The administration can contribute by fostering research and by heeding the words of researchers. The industry can, and should, contribute funds to research, and public respect can best be developed by a national appreciation of research, by more research-oriented university training and by good publicity. Most of the above become simplified with money. Total annual expenditure on research in the United States was, in 1959, about 1.3% of total *consumer expenditure,* whilst expenditure for forestry research amounted to 0.2% of *consumer expenditure* (Cowlin, 1959). Why is forestry always the poor cousin? The trend now is to increase expenditure on research in forestry, with an aim towards achieving parity with that of other industries. One can realise that, where forestry is a remunerative business, the percentage of *investment* devoted to research will be considerably greater than the figures quoted, whereas in Ireland the situation occurs where expenditure exceeds income by a great degree. But we are building for the future and should think in terms of our prospective income. The Forestry Division's investment in research may compare reasonably well with American figures, but I feel our relative need for research and facilities is far greater than theirs; we have a long road to travel to catch up, both with the rapid expansion of the forestry programme and with the ever increasing needs so imposed to improve our forest production. Of course, I refer here to a percentage of that money devoted to direct forestry work. What about the total involvement of money in the produce of the forest? This is quite immense and this source has hardly been tapped. When all is said and done we are hardly giving forestry research the means to show its true worth.

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