SELECTION AND JUSTIFICATION OF RESEARCH PROPOSALS

By

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Address at a Seminar on Presentation of Scientific Papers organised by Natural Resources, Energy & Science Authority of Sri Lanka held on 22.10.1983.

SELECTION AND JUSTIFICATION OF RESEARCH PROPOSALS

I had the privilege of professional involvement in research and development or more appropriately research for development to understand and perceive the psychological and mental processes of some of the most knowlegeable men and women like you in research, planning, direction and education and more importantly been associated with the inception of the Agricultural Panel of the then NSC and now NARESA in evaluating research proposals of varying dimensions.

Let me for a moment ramble for a while as a matter of mutual and national interest to all of us. Sri Lanka is the 16th poorest country in the world, 50 per cent of her population is below the poverty line receiving an income of less than Rs.300/= per month and 90 per cent find cost of living unbearably high - and of course many more maladies like galloping inflation, increasing debts, widening budget deficits etc. haunt our economy today. Scientists in Sri Lanka like in other developing countries work under financial, manpower, technical and sometimes even management constraints - these singly or collectively lead you into false starts and the desire to achieve a broad coverage with meagre resources at hand, usually at the expense of quality ends up in disastrous failure and frustration. I have seen and evaluated research proposals that ranks on par with some of the best in the world, whilst others down the scale some even to the extent of being mediocre. My honest opinion is that too many of us in Biological and agricultural research fail to understand their responsibility to the nation, project or the terminal point

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or beneficiaries of the results of their expensive research and investigations. I have had the unsavoury experience of asking a young scientist who was somewhat over-conscious of his dress to step into the paddy field so that he would learn how deep he sinks in the mud even if he learns nothing else in this practical exercise. On the other extreme, I had the greatest pleasure calling young researchers once or twice a week and sharing with them the 'inner secret' of our profession - perhaps one or two of them are in this audience today.

Let me shift now to the subject assigned to me 'Selection and Justification of Research Proposals'. Perhaps in the normal bureaucratic process I would refer you to item 9 on page 4 of the NARESA application for research grants which reads "Significance of overall problem in the light of the development needs of the country", fearlessly conclude my address and refer all your questions to the Director General of the NARESA. However, within the framework of social and economic goals of a developing country like Sri Lanka many economic development activities depend on, and use of a data base developed through years of well designed research, investigation and experience and in this context further elaboration and justification of the subject becomes necessary.

Thoughtful selection and planning of research projects is much more difficult than mechanistic Cookery book or ivory tower approach to problem identification. Greater reliance on common sense, tradition, experience, comprehensive literature review, 'thinking through' the information and analysis are

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all useful tools in decision making, in this case final selection of research projects. Historical or sequential analysis or in other words a comprehensive literature review is most critical to proper problem identification, a practice woefully lacking in the applications received by NARESA for research support.

It is not the intention of this paper to tred the dangerous path of discussing academic or fundamental versus applied or adaptive research. The final goal of developing countries like Sri Lanka is economic development, also synonymous with economic growth that generates a social change ultimately resulting in a decent and satisfying living to all citizens - in turn dependent on full employment, high standard of living and good health and nutrition. Almost all economic development activities depend on, and use of a reliable data base developed through years of well planned research, investigation and experience and it is in this context that I wish to identify my perceptions on the nature, scope and justification of research in Sri Lanka.

Selection and planning of research projects must benefit the terminal point or user at the village, regional or national levels. In contrast scintillating discoveries may be made by a researcher working on the 13th Chromosome, if there exists one, of Cymbidium species that satisfies him alone with no benefit whatsoever to the expanding orchid industry of the country or another may spend almost his lifetime on research on the Anophelese mosquito generating exciting and satisfying results without fully appreciating his responsibility that the findings should extend to the terminal point or benefit his society that funded the expensive investigations.

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Some scientists are quick to react to sensational news items like the establishment of an International Winged Bean Research Centre or a National Environmental Authority, subjects of special interest to this audience. The consequence had been a flood of applications for research support from NARESA in fields of research which I cannot perceive would have neither short nor long run benefits to our economy. It is not the intention of this paper to determine for example the benefits of either winged bean or any research relating to this vegetable known to Sri Lankan peasants for several centuries and I leave it to those more knowledgeable in the academic world and decision makers for a better definition of its character, purpose and usefulness. The above illustration should not be measured with any malice nor prejudice, but is indicative of the scale of priority we tend to establish in the selection of research projects as part of the development strategy of the country.

Sri Lanka has totally ignored the potential of collaborative research despite the creation and multiplication over several decades many problem or regional oriented research Institutes or Faculties of Universities. Collaborative research or cross fertilization is essential not only among scientists of different disciplines working within a single Institution, often grouped together under one roof, but also among those between Institutions, Faculties and Universities for greater effectiveness of research on economic development, maximum utilization of resource personnel, equipment, finances etc. As an illustration greater benefits to the society have resulted from cross fertilization of biological and chemical sciences, now

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termed biochemistry rather than through research generated independently by the two sciences biology and chemistry. I may also refer to a case where 3 or 4 researchers of the same Institution requesting independently to investigate 'X' on the growth and yield, pest resistance, quality of produce etc. with separate estimates for procurement of equipment, personnel, travelling etc. with the customary official seal recommended and forwarded by the Head of the Institution. Research scientists, more particularly in this case Directors and supervisors must broaden their horizons and begin to feel that they are not all competing for the same prize - they must all join forces to select, plan and justify research projects to make them collaborative or team work and thereby make them more economical, effective and useful to the terminal point or the beneficiaries of their research findings.

My job experience particularly during the past ten years afforded me an excellent opportunity to interact with both foreign and local scientists of different professional interests - engineering, hydrology, agriculture, social science, economics etc. They all contributed towards the achievement of the Government's goal of harnessing the Mahaweli Ganga resource with its multiple benefits to the nation. Excellent feasibility reports on reservoir construction, hydro-power generation, irrigation, human settlement and agricultural development have been produced by the collaborative efforts of multi-disciplinary teams of scientists - namely engineers, hydrologists, agronomists, settlement planners, economists, environmentalists etc. In contrast I have had the unsavory experience with scientists of certain professions turning a deaf ear to those of others without appreciating that the interests are critical to the goal of economic development - these are understandable sins of omission as well

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as commission in multidisciplinary approach to scientific research or investigation. What causes such apathy? In my opinion it emanates from an incomplete comprehension of the ultimate goals of research and investigation, that is, economic and social welfare on which one's research should be targetted to in a developing country.

Certain situations demand limited snapshot or shotgun approach to problem identification and investigation than involve in sophisiticated and long term experimentation. Well designed surveys of established traditions and practices weathered over years of trial and error offer insights into selection and preparation of future research models. Similarly introduction and adaptation of plant or animal species from regions approximating Sri Lankan conditions would generate quicker benefits than launching out on an expensive and comprehensive plant or animal breeding programme with its inherent technical and economic uncertainties. It would prove stupid if all geneticists and breeders down tools and depend totally on borrowed technologies, though having a great potential for adoption and offer quick returns, may not necessarily fit our precise requirements.

Finally, an important adjunct to selection and implementation of research programmes is the availability of equipment and instrumentation - airconditioned laboratories, ultra modern equipment, phytotrons, computers etc. I do not hesitate to say that these are the tools of modern scientific research but can we afford this luxury in every Institution or laboratory in the country? On the other hand have the existing equipment and capacity

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utilized to its maximum. Foolproof, non-bureaucratic systems of sharing the available capacity among scientists must be evolved in the greater interests of the scientific community.

In conclusion, let me recapitulate quickly some of the criteria discussed above, in the Selection and Justification of Research Proposals.

(1) Generate and re-model biological and agricultural research to meet the economic and social aspirations of the nation.

(2) Comprehensive literature review and 'thinking through' the information would eliminate false starts, waste of resources and disappointment.

(3) Model the research projects to benefit the terminal point or user of the research findings.

(4) Generate collaborative research among different professional interests within and between Institutions or Faculties as results from collaborative research are often more rewarding and economical.

(5) Limited 'snapshot' research and surveys generate better insights on problem identification and even quicker and more economical results.

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(6) Generate foolproof, non-bureaucratic systems of sharing and use of existing equipment capacity in the country.

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THE PREPARATION OF A SCIENTIFIC PAPER FOR PUBLICATION IN A JOURNAL

BY

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INTRODUCTION

The young scientist must accept that the goal of scientific research is publication. There is no other easy method of assessing a research worker's ability than the study of this research papers. Therefore, however capable a scientist is, he will remain unknown, unless he publishes the results of his research findings. It is also important to know that writing a scientific paper for publication in a suitable journal is relatively easy, if the writer follows a few simple rules. One of the most important of these rules is that the paper must be written in a clear logical sequence, in simple, easily understood language, so that the reader can easily get the information that ~~you are~~ the writer is presenting to him. This paper gives details of the general rules that should be followed for preparing a scientific paper for publication. Each scientific journal has certain special requirements in regard to subject headings, references, footnotes and other components of the paper. These can be easily studied by reference to the Journal concerned; as each of them carries a "Guide to the preparation of papers for publication" at least in one issue per year.

THE BASIC STRUCTURE

All Scientific Journals usually require the following sections, after the title and author's name in the papers to be published:

SUMMARY	
INTRODUCTION	- which states WHY you did the study
MATERIALS & METHODS	- explains HOW you did it
RESULTS	- records WHAT you observed
DISCUSSION	- discusses HOW you interpret the results.
ACKNOWLEDGEMENTS	
REFERENCES	

~~Let us see whether we can agree on some simple rules for the preparation~~

Let us see whether we can agree on some simple rules for the preparation of each of these sections, which will be acceptable for all scientific journals, in general.

First, the title of the paper is very important; as many people will read the title and it is on the basis of the interest created by the title that most people will decide to read the full text. Therefore, it is worthwhile preparing a meaningful title, which is one that "in the fewest possible words adequately describes the contents of the paper" (Day 1979). In this age of computers, it is important to remember that there are 'key words', and abstracting journals depend heavily on them; therefore, it is essential to give an accurate title, otherwise the paper can be lost to the readership it was intended for.

The title should not be too long or too short; but must have sufficient detail to encourage the reader to study the text too. If the paper is on a fungal disease of a plant, it should have some reference to the host and the pathogen and what aspect of the subject is being discussed eg. the physiology of the fungus or disease control.

Summary

The summary should do exactly what it claims to do, and summarize the information in the paper. It should enable the reader to assess the content of the full text rapidly and accurately, so as to enable him to decide whether he should read the full paper. A summary should contain only about 250-300 words and should generally be written in one paragraph without reference to literature cited or figures and tables. A good summary should describe the scope of the study, the methods used, the results obtained and the principal conclusions reached.

It is well worthwhile making an effort to write a good summmy; because the great majority of people who notice your paper will make up their mind to read it, on the basis of the summary. Select the words carefully, use the minimum number of them and make sure that you fulfil the requirements of the summary, spelled out above.

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Introduction

The basic aims of the Introduction are to state clearly the nature and scope of the problem carried out by you, very briefly describe the methods used, summarize the most important results and give the main conclusions. This sequence would produce an ideal introduction to the subject of the paper.

In stating the nature and scope of the study, the reader must be given some background information and this calls for a review of the relevant literature. This section should explain exactly what the problem was and why you undertook the study. The brief review of the methods used shows how you attempted to resolve the problem. The principal results are the outcome of your study, so that the reader is fully aware of the importance of your study by the time he has read the Introduction. This is the purpose of the excercise.

Materials and Methods

You have already referred briefly to the methods used in the introductory paragraphs. Now in the Materials and Methods you must give the full details of both materials and methods used. The main purpose of this section is to give sufficient detail for another research worker to repeat your work. This does not mean that anybody will repeat your studies; obtain the same or similar results, otherwise your paper has failed in this section.

Materials:

Run on

All the materials used in the study must be described in detail here eg. if chemicals were used, their grade, purity and concentration. It is best to avoid the use of propreitory names of chemicals and to use their generic names. This will avoid confusion as the same chemical compound may be known by different names in different countries.

Animals, plants and microorganisms, when used should be clearly identified as to genus, species and strain, if any. The parts used must be described, the time of collection and the method of transport to the laboratory. This is important eg. in the case of plant parts, the composition of leaves

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collected in the morning will be different from those collected in the evening.

Run on

Methods:

Describe the methods in the order that they were used. If you repeated a method used by someone else earlier, a literature reference to the previous work sould be sufficient; but any modification you adopted should be described fully. If you devised a new method altogether for your study, then that must be described fully.

#

Run on

Results:

The section where the results of your experimental work are presented is the really important part of your paper. You should give all the important data that you collected in your studies, in this section; but there should be no discussion on the significance of the results. The data should be presented in the best possible manner for easy understanding; therefore, tables, graphs, figures and other devices that make the reader's job easy, should be used.

It is essential to be precise in the presentation of results; remember that a table or graph is used to make it easier for the reader to grasp a certain point. Therfore, it is generally agreed that a table, a graph and a long description to clarify the same subject matter, should generally be avoided unless there is a special reason for it. Tables are used to give discrete data that do not show any trend lines, graphs are used where definite trends are present and histograms are used to present statistical data. Statistical analysis are very important; but when presenting them discuss the data and not the statistics. It is also important to confine yourself to relatively simple statistical methods, understood by the majority of scientists, rather than use involved special methods.

This section of your paper should be short and to the point, avoiding the use of excess words. Therefore, the Results, although the most important part of your paper, can also be the shortest, particularly if the Materials and Methods section, immediately preceding, has been well written, and the the results are followed by a good discussion. Remember also that, this is the section in which you are presenting the new knowledge on the subject, that you discovered, so make every effect to write it well.

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Discussion:

This is perhaps the hardest section of the paper to write; because many people can carry out high quality research work mechanically; but it takes a good mind to interpret and explain the correct significance of the results obtained. The essential features of a good discussion are so important that I should like to quote Day (1979) on the subject:

" 1. Try to present the principles, relationships and generalizations shown by the Results. And bear in mind, in a good discussion, you discuss; so do not recapitulate the Results.

2. Point out any exceptions or any lack of correlations, and define unsettled points.

3. Show how your results and interpretations agree (or contrast) with previously published work.

4. Don't be shy; discuss the theoretical implications of your work, as well as any possible practical applications.

5. State your conclusions as clearly as possible.

6. Summarize your evidence for each conclusion."

Do not allow yourself to wander away from the results presented in your paper, in the course of your discussion. In other words, you should discuss only what is relevant yo your results, not the whole gamut scientific knowledge on the subject. Therefore, a good suggestion is to note down each result separately on a paper, think out the significance of each factor, and write down only what is important. The reader should never be allowed to think that you are refering to irrelevant material or padding your paper, when he is reading your discussion. On the other hand, he should be mentally congratulating you on your wide knowledge on the subject, nimble mind, and your ability to interpret the real value of your work.

Acknowledgements

This is more a metter of courtesy than science. It is in this section

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that you thank the sponsors, who financed your work; the technicians, who assisted you, and your colleagues, who spent time with you discussing the significance of your results. In certain cases, it would be best to show this section to those whom you wish to thank for ideas or discussion; as the persons concerned may not wish to be associated with the work done. In the extreme case, someone may decide to thank an eminent colleague merely to have his name as an ornament in the study; whereas the persons cencerned may not have any desire to be so named.

References

There are many ways in which the literature cited in a paper is referred to in the text and listed at the end of it. The best method of handling this section is to refer a recent issue of the journal you are writing for, and to follow the style required by the Editorial Board of that publication.

Although there are many reference styles, the three used by a majority of journals are, the "Harward System", "the number in order of citation" and "the number and alphabetical listing" of references. The "Harward System" is the most popular of these, and in that method the name of the author with the year of publication, in brackets, is given in the text. The references are then listed in the alphabetical order at the end of the paper. Each reference gives the name of the author, followed by his initials, the year pf publication in brackets, the full title of the paper, the name of the Journal, which is sometimes given in the abbreviated form or in full, then follows the volume number of the journal and finally the number of the first and last page over which the paper is printed. This gives the complete details for a search of the literature cited, and makes for rapid access to the paper required.

Authorship

I have left this to the end of this paper, so that the writer has a grasp of the importance of the presentation and the responsibility for it. Now the authorship can be clearly and concisely defined. The names of authors listed should include only those who actively contributed to the overall design and execution of the experiments. Names of persons who merely discussed experimental designs or their results should not be listed as authors. The most important question to ask here is: Whose original ideas and thinking

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went into the design and execution of the study?" Then list only those names as the authors of the paper. Many people may have carried out technical work in the laboratory and field, you can acknowledge their assistance; but they need not be authors.

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REFERENCES

Day, R.A. (1979). How to write and publish a scientific paper. I.S.I. Press, Philadelphia, Penn. U.S.A.

(Talk at Seminar on 'Presentation of Scientific Papers', Colombo, 22 October 1983, organised by the Natural Resources, Energy & Science Authority of Sri Lanka)

THE WRITING OF A SCIENTIFIC PAPER

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Introduction

> It is far more difficult to be simple than to be complicated, far more difficult to sacrifice skill and cease exertion in the proper place, than to expend both indiscriminately.
>
> - John Ruskin[1]

I offer these few hints and suggestions to writers of biological research papers, not only as a writer, reader and assessor of such papers for the last 40 years, in the ordinary course of my university duties, but also as co-editor of the journals Biological Conservation (Gt. Britain) and Systematic Parasitology (The Netherlands), former general editor and editor for zoology of the Ceylon Journal of Science: Biological Sciences, and an abstractor (1947-1970) for Helminthological Abstracts (Gt. Britain) from papers published in English, French and German. My main aim however will be not to dish out trite and hackneyed rules, but to stimulate and even provoke.

I have the following two recommendations to make, at the very outset, to the sponsors of this timely and useful seminar, lest they get lost among the various other matters dealt with later on in my talk: (1) Publish a revised and updated edition of Dr Joseph Pearson's

Instructions and Rules to be Observed by Authors and Editors (Colombo, 1933). It was published by the Ceylon Journal of Science, for private circulation only and carries valuable material, with special reference to Sri Lanka, on duties of editors and associate editors, and instructions to authors on such topics as: the general plan of paper; references to literature; footnotes; illustrations (line and half-tone blocks); other general instructions; correction of proofs (with a practical example of proof correction); orthographical and typographical rules; and, what is also very useful, abbreviations of journal titles according to the World List of Scientific Periodicals,[2] and a relevant extract from the International Code of Zoological Nomenclature.[3] In fact this publication, old as it is, could even have been used as a basis for the present seminar. (2) Organise short courses for authors and editors on such topics as (a) botanical and zoological nomenclature, and (b) Greek and Latin for biologists.

One other matter before I get on to my main assignment. My experience has been that one learns most and best of all when one is shocked into a realisation of one's lapses. Such experiences in my own career have made me a near-perfectionist in these matters. Not a desirable thing altogether. But perfectionism pays, especially if one wants to get a paper accepted and published. Therefore, please do not think me malicious in any way, if I sometimes draw upon the lapses of scientific workers, in order to drive home my points.

The previous speaker (Dr O.S. Peries) has admirably dealt with the subject of format and structure of a scientific paper. I have been asked to speak on precision, reportage, language, tables and illustrations, bibliographic references, and abstracting. Let me begin straightway with the subject of precision.

Precision

Precision is a desirable thing, because it enhances one's credibility and status as a researcher. But precision comes only with practice. A fundamental requirement for increase of precision is the reading and re-reading, and re-writing, of what one has written. Actually, typing and re-typing drafts (which is better than writing them in long hand) before one gets to the final typescript for publication, is a rewarding practice. Each time one will discover errors of all sorts, and want of precision. Even after the manuscript has gone to the editor and printer, one must always be onthe look-out for errors, by then small errors perhaps, especially when the paper is in the press.

Precision can refer to many things, such as precision in the use of language and nomenclature (e.g. according to the international rules of botanical and zoological nomenclature); precision in numerical and other data; precision in tables, graphs and illustrations; precision in bibliographic references. A golden rule would be to get a senior, tested scientist (more than one in fact), to read one's manuscript. One should never eschew assessment by those who could claim to know better, even simply by virtue of their having been longer in the field. A fishery biologist could recently have avoided a mistake in the very titling of his lecture, if he had taken the trouble to follow this advice (or was the mistake due to some other factor?). A forthcoming lecture of his was announced by letter and in a newspaper[4] as being on "the behaviour of some endemic cichlid fishes of Sri Lanka", whereas there are no such cichlids in this country. Indigenous yes, endemic no. In fact the same zoologist had earlier read a paper on <u>indigenous</u> cichlids, at a session of the Sri Lanka Association for the Advancement of Science.[5] (I am open to correction here, wondering whether since then endemic cichlids have indeed been found in Sri Lanka).

The rules one simply has to follow without questioning are those of the journal in which one hopes to publish a paper. The paper must be re-written to conform to what Medawar calls the "house style" of the journal,[6] but only after it has been written up, to begin with, without reference to the journal and according to the generally accepted norms of scientific paper writing. As an example of the idiosyncracies of journals, I would cite the case of Systematic Parasitology, which requires that in the references section a journal's complete title should be used, not one abbreviated according to the World List of Scientific Periodicals, e.g. Ceylon Journal of Science: Biological Sciences, not Ceylon J. Sci. biol. Sci. Some journals even omit references to the titles of research articles.

Numerical results, statistical calculations, scientific names of organisms etc., should be checked over and over again. And here I refer to a case which led to an uneasy correspondence between the principal author, the editor and myself. The journal Nature carried an article on "Habitat values and endemicity in the vanishing rain forests of Sri Lanka".[7] Table I in that paper set out habitat value data and indices for the amphibians, lizards and birds of Sri Lanka. The calculations were done on the basis of each of the 4 regions (into which Sri Lanka was divided) having an area which I found to be 1½ times too large (a factor of 1.4, to be precise). The same paper carried in Table II numerous misspelt scientific names of the vertebrates dealt with, showing disregard for the norms of biological writing. All in all, the lack of care and precision was appalling. What is more, when I drew attention to all this, the passing-the-buck syndrome manifested itself immediately. The wrong area-values were blamed on Sri Lanka's Department of Agriculture, and the misspelt names on one of the funniest methods I ever heard of, namely "proof reading by committee". A correction was published in the same journal, stating all this and with due acknowledgment made to me for having pointed out the errors.[8]

One would think that Nature was so taken up by the "bold attempt" (as I myself described it in a chapter I wrote later, referring to this paper[9]) of the authors to work out an index of endemicity, that they assumed that the authors could be relied upon at least to use correct values in their calculations. The paper also has some fundamental theoretical flaws which I hope very much to reveal later on in the course of my own researches.

I think enough has been said to whet your appetite for precision, and at the same time to set you up against me as an intolerable purist. As to the latter, I would only say that I am my own worst critic, but that my lapses are remedied by me, happily, in the course of writing the many drafts before I prepare the final manuscript, and also when I read the proofs that precede publication of my papers. Even then I am often left a bit frustrated in the end.[10]

Reportage

Report only what is relevant and essential to the argument, in regard to past, present and even future work on the subject. On the other hand, do not fail to include anything untoward, anything not quite fitting the hypothesis, or any fact or occurrence in the course of an experiment, which may have arisen even as a result of carelessness, for it may have a significance that is not immediately seen. In short, report briefly, accurately, and truthfully. Truthfully, above all. And here, one can sin not only by commission but by omission as well. The morality scientific activity instils springs from attention to this aspect of one's work.

Report what is "sufficient and necessary",[11] in your preamble or introduction, but do justice to those who published before you. Here is an example of a serious lapse in this regard. As editor of the Ceylon J. Sci. biol. Sci., I edited and published a paper on a pedunculate crustacean's distribution and variation in relation to the respiratory currents of its crab host. The paper was received in June 1967 and published in October 1967.[12] It was later discovered that no acknowledgment had been made in it to a remarkably similar paper, on the same subject, but with different numerical details, published in Malaya in 1964 in a journal[13] which was being received by the

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Colombo Museum. Internal evidence in both papers, and the fact that the particular issue of the Malayan journal was missing at the material time from the Museum Library, made the situation not altogether satisfactory. To my very great regret, I myself did not move in the matter with the author, because by that time the well-known specialist who had helped the author with the literature etc. was going to take it up with him.

The only point I wish to make here, for the benefit of all young research workers, and to put the matter in the best possible light, is that if after writing a paper you discover that you have only more or less repeated work published before by others, modify your paper adequately and generously if it still has something new and worthwhile to offer, so that at least doubts may not be raised in the minds of perspicacious readers; or come up with a brief statement, by way of explanation, in a later issue of tne same journal.

Scientific literature, and especially the journals Nature[14] and Science[15] have many references nowadays to cases of scientific fraud; which comes so subtly within the purview of reportage. The most remarkable case, to my mind, is that of Dr Robert J. Gullis, who was successful for well over two years in deceiving his co-author and director of the Max-Planck-Institut laboratory where he was doing postdoctoral work, and several other co-authors, and the editors of Nature and other journals, and the scientific community at large. His director Dr Hamprecht exposed him in Nature as one who had "invented the results of his experiments". This was immediately followed, on the same page, by Dr Gullis's own admission of "tne curves and values published" being "mere figments of my imagination". He took full responsibility for the want of moral rectitude, for which he had "suffered", and he hoped that his "experiences" would be "noted by others", and he apologised to "the scientific community and the various people involved".[16] This story could well be titled "The Case of Dr Gullis and the Gullibles".

Language

Language is the vehicle of ideas. I feel frustrated when I even think of dealing with the subject of language, owing to the rapid deterioration that has set in, in this area, all over the world. This is especially so in Third World countries, which have become more and more ill-disposed towards the languages out of which much of modern scientific writing grew. I cannot do better here than plead with all who wish to be entertained, instructed, and made deeply wise about this, to read Theodore Savory's admirable book The Language of Science: Its Growth, Character and Usage.[17]

It is significant that hardly any work dealing with the writing of scientific papers refers to the question of language as such, except for admonitions on the need for clarity, accuracy, avoidance of jargon and long-windedness, etc.

By a brilliant reference to, and analysis of, three passages on the subject of man, the first from a psalm of King David's (in English translation), the second from Shakespeare (a soliloquy of Hamlet's), and the third written by zoologist L.A. Borradaile, Savory puts his finger on the essence of the language of modern science, namely (a) the general absence in it of metaphor and simile, and the purple passage, and (b) the dominance in it of Greek over Latin, and Latin over Anglo-Saxon. Here are his figures for the latter:-

	Greek	Latin	Anglo-Saxon
King David	1 %	5 %	94 %
Shakespeare	5 %	20 %	75 %
Borradaile	24 %	15 %	61 %

Savory, however, takes us through the development of the language of science over the centuries, and the contribution to it of other languages as well. But this very telling paragraph occurs towards the end of his work: "Moreover a curious change has begun to spread over the world of science. Workers in the Far East, in Denmark and the Scandinavian countries used at one time to publish the results of their work in German. They knew that the Japanese and the Norwegian languages were not widely read outside their own countries, and they sought greater appreciation of their efforts by having their work translated into a language more widely understood. Today the work in these countries,and others, is very largely published in English..... English shows signs of becoming the language of science".[18]

Since English is almost entirely the language used in Sri Lanka for writing and publishing scientific papers we could only hope that good writing in English would be promoted and fostered by the scientific community in Sri Lanka, by responsible supervision and generous help given especially to our younger scientists who had their early education in their own local language.

Tables and Illustrations

Following on The Royal Society's Scientific Information Conference of mid-1948, a handbook was published in 1950 entitled General Notes on the Preparation of Scientific Papers.[19] This publication deals concisely and succinctly with all the matters of this seminar, and is so useful and valuable that it should be on the desk of every writer of scientific research papers. There is advice in it on how best to prepare tables, graphs, drawings, photographs and other illustrations, for publication.

Pearson's book, which I referred to at the beginning of my talk, is also a mine of information on such matters; and the previous speaker touched on so many important aspects that he has almost relieved me of doing my own thing on this subject. I propose therefore to do no more than draw your attention to the following which comes from my own experience as an author and editor.

(1) Line-drawings are the preferred mode of illustration for scientific papers. They reproduce better

...../9

when reduced, during block-making, to the size required by the journal. They must therefore be large, the larger the better, and the author must have an idea of the degree of boldness lines and dots need to have to be able to 'survive' reduction in size in a line-block. A useful illustration, first published in a book[20] as far back as 1936, is a handy 'chart' for you to have to enable you to know how line-drawings would fare when printed after such reduction. I once had repeatedly to return a number of drawings of graphs for re-doing, owing to the authors' inability to realise that they would not stand up satisfactorily to even the slightest size-reduction. The drawings had eventually to be done in our department, at least for the sake of the journal.

(2) Photographs and wash-drawings do not come out so well on reduction, and are more expensive to print. When they are contrasty black-and-whites, with a gradation from black, through shades of grey, to white, they make excellent half-tone blocks for printing.

(3) Drawings and photographs must carry on the reverse side the author's name and abbreviated title of paper, together with instructions to the block-maker on the degree of reduction required, for example, x ½ linear. The editor, however, will generally have the last word on this matter.

(4) Tables are most useful, but avoid them if you can. Vertical lines in tables are not looked upon with favour by printers and publishers.

(5) You have already been advised about using tables and illustrations only where absolutely necessary, for emphasizing important points in a paper. Drawings are of course almost mandatory in taxonomic work involving descriptions of new species etc.

(6) In this, as in all other aspects of writing scientific papers, precision and a clear idea of what you want to put across, should play a dominant role.

.... /10

Bibliographic References

I personally use one of two subtitles for this section of a research paper, (1) References, or (2) Bibliography. The first is preferred by most journals, where all references in the text must be listed, in detail, under the subtitle 'References'.

The usual advice in journals is to list authors and titles alphabetically, following the 'Harvard System'. The journal Systematic Parasitology, for instance, states: "At the end of the text they should be arranged alphabetically and cited in accordance with the Harvard System, with full journal title,e.g. Bisset, S.A. (1977) Notocotylus tadornae n. sp. and Notocotylus gippyensis (Beverley-Burton, 1958) (Trematoda: Notocotylidae) from water-fowl in New Zealand: morphology, life history and systematic relations. Journal of Helminthology, 51, 365-372". Titles of books and of journals, but not of articles, are printed in italics, and have therefore to be underlined in the manuscript.

Most journals do not want the full journal title, but an abbreviated title, according to the World List of Scientific Periodicals. An important feature of abbreviating is the use of capital initial letters for nouns, and the lower-case for adjectives, and the omission of prepositions, conjunctions etc., e.g. Transactions of the Royal Society of Tropical Medicine and Hygiene, becomes Trans. R. Soc. trop. Med. Hyg. In a sense, insistence on full titles does away with unnecessary work for both author and editor, but printing costs seem to be a factor working against it, especially nowadays, when every extra letter costs money to print.

The subtitle 'Bibliography' is generally used when the list contains, in addition to actual references in the text, those works which have been consulted and are of importance for an overall view of the subject. Most often they are important books, not research articles. Some journals, however, have no set practice in this regard, and use whatever the author prefers. It is here that authors need to be specially careful.

As in all other matters, the golden rule is: follow closely the journal's stated instructions. The more closely this is done, the more endearing one becomes to overworked and exasperated editors!

Abstracting

The Royal Society's handbook mentioned earlier deals with synopses, summaries and abstracts, under the subtitle 'Synopses', and advises authors to find out the requirements in this regard of the particular journal, by examining recent issues of it. I do not propose to reproduce the excellent, detailed, hints given in the book, on such matters as style of writing, and content and lay-out, of a synopsis, but the following are important:-

The title of the paper is usually read as a part of the synopsis and repetition of it in the body of the synopsis should be avoided. A synopsis should be intelligible in itself, without reference to the paper. It should be concise, and in normal rather than abbreviated or telegraphic English, and preferably in the third person. It should be revised and re-revised for redundancy, obscurities and errors.

The writing of a synopsis or summary or abstract, is, in the words of Medawar "the severest test of an author's literary skill, particularly in days when 'précis-writing' has been dropped from the syllabus in most schools for fear of stifling the scholar's creative afflatus. The writing of a summary tests the author's powers of apprehension and sense of proportion - the feeling for what is really important and what can be left out. A summary must be complete in its own limits. Nothing is more abjectly feeble than to write some such sentence as 'the relevance of these findings to the etiology of Bright's disease is discussed' ".[21] For, as is obvious to us, as much as to Medawar, it is the exact nature of this very relevance which is the new knowledge the author is giving the world.

For all that has been said, the writing of abstracts can also have its shady side particularly when writing abstracts for sessions of science associations. This is becoming, if it has not already become, such a 'booming industry' in India and Sri Lanka, that I have often been tempted to write a full article on what I should like to call "The Abstract Method of Running the Rat Race", whereby scientific workers increase the number of their research publications by listing titles of the abstracts read by them at scientific meetings, rather than of the entire papers subsequently published. Abstracts, as such, cannot reveal methods and thinking, and cannot lead other scientists to repeat the work promptly for confirmation or refutation.

Consider carefully the following case which will illustrate this point cogently. Srivastava (1944) presented two papers at the 31st Indian Science Congress,[22] one on the life history of Gastrothylax crumenifer and the other on Paramphistomum explanatum (= Gigantocotyle explanatum). In both cases the snail Indoplanorbis exustus was reported to be the proved experimental intermediate host. Many years later, I wrote repeatedly to Srivastava for the full papers but failed to get any reply. I almost guessed what had happened, for I myself failed to get this same snail to become infected with the same trematode parasite, both of which are common in Sri Lanka too. It was only later that K.S. Singh (1958) discovered that a tiny snail Gyraulus convexiusculus is the susceptible experimental host and that other snails are all refractory to infection.[23] Only the abstract of Srivastava's paper is published, and therefore available, to the scientific world. Singh referred to it, and refuted what it had claimed under cover of such utter brevity. Authors are too frequently in the habit of referring to such abstracts of theirs, and not to any fully published papers, to draw attention to their previous 'discoveries' which may be even so-called discoveries after the manner of Srivastava.[24] It is in this context that I plead for a careful surveillance of scientific papers and abstracts, and a careful follow-up of abstracts presented especially at science association sessions. It has been the experience of responsible scientists that there is far too low a correlation between the number of such published abstracts, and the number of published papers to which they refer.

Let me end this talk by setting you some homework which I am sure you will, with mischievous glints in your eyes, start doing immediately. Make a list of all the lapses in this latest presentation of mine, noting especially any symptoms of that dreadful disease of not practising what is preached, and bearing in mind also that this is not a scientific paper!

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DRAWING GUIDE FOR ZINC ETCHINGS

Letters, lines, and dots in relation to reduction for the printed page. Top—Reduction to 1/4. Middle—Reduction to 1/2. Bottom—Original size.

Reprinted by permission of the authors from: RIKER, A. J., *and* REGINA S. RIKER. *1936. Introduction to research on plant diseases. 117 p. Published by the Authors, Department of Plant Pathology, University of Wisconsin, Madison, Wis.*

Thin black lines may be shortened, but otherwise hold up fairly well in reduction, but small black dots and small white spaces between black lines or dots may be lost. If placed too close together, dots and lines produce black blotches when the drawing is reduced. The shading should be rather open. The degree of reduction needs to be known before the drawing is inked in.

Delicate shading may be obtained if the size and spacing of the dots are adjusted to the degree to which the drawing is to be reduced.

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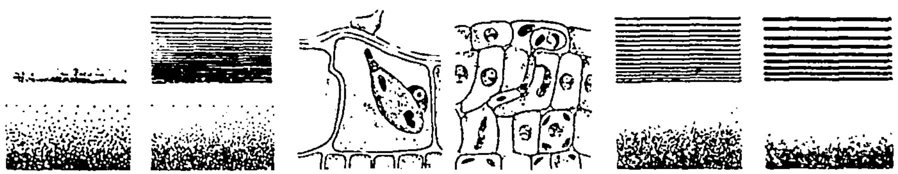

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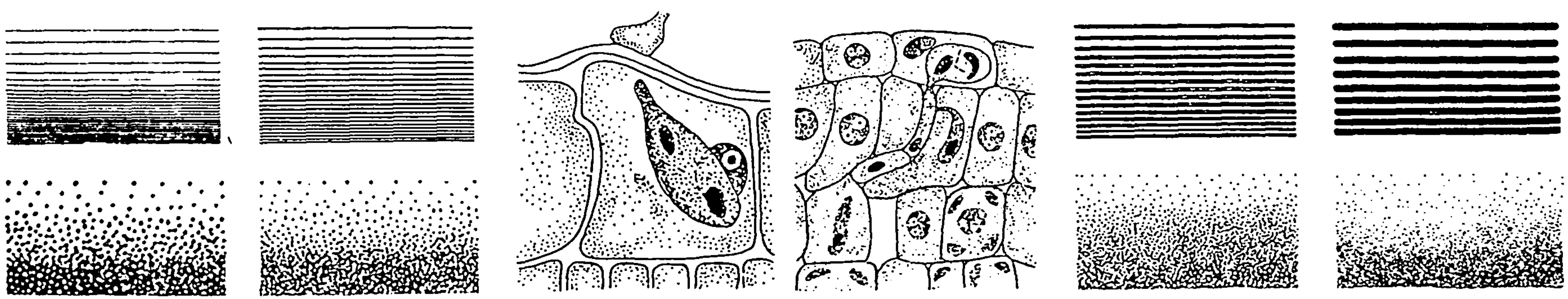

Devonian palaeogeography of Northern Britain

DONOVAN *et al.*[1] have recently summarised sedimentological data from the Devonian of North Scotland which they believe invalidate my suggestion of a large-scale sinistral shift along the Great Glen Fault (GGF) in late/post Devonian time[2-4]. Their main arguments are that "the ORS sediments of the Orcadian Basin on both sides of the GGF are very similar in character and show identical history of development" and that palaeocurrent vectors support sedimentation within a single basin. The authors consider the Orcadian Basin to have been of oval shape, the long axis being of the order of 400 km. If a basin of this size can have similar characteristics throughout there is no reason why the outcrops from a basin only about twice as long (corresponding to my proposed 500-km transcurrence) should not exhibit an equally good match of sedimentological and other features. The evolution of the Orcadian Basin may have been strongly linked with lateral and vertical movements of the GGF, causing simultaneous variation in sedimentary facies, tectonic structures and so on over extensive parts of its length. Furthermore, from the palaeocurrent vectors presented (dominantly from west of the fault) one can devise widely different reconstructions within the framework of a single basin: data from the adjoining shelf areas are very necessary before such information becomes relevant to the problem under consideration.

The available data can equally well be fitted to the model of the Orcadian Basin having subsequently become subdivided by a major transcurrent movement. With a 500-km sinistral displacement the East Shetland Basin, which includes thick basal breccias and coarse fluvial conglomerates derived from a metamorphic/plutonic terrain to the west[5], would fit in well with the geology of areas around southern Inverness-shire (west of the GGF).

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2 Storetvedt, K. M., *Geol. Mag.*, 111, 23–30 (1974).
3 Storetvedt, K. M., *Nature*, 249, 777 (1974).
4 Storetvedt, K. M., *Geol. Mag.*, 112, 94–96 (1975).
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Statement

IN September 1976 Dr Robert J. Gullis left our laboratory after having spent two postdoctoral years with us. He had been engaged mainly in measuring the levels of cyclic GMP in neuroblastoma cells and neuroblastoma × glioma hybrid cells. We published four papers on this matter together with him. After Dr Gullis had left, several of my colleagues (M. Brandt, J. Traber and D. van Calker) repeated this work, but were unable to reproduce it. Dr Gullis was therefore asked to return to our laboratory and repeat his essential experiments under supervision. During a 2-week period Dr Gullis carried out four series of experiments. After the experimental incubations, the samples were coded. In none of the experiments was Dr Gullis able to obtain his previous results. Neither morphine nor levorphanol nor the enkephalins nor cholinergic agonists changed the level of cyclic GMP in the hybrid cells.

In some of the publications listed below cyclic AMP was determined in the same samples in which cyclic GMP had been determined. The cyclic AMP assays were carried out by other members of the laboratory. But the printouts from the scintillation counter were left to Dr Gullis for evaluation.

Dr Gullis admitted having invented the results of all his experiments. Thus, I should like to let it be known to the scientific community that the following three publications are based on invented data:

Gullis, R. J., Traber, J. & Hamprecht, B. Morphine elevates levels of cyclic GMP in a neuroblastoma × glioma hybrid cell line. *Nature* 256, 57–59 (1975).
Gullis, R. J., Traber, J., Fischer, K., Buchen, C. & Hamprecht, B. Effects of cholinergic agents and sodium ions on the levels of guanosine and adenosine 3′:5′-cyclic monophosphates in neuroblastoma and neuroblastoma × glioma hybrid cells. *FEBS Lett.* 59, 74–79 (1975).
Gullis, R. J., Buchen, C., Moroder, L., Wünsch, E. & Hamprecht, B. Opiate-like effects of enkephalin on neuroblastoma × glioma hybrids, in *Opiates and endogenous opioid peptides* (ed. Kosterlitz, H. W.) 143–151 (Elsevier, Amsterdam, 1976).

The data on cyclic AMP were falsified by Dr Gullis in a fourth paper (Figs 3 and 4).

Brandt, M., Gullis, R. J., Fischer, K., Buchen, C., Hamprecht, B., Moroder, L. & Wünsch, E. Enkephalin regulates the levels of cyclic nucleotides in neuroblastoma × glioma hybrid cells. *Nature* 262, 311–313 (1976).

B. HAMPRECHT

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DR GULLIS WRITES—I wish to disclose the fact that papers published in several journals with myself as principal author are not reliable. The curves and values published are mere figments of my imagination, and during my short research career I published my hypotheses rather than experimentally determined results. The reason was that I was so convinced of my ideas that I simply put them down on paper; it was not because of the tremendous importance of published papers to the career of a scientist.

Therefore I would like to let it be known that the following papers published while I was working in the laboratory of Dr B. Hamprecht are not reliable.

Gullis, R. J., Traber, J. & Hamprecht, B. *Nature* 256, 57–59 (1975).
Gullis, R. J., Traber, J., Fischer, K., Buchen, C. & Hamprecht, B. *FEBS Lett.* 59, 74–79 (1975).
Gullis, R. J., Buchen, C., Moroder, L., Wünsch, E. & Hamprecht, B. Opiate-like effects of enkephalin on neuroblastoma × glioma hybrids, in *Opiates and endogenous opioid peptides* (ed. Kosterlitz, H. W.) 143–151 (Elsevier, Amsterdam, 1976).

Another paper in which I was co-author and submitted cyclic GMP values is also wrong in terms of the cyclic GMP content (Figs 3 and 4). The paper is

Brandt, M., Gullis, R. J., Fischer, K., Buchen, C., Hamprecht, B., Moroder, L. & Wünsch, E. *Nature* 262, 311–313 (1976).

I would also like to disclose the fact that the following papers published with Dr C. E. Rowe are purely hypothesis.

Gullis, R. J. & Rowe, C. E. *Biochem. Soc. Trans.* 1, 849 (1973); *Biochem. J.* 148, 197–208; 557–565; 567–581 (1975); *J. Neurochem.* 26, 1217–1230 (1976); *FEBS Lett.* 67, 256–259 (1976).

This letter is to point out to the scientific community that the results presented in these papers are wrong and based purely on hypothesis. I must take full responsibility for these unfortunate incidents and have consequently suffered. I hope that my experiences are noted by others, and I would like to apologise to the scientific community and the various people involved.

R. J. GULLIS

NARESA Symposium

on

Standards for Editing and Publishing Scientific Journals

held on 22 October 1983

Basic Research Methodology and Analysis and Interpretation of Data*

by

Winston E. Ratnayake, Professor & Head, Department of Zoology

University of Sri Jayewardenepura,

Nugegoda.

(9.30 a.m. - 10.30 a.m.)

1. Introduction

I have to thank NARESA for inviting me to talk on the above subject at this Symposium. This has given me the motivation and opportunity to read a few books on the subject and to mull over in my own mind my experiences with regard to research over the last twenty-five years. This has helped me to condense the inchoate thoughts I myself have acquired over that period of time.

A discussion of the basic research methodology in Science takes us deep into the roots of epistemology and the philosophy of Science. I shall, therefore, have to delve a bit deep into this aspect in order to explain our present understanding of Science. I shall take a greater portion of the time allotted to me for that purpose and then I shall rapidly deal with the second part of my talk on Analysis and Interpretation of Data.

It is best that I first quote some passages from the writings of a few great scientists who have given deep thought to what Science really means— who with their broader experiences and deeper insights have put down their thoughts so succinctly and clearly.

I shall first quote at some length from John Desmond Bernal, the one - time eminent Professor of Physics from Birkbeck College, University of London, from his book "Science in History" (Watts & Co., London, 1954). The particular section I quote is titled Methods of Science and I think is very appropriate to start my talk. (See Appendix I for first quotation).

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* This paper was prepared from the actual talk and incorporates corrections arising from the discussion that followed.

As you can see, it is a very comprehensive account of what Science is, and I need not, and, really of course, cannot, condense it any further.

The historical development of Scientific thought and Scientific methodology really started with the English Franciscan monk Roger Bacon (? 1214 - 1294) who "postulated a theory of science as a means of discovering reality and truth". He used both geometry and arithmetic in "some simple physics", and for him numbers (mathematics) rather than light were the primal matter. This tradition culminated in another Englishman this time an MP and Chancellor of the Exchequer (Finance Minister) Francis Bacon (1561 - 1626) who in his two Books of Aphorisms said that negative observations should be used to correct superstitious beliefs. These books also outlined the methodology to be used in such a process. The scientific method he advocated was the gathering of facts which he assumed would lead to scientific truths. This is the process of induction - whereby the Particular leads to the General or else, facts (particulars) reveal the abstract truth (or General law). Copernicus (1473 - 1543) would have been greatly influenced by this tradition of Science - the inducto - hypothetico method.

Almost at the same time as Francis Bacon, the French mathematician and philosopher Descartes (1596 - 1650) (pronounced Day - cart) - proposed that Scientific thought advances by the reverse process. That of the General leading to the Particular. This is the process of deduction , and is analogous to the solving of a problem in geometry with the help of a few axioms (or ~~simple~~ truths). Thus, a simple and clear truths by a step by step process leads to complex truths (or explain facts which are actually complex indeed like the problem in geometry). Kepler (1571 - 1630) and Galileo (1564 - 1642) may have been influenced by this tradition - the hypothetico - deductive - method.

Scientists were busy studying nature, making surveys and carrying out experiments and discovering fundamental truths of nature to bother and worry about the actual processes of scientific method and discovery. Newton, Dalton, Darwin, Mendel and whole host of eminent scientists carried out their experiments and made epoch making fundamental discoveries of the hidden Laws of Nature, by following this new research methodology without, perhaps, analysing this process in depth.

It was only in 1934 that once again a serious attempt was made to study the Science of Science. Karl Popper (professor of Logic & Scientific Method in the London School of Economics until 1969) while in Germany wrote his book "Logic der Forschung" (the Logic of Scientific Discovery) in 1934, where he proposed that science progresses by a process of falsification (negation) of hypothesis. Scientific truths are, therefore, only temporary truths, which have to be discarded when negative results are obtained (harking back to Francis Bacon). But Popper, further, showed that the rational and logical part of Science is this critical search for negative results. A Scientific experiment is, therefore, simply a very rigid and thorough critisism of a Scientist's own hypothesis. This analysis also showed that the origin of an hypothesis is a mystical, creative process, like that of mental creativity in any other sphere of mans intellectual activity just like the creation of a work of art, music, literature and even government and politics. There seems to be no logic in the process of induction. Therefore, induction has been left out of Science, and Scientific progress is heavily dependant on the process of falsification of hypotheses through the deductive process. Science advances by the rejection of hypotheses proven (logically) to be wrong. This method combined with the social nature of Science and the newer methods of information transfer has made it progress with remarkable success and rapidity over the last 384 years, particularly the last 100. This is the only conscious method which the mind of man has discovered or invented so far which enables him to obtain knowledge and use it for his advantage. Perhaps mystics arrive at their truths in a similar fashion, but, such ideas are very personal and unconcious in their origin. All what Science has done is to make this personal method of arriving at objective knowledge more public and democratic.

These two aspects of knowledge - the inductive and creative and warm part, common to all aspects of human knowledge and the deductive - falsification part which is cool, logical and rational, that is recognized as the Scientifc method - perhaps from the sum total of scientifc and may be the totality of human mental achievement and endeavour. These two aspects have been very vividly brought out by Professor P.B. Medawar, the Nobel Prize winning biologist in his book "The Art of the Soluble" (Penguin, 1969). (See Appendix II for the second quotation). He more recently brought out a small book - Advice for a young Scientist - which admirably summarizes almost every aspect of the Scientific method.

More recently the American T.S. Kuhn has critisized the extreme point of view taken by Popper. (from I. Kakatos & A. Musgrava - Editors of "Criticism and the Growth of Knowledge" - Cambridge University Press 1970) (See Appendix III for third quotation). While agreeing with Popper on his basic definition of Scientific Logic, Kuhn stresses the fact that it is only when major revolutions in Scientific thought occurs that the falsification concept operates. He refers to such major Scientific concepts as paradigms. The change from the Baconian concept of induction to the Cartesian concept of deduction is a change in paradigms which applies to the whole of Scientific thought. Similarly with regard to the change from a terracentric cosmology (of Ptolemy) to the helio-centric cosmology (of Copernicus Brahe & Kepler); the Darwinian concept of Evolution by Natural Selection, the Mendelian concept of particulate genes for inheritance, were all major changes in paradigms - setting out newer paradigms of thought. However, Kuhn contends that within a single paradigm a major portion of scientific research develops without falsification - actually by adding evidence in support of the paradigm (not to refute it !) He thereby, separates Science into Big and Small. Big (revolutionary) and Small, (conservative) or Super Science and Mediocre Science.

We may, now conclude, therefore, that Science is an Inducto - Hypothetico - Deductive process. A Supa-rational (irrational ?) inductive process produces an hypothesis which is then temporarily accepted or rejected by a logical and rational deductive process. Scientific achievement seems to be built on very impermanent, & shifting quicksand - and not on a solid bedrock infalliable knowledge !

Now, what is this scientific method which converts quicksand to hard rock! This can be summarized in the following flow diagram.

Personal Experiences (experienced & read from books etc.)
↓
Hypothesis
↓
Pilot experiment (or survey)
↓
Design of Experiment (or survey) (Statistical)(The critical method).
↓
Experiments (or survey) conducted (Controls: Positive, Negative etc. and Blind or coded expts.)
↓
Results (Data) obtained (Observations).
↓
Conclusions from Data.
↓
Test agreement or disagreement with Hypothesis (statistical Evaluation)
↓
Temporarily Accept Hypothesis

or

Reject Hypothesis
↓
Repeat Experiment (or Survey)

(repeatability)

Intellectual honesty (personal bias) [bracketing the steps from Design of Experiment to Test agreement]

Science is possible only where events are repeatable. Once and for all phenomena (which, maybe, are possible) are outside the scope of Science. Science is not only the Art of the Soluble - it is also the Art of the Probable). Predictability is the essence of Science.

The concepts of Statistics created by Gas, Pearson, Fisher and others is the yardstick that measures and compares this probability. Probability is a simple concept of the recurrent event. The Sun rises in the east and sets in the west. This has happend as long as human share experienced it. It is said to have a probability of 1. (Astronomers say it will collapse and burn itself out into a cinder a few billion years from now - but from our personal experience it will rise in the east and set in the west tomorrow and the day after). On the other hand the probability that the Sun will rise in the west and set in the east is zero (0). We believe it will never happen. The number of hours the Sun shines on the globular earth is another matter - the duration of Sun - lit hours varies from place to place on the earth and from time to time on the same place on earth. The probability, therefore, varies. In between this 1 and 0 the probability can vary from 0 to 1. It is this probability we have to Statistically measure in our experiments. The probability that our results (practical reason) agree with our hypothesis (pure reason) has to be tested. Pragmatic reason is quite a different type of reason standing in between the other two. This reason is what delesmines what action we have to actually make after considering various other matters.

For this purpose we must know to which category of events (or facts or measurements) our variables fall. They can be normal (gausian), binomial or poisson - These are called populations. Each population has what are called parameters (for the entire population) or Statistics (for Samples drawn from the populations). We always work with Samples (for economy) and can derive only statistics. These statistics have to be converted into parameters to hold true for whole populations and therefore, be universal and true. These are done by various statistical devices - the logic of which is sometimes quasi - scientific and arbitrary. But, there is a universality in their usage and hence comparisons can be effected. These concepts are arbitrary, as languages are to convey meaning and sound, - but being universal (or nearly so) the social awareness of information is assured.

What are these parameters or statistics ? Lets consider a normal population of events (or measurements) which vary from a minimum to a maximum very gradually, like heights or weights of human beings. In a sample there are a number of persons who are measured and the raw - data from a hodge - potch of discrete measurements which appear chaotic. We have to condence that mass of data to just one or two numbers which will adequately describe all the raw-data. This is done with the help of the average (Arithmatic Mean) and the variance. There are other averages (Geometric mean & Harmonic mean). But, we will consider only the arithmatic mean. The simple equation to obtain this is.

$$\bar{X} \text{ (Arithmetic Mean)} = \frac{\sum_{n}^{i=0} X_n}{n}$$

But this number is not enough, by itself, to describe the entire mass of data. It must also have another number to denote the variability (and range) of the individual values from this Mean. This statistic (or parameter) is called the Variance and is obtained from the following equation.

$$S^2 \text{ (variance)} = \frac{\sum_{n}^{i=0} (X-\bar{X})}{n-1}$$

These two numbers describe perfectly all the data in that sample. The sample mean is a good approximate of Population Mean while to get the Population variance the $\sum (X-\bar{X})$ has to be divided by n-1 instead of n, n-1 is called the degrees of freedom.

Similarly the standard deviation is the square root of variance which converts the square units into the same units as that of the mean and the individual values. There are other statistical devices to help in condensing a mass of data into just a few numbers to help in comparisons between samples (or populations).

Similarly when two variables like height & weight of individuals are considered both together - the relationship between them can be measured by the Coefficient of Correlation or Regression Coefficient respectively.

$$r \text{ (Coefficient of correlation)} = \frac{\sum (X-\bar{X})\ (Y-\bar{Y})}{\sqrt{\sum (X-\bar{X})^2\ (Y-\bar{Y})^2}}$$

$$b \text{ (regression Coefficient)} = \frac{\sum (X-\bar{X})\ (Y-\bar{Y})}{\sum (X-\bar{X})^2}$$

The whole mass of data, therefore can be condensed into just two numbers which perfectly describe the population.

Once these parameters (or statistics) are obtained then tests of Significance can be carried out to see whether the normal population (Your control) differs from or agrees with any treatment (perturbation) you have made in another sample drawn from the same population. For this purpose a whole battery of Statistical tests have been devided - called "t" test, x^2 - test, Analysis of Variance etc.

My purpose is to make you aware (as young research scientists) what statistics really means and not to give complete lectures on statistics. You have to follow a complete course in statistics to obtain that knowledge. Also, you have to consult trained statisticians to get your experiments designed as well as to get your results analysed. But, beware ! Statistics can lie (it can be a damn liar - not the statistician !) Data have to be obtained by you with great care and honesty. Then only is statistics used on that data. So you have to discuss your problems thoroughly with statisticians so that they can derive the proper designs and tests for your experiments.

I have to discuss a few more problems with regard to Science before I wind up.

One, is with regard to the selection of a research topic. In this connection I shall quote from Herman Bondi (1983) (See Apendix IV - for 4th quotation). You see, therefore, that there are scientific problems all round you - but finding the correct soluble problem is very difficult and you must have the capability to select such a problem. There is no logic in this selection - only experience and awareness, and an alert and agile mind can detect such soluable problems. Even the so called accidental or chancy discoveries are possible because of a well prepared mind.

Then also with reference to Scientific research - there is hard science and soft science. Soft science does not necessarily mean that it is easy science.

What it really means is that hard Science is more logical, mathematical, experimental and predictable, hence, really scientific. Soft science is more empirical, observational and less predictable. Therefore, of the two, it is soft science which is more difficult, for generalizations have to be made on mere surveys and observations and not on critical and definitive experiments. This is because the hard sciences are about nature outside of man, and is based more on facts and not so much on values. Judgement of facts is more easy than that of values. It is the human element with the ability to remember and think that complicates the soft sciences. Even within the hard sciences, Biology & Chemistry are not so hard as Physics. The motion of Individual molecules of a gas cannot be predicted - but when contained in a vessel the physical laws pertaining to its total behaviour with regard to pressure and temperature can be very rigidly predicted. It is not so easy with chemical reactions (of large molecules) or of even larger animals & plants - and becomes almost impossible with units like animals, plants or human beings. We cannot put human beings in containers and experiment with them (although such bestial attempts were made at Aushwitch and Belsen concentration camps by the Nazis during the last World War !

The other matter to be discussed is with regard to pure (curiosity - oriented) and applied (mission - oriented) research. These have been adequately covered by the quotations I have made. But, I have to mention that these two are entirely different activities - one, is purely about ones innate curiosity about Nature and are the attempts to understand it without any particular use being thought of before the study, while the other is directly tied to a specific practical problem at hand or in mind. It is this latter method which converts science into technology. But technology itself is not just applied research. Technology is most often a craft practised by man without research but based on the knowledge already available. Technology most often arises spontaneously due to the desired requirements of society and is as mystical like creativity itself. Once a technology is made it is further improved with scientific know - how, which part is then dependent on applied research. Agriculture, bread making, brewing of liquor, and navigation, arose as pure technologies without much Scientific understanding in ancient times - but has considerably improved with time with the application of Science. The knowledge of a particular period of time produces a technology. For instance, radio technology, aerial navigation, computers and genetic engineering, certainly started with the base of modern scientific knowledge. It is ideas that are central to any activity or technology, and Sri Lanka has to indulge in pure science to give the scientific community confidence and spirit in their scientific endeavours. As the Buddha said, "Chetana ahambikkhave kammam vadami".

This question leads to the social relevance of science. All science is Social; Social in principle and in activity. Applied Science is directly socially relevant than pure science - but the major technologies arise unpredictably from pure science and not from applied science. Hence, Pure Science is supreme. Here we are reminded of the Little Science - Big Science of Kuhn and Price - Solla. Little Science must also contain the whole of the applied sciences while Big Science can be equalled only with Pure Science producing changes in paradigms on the grand scale as expounded by Popper and Kuhn.

Science is a social activity - made by individual scientists to be communicated to others to accept or refute. It is a free and very democratic process (except for a few charlatans and quacks who use it for their own persomal benefit !)

Reference :

1. Aligarh Muslim University, Science : Its Method and Outlook., Asia Publishing House., 1963.

2. Harper, W.M., Statistics, Macdonald & Evans Ltd., 1971.

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Appendix I :-

"The institution of science is a social fact, a body of people bound together by certain organizing relations to carry out certain tasks in society. The method of science is by contrast an abstraction from these facts. There is a a danger of considering it as a kind of ideal Platonic form, as if there were one proper way of finding the Truth about Nature or Man, and the scientists' only task was to find this way and abide in it, Such an absolute conception is believed by the whole history of science, with its continual development of a multiplicity of new methods. The method of science is not a fixed thing, it is a growing process. Nor can it be considered without bringing out its closer relations with the social and particularly the class, character of science. Consequently, scientific method, like science itself, defies definition. It is made up of a number of operations, some mental, some manual, that in the past have been found to lead to the formulation, finding, testing, and using of the answers to the general questions that are worth asking and can be answered at any stage of social development. In the distant past the questions that could usefully be answered were mostly in the field of the mathematical sciences, such as astronomy and physics. In all other fields there were only particular results found by experience and guaranteed by technical usefulness. Later, the scientific method came to be applied and modified in the fields of chemistry and biology, and now, in our owntime, we are just beginning to learn how to apply it to problems of society.

Now the study of the method of science has proceeded much more slowly than the development of science itself. Scientists find out things first, and then, rather ineffectively, muse on the way in which they were discovered. Unfortunately most of the books written about the methods of science have been by people who, though philosophically or even mathematically gifted, are not experimental scientists and strictly speaking do not know what they are talking about.

Observation and Experiment

The methods used by working scientists have evolved from a separation of methods used in ordinary life, particularly in the manual trades. First you have a look at the job and then you try something and see if it will work. In more learned language, we begin with observations and follow with experiments. Now everyone, whether he is a scientist or not, observes; but the important things are what to observe and how to observe them. It is in this sense that the scientist differs from the artist. The artist observes in order to transform, through his own experience and feeling, what he sees into some new and evocative creation. The scientist observes in order to find things and relations that are, as far as possible independent of his own sentiments. This does not mean that he should have no conscious aim. Far from it; as the history of science shows, some objective, often a practical one, is most an essential requirement for the discovery of new things. What it does mean is that in order to achieve its goal in the inhuman world, deaf to the most emotional appeal, desire must be subordinated to fact and law.

Classification and Measurement

Two techniques have in time grown out of naive observation: classification and measurement. Both are, of course, much older than conscious science, but they are now used in quite a special way. Classification has become in itself the first step towards understanding new groups of phenomena. They have to be put in order before anything can be done with them. Measurement is only the further stage of that putting in order. Counting is the ordering of one collection against another in the last resort against the fingers. Measuring is counting the number of standard collection that balance or line up with the quantity that is to be weighed or measured. It is measurement that link science with mathematics on the other. It is by measurement that numbers and forms enter science, and it is also by measurement that it is possible to indicate precisely what has to be done to reproduce given conditions and obtain a desired result.

It is here that the active aspect of science comes into the picture - that characterized by the word "experiment". An experiment, after all, as the word indicates, is only a trial, and early experiments indeed were full-scale trials. Once measurement was introduced it was possible not only to reproduce trials accurately, but also to take the somewhat daring step of carrying them out on a small scale. It is that small-scale or model experiment that is the essential feature of modern science. By working on a small scale far more trials can be carried out at the same time and far more cheaply. Moreover, by the use of mathematics, far more valuable results can be obtained from the many small-scale experiments than from one or two elaborate and costly full-scale trials. All experiments boil down to two very simple operations: taking apart and putting together again; or, in scientific language, analysis and synthesis. Unless you can take a thing or a process to bits you can do nothing with it but observe it as an undivided whole. Unless you can put the pieces together again and make the whole thing work, there is no way of knowing whether you have introduced something new or left something out in your analysis.

Apparatus

In order to carry out these operations, scientisits have, over the course of centuries, evolved a complete set of material tools of their own-the apparatus of science. Now apparatus is not anything mysterious. It is simply the tools of ordinary life turned to very special purposes . The crucible is just a pot, the forceps a pair of tongs. In turn, the apparatus of the scientist often comes back into practical life in the form of useful instruments or implements. It is not very long, for instance, since the modern television set was the cathode-ray tube, a purely scientifc piece of apparatus devised to measure the mass of the electron. Scientifc apparatus fulfils either of two major functions : as scientific instruments, such as telescopes or microphones, it can be used to extend and make more precise our sensory perception of the world; as scientific tools, such as micro-manipulators, stills, or incubators, it can be used to extend, in a controlled way, our motor manipulation of the things around us.

Laws, Hypotheses, and Theories

From the results of experiments, or rather from the mixture of operation and observation that constitutes experiments, comes the whole body of scientific knowledge. But that body is not simply a list of such results. If it were, science would soon become as unwieldy and difficult to understand as the Nature from which it started. Before these results can be of any use, and in many cases before they can even be obtained, it is necessary to tie them together, so to speak, in bundles, to group them and to relate them to each other, and this is the function of the logical part of science. The arguments of science, the use of mathematical symbols and formulae, in earlier stages merely the use of names, lead to the continuous creation of the more or less coherent edifice of scientific laws, principles, hypotheses and theories, And that is not the end; it is here that science is continually beginning, for, arising from such hypotheses and theories, there come the practical applications of science. These is turn, if they work, and even more often if they do not, give rise to new observations, new experiments, and new theories. Experiment, interpretation, application, all march on together and between them make up the effective, live, and social body of science.

The Language of Science

In the process of observation, experiment, and logical interpretation, there has grown up the language, or rather, the languages, of science that have become in the course of time as essential to it as the material apparatus. Like the apparatus, these languages are not intrinsically strange, they derive from common usage and often come back to it again. A cycle was once kuklos, a wheel, but it lived many centuries as an abstract term for recurring phenomena before it came back to earth as a bicycle. The enormous convenience of making use of quite ordinary words in the forgotten languages of Greece and Rome was to avoid confusion with common meanings. The Greek Scientists were under the great disadvantage of not having a word - in Greek - for it. They had to express themselves in a roundabout way in plain language - to talk about the submaxillary gland as "the acorn-like lumps under the jaw". But these practices, though they helped the scientists to discuss more clearly and briefly, had the disadvantage of building up a series of special languages or jargons that effectively, and sometimes deliberately, kept science away from the ordinary man. This barrier, however, is by no means necessary. Scientific language is too useful to unlearn, but it can and will infiltrate into common speech once scientific ideas become as familiar adjuncts of everyday life as scientific gadgets.

The Strategy of Science

This discussion of the method of science has been limited so far to what might be called the tactics of scientific advance. This is primarily a method of solving problems and being reasonably sure that the solutions are satisfactory. It is clearly insufficient by itself to explain the advance of science as a whole over long periods of time. To complete the picture it is necessary to say something of what corresponds to the strategy of science. Now, of course, there is no absolute need for science to have a conscious strategy in order to advance, and indeed in earlier times it certainly was not directed with any long-term ends in view. Nevertheless, as we shall see, the path of advance of science was by no means a random one, and all the time something like a strategy must have been operating, unconsciously for the most part, but sometimes consciously as well.

The essential feature of a strategy of discovery lies in determining the sequence of choice of problems to solve. Now it is in fact very much difficult to see a problem than to find a solution for it. The former requires imagination, the latter only ingenuity. This is the sense of Kosambi's definition of science as the cognition of necessity. The general advance of science has, in fact, taken place in following out the solutions of problems set in the first place by actual economic necessity, and only in the second place arising out of earlier scientific ideas. At any given time there are usually a set of challenging problems like the doubling in bulk of the cubic alter at Delphi, which involved extracting a cube root, or the finding of the longitude, which led to Newton's laws, or the curing of the silkworm disease in France, which helped Pasteur to arrive at the idea of the germ theory of disease. The danger in science is that the number of such recognized classical problems tends to be limited. The efforts of scientists, generation after generation, are concentrated on solving them and on elaborating on the solutions.

It is this tendency that has kept science for long periods of its history within narrow bounds. It is by breaking with it and finding new problems in outside life that it expands into new fields. Some of the greatest scientists of the past, like Newton, Darwin, and Faraday, set themselves to find and solve problems according to a plan of their own, Faraday, for instance, early in his career set himself the general problem of finding the connections between the separate forces of physical Nature-light, heat, electricity, and magnetism and taking them pair by pair, nearly completed the programme.

Now we are beginning to see that what could be done consciously, though on a small scale, by such great individuals is an essential part of the growth of science, and we are finding it possible to plan science consciously on a collective rather than a purely individual basis. Here the wider problem comes from the need to reconcile and combine the questions arising from the social and economic requirements on the one hand, and the intrinsic developments of science on the other. This, however, involves, for its full advantages to be discovered and used, a far greater control over the economic life of the country than is to be found outside Socialist countries. These advantages are, nevertheless, so great in the long run that no nation will be able to hold its own in the world without making positive and planned use of science. Consequently, the advance of science and its increasing utilization in social life are likely in the future to take a far more rational and less accidental course than in the past.

Viewed in the perspective of evolutionary history, science marks a conscious elaboration of the experience provided by the sensory and motor organs of the body. It extends consciously and socially, the unconscious processes of learning, common to all higher animals. An animal can learn by experience; man in using science goes beyond this and experiences to learn. In the same sense the scientific method itself, with its codified processes of comparison, classification, generalization, hypothesis, and theory, is an extension of the mechanism of the brain, which had already evolved in the higher mammals the capacity of dealing with highly complex situations, such as those involved in hunting. The essential difference, however, between these animal performances and the achievements of human science is that the latter is no longer an individual but a social achievement. It arises from the co-operative effort of work and is co-ordinated by language.

Science and Art

The extension of the physical powers of man through science is no longer, as in animals, a continuous, almost automatic, evolutionary process. It comes about as a necessary correlative of social changes and is marked by the same internal struggles and conficts of successively emerging classes. Bearing always in mind the inseparability of science from society, it may yet be useful to abstract still further and to consider the features of science which distinguish it from other aspects of human social activity such as those of art or religion. The major grounds for the distinction of the scientific aspect are that it is concerned primarily with how to do things; that it refers to a cumulative mass of knowledge of fact and action; and that it arises first and foremost in the understanding, control, and transformation of the means of production-that is of techniques for providing human needs.

The first of these distinctions can be expressed by saying that the mode of science is indicative, in that it can indicate or show people how to do what they want to do. In itself the scientific mode does not attempt to make people want to do one thing rather than another. That is more properly the task of the artistic mode, a mode equally social, one of whose functions it is to generate first the wish and then the will for specific action. Neither of these modes is complete without the orther and, in fact, neither in science nor in art is one to be found without the other. Nor between them do they exhaust the significance of art of science for the individual. Beyond them, and common to all forms of human achievement, is intrinsic pleasure produced in the contemplation, or still more in the creation, of new combinations of words, sounds, or colours, or in the discovery of combinations already existing in Nature. This pleasure, though felt individually in the first place, is by no means a private emotion. As the first interest derives from society, so the contemplative act is social at one remove, as is shown by the intense desire, common to artist and scientist, to communicate it.

Every work of science has a purpose and generates a further purpose, but that purpose is the characteristically scientific aspect of the work, neither is it the beauty or pleasure to be appreciated in the work of science. In its purely scientific aspect, it is recipet: it tells you how to carry out certain things if you want to do them. Nor, on the other hand, is a work of art something that merely moves or pleases. Works of art themselves contain invaluable information about the world and how to live in it, especially when, as in the novel, they deal with social problems.

In stating these abstract characteristics of science, there is always the danger that the abstract may be taken as the ideal, that is, what science should be if only all the unessential aspects of social morality or usefulness could be removed. Indeed, the ideal of pure science- the pursuit of Truth for its own sake is the conscious statement of a social attitude which has done much to hinder the development of science and has helped to put it into obscurantist and reactionary hands. It should always be remembered that science is complete only if the indications are followed. Science is not a matter of thought alone, but of thought continually carried into practice and continually refreshed by practice. That is why science cannot be studied separately from technique. In the history of science we shall repeatedly see new aspects of science arising out of practice and new developments in science giving rise to new branches of practice. The professions of the modern engineer are very largely directly due to scientific progress. The very names of the different kinds of engineers there are today, electrical engineers, chemical engineers, radio engineers, indicate that all were originally branches of science that have now become branches of practice.

Scientist and Engineer

But the fact that the engineers have arisen from the scientists, and are continually and closly linked with them, does not mean that the two professions are indistinguishable. In fact, the functional aspects of the scientist and the engineer are radically different. The scientist's prime business is to find out how to do things, the engineer's business is to do them. The responsibility of the engineer is much greater, in the practical sense, than that of the scientist. He cannot afford to rely so much on abstract theory; he must build on the traditions of past experience as well as try out new ideas. In certain fields of engineering, indeed, science still plays a subsidiary role to experience. Ships today, although full of modern scientific devices in their engines and controls, are still built by men who have based their experience on those of older ships, so that one may say that the building of ships, from the first dug-out canoe to the modern liner, has been one unbroken technical tradition. The strength of technical tradition is that it can never go far wrong if worked before, it is likely to work again; its weakness is that it cannot, so to speak, get off its own tracks. Steady and cumulative improvement of technique can be expected from engineering; but notable transformations, only when science takes a hand. As J.J. Thomson once said "Research in applied science leads to reforms, research in pure science leads to revolution". At the same time engineering successes, and even more engineering difficulties, furnish a continually renewed field of opportunity and problems for science. The complementary roles of science and engineering mean that both need to be studied to understand the full social effects of either.

Appendix II :-

P.B. Medawar (form the Art of the Soluble, Penguin, 1969)

"According to the first conception, Science is above all else an imaginative and exploratory activity, and the scientist is a man taking part in a great intellectual adventure. Intuition is the mainspring of every advancement of learning, and having ideas is the scientist's highest accomplishment; the working out of ideas is an important and exacting but yet a lesser occupation. Pure science requires no justification outside itself, and its usefulness has no bearing on its valuation. 'The first man of science', said Coleridge, 'was he who looked into a thing, not to learn whether it could furnish him with food, or shelter, or weapons, or tools, or ornaments, or play-withs, but who sought to know it for the gratification of knowing"..

"The alternative conception runs something like this: Science is above all else a critical and analytical activity, the scientist is pre-eminently a man who requires evidence before he delivers an opinion, and when it comes to evidence he is hard to please. Imagination is a catalyst merely: it can speed thought but cannot start it or give it direction; and imagination must at all times be under the censorship of a dispassionate and sceptical habit of thought. Science and poetry are antithetical, as Shelley so rightly said. Scientific research is intended to enlarge human understanding, and its usefulness is the only objective measure of the degree to which it does so; as to freedom in science, two world wars have shown us how very well science can flourish under the pressures of necessity. Patrons of science who really know their business will support projects, not people, and most of these projects will be carried out by teams rather than by individuals, because modern science calls for a consortium of the talents and the day of the individual is almost done. If any scientist should spend five years getting nowhere, his ambitions should be turned in some other direction without delay".

"There is no paradox here: it just so happens that what are usually thought of as two alternative and indeed competing accounts of one process of thought are in fact accounts of the two successive and complementary episodes of thought that occur in every advance of scientific understanding. Unfortunately, we in England have been brought up to believe that scientific discovery turns upon the use of a method analogous to, and of the same logical stature as deduction, namely the method of Induction - a logically mechanised process of thought which, starting from simple declarations of fact arising out of the evidence of the senses, can lead us with certainty to the truth of general laws. This would be an intellectually disabling belief if anyone actually believed it, and it is one for which John Stuart Mill's methodology of science must take most of the blame. The chief weakness of Millian induction was its failure to distinguish between the acts of mind involved in discovery and in proof. It was an understandable mistake, because in the process of deduction, the paradigm of all exact and conclusive reasoning, discovery and proof may depend on the same act of mind : starting from true premises. We can derive and so 'discover' a theorem by reasoning which (if it has been carried out according to the rules) itself shows that the theorem must be true. Mill thought that his process of 'induction' could fulfil the same two functions; but, alas, mistakenly, for it is not the origin but only the acceptance of hypotheses that depends upon the authority of logic".

Appendix III :-

T.S. Kuhn (from I. Lakatos and A. Musgrave - eds - Criticism and the Growth of Knowledge - Cambridge University Press 1970)

"Among the most fundamental issues on which Sir Karl (Popper) and I agree is our insistence that an analysis of the development of scientific knowledge must take account of the way science has actually been practised. That being so, a few of his recurrent generalisations startle me. One of these provides the opening sentences of the first chapter of the Logic of Scientific Discovery: 'A scientist', writes Sir Karl, 'whether theorist or experimenter puts forward statements, or systems of statements and tests them step by step. In the field of the empirical sciences, more particularly he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment'. The statement is virtually a cliche, yet in application it presents three problems. It is ambiguous in its failure to specify which of two sorts of 'statements' or 'theories' are being tested. That ambiguity can, it is true, be eliminated by reference to other passages in Sir Karl's writings, but the generalisation that results is historically mistaken. Furthermore, the mistake proves important, for the unambiguous form of the description misses just that characteristic of scientific practice which most nearly distinguishes the sciences from other creative pursuits'.

There is one sort of 'statement' or 'hypothesis' that scientists do repeatedly subject to systematic test. I have in mind statements of an individual's best guesses about the proper way to cannect his own research problem with the corpus of accepted scientific knowledge. He may, for example, conjecture that a given chemical unknown contains the salt of a rare earth, that the obesity of his experimental rats is due to a specified component in their diet, or that a newly discovered spectral pattern is to be understood as an effect of nuclear spin. In each case, the next steps in his research are intended to try out or test the conjecture or hypothesis. If it passes enough or stringent enough tests, the scientist has made a discovery or has at least resolved the puzzle he had been set. If not, he must either abandon the puzzle entirely or attempt to solve it with the aid of some other hypothesis. Many research problems, though by no means all, take this form. Tests of this sort are a standard component of what I have elsewhere labelled 'normal science' or 'normal research', an enterprise which accounts for the overwhelming majority of the work done in basic science. In no usual sense, however, are such tests directed to current theory. On the contrary, when engaged with a normal research problem, the scientist must premise current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that, given sufficient brilliance, it can be solved. Of course the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. If it fails the test, only his own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than current theory which is tested".

I suggest then that Sir Karl has characterised the entire scientific enterprise in terms that apply only to its occasional revolutionary parts. His emphasis is natural and common: the exploits of a Copernicus or Einstein make better reading than those of a Brahe or Lorentz; Sir Karl would not be the first if he mistook what I call normal science for an intrinsically uninteresting enterprise. Nevertheless, neither science nor the development of knowledge is likely to be understood if research is viewed exclusively through the revolutions it occasionally produces. For example, though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing. Or again, it is for the normal, not the extraordinary practice of science that professionals are trained; if they are nevertheless eminently successful in displacing and replacing the theories on which normal practice depends, that is an oddity which must be explained. Finally, and this is for now my main point, a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extra ordinary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores".

Appendix IV

Conclusions .

Finding research problem - Professor Sir Herman Bond - The Making of a Scientist, Royal Society of Arts Journal (1983) Vol: 5323 & pp. 405 - 406)

Science is, above all, concerned with research. To do something new, to find out something that nobody knew before, is its prime aim. It is like a cart that can be rocked backwards and forwards and thus sometimes makes a little progress in its bed of mud. It is this progress that I see both as its ultimate attraction and the ultimate test in the making of a scientist. It is when you have done something new, however minor it may be, that you really join the ranks of the profession. I am not sure that we always make it appear sufficiently exciting for young people when they first achieve something that has not been done before. Americans often say that there are no frontiers. You can always do something new and to have done so should be a tremendous moment. Of course, as I have indicated, reaching this stage is not easy, particularly if you try to do it within the three-year confines of a research studentship. The very essence of science is that one does not just look at the interesting problems, but at those where progress appears to be just possible. Peter Medawar once called science the art of the soluble, and I have the greatest respect for that terminology. I often say that if you walk along the street you will encounter a number of scientific problems. Of those, about 80 per cent are insoluble, while 19½ per cent are trivial. There is then perhaps only half a per cent where skill, persistence, courage, creativity and originality can make a difference. It is always the task of the academic to swim in that half a per cent, asking the questions through which some progress can be made. This is where greatness in science shows itself. It is no use thinking great thoughts about great problems if you make no impression on them. Your choice should always be for an area where you can make an impact. This is perhaps the most crucial feature in making a scientist: that his supervisor, professor, teacher chooses something soluble for him, something that can be done in a very limited timespan by somebody who is new to the topic. This is not an easy task for the teacher".

NARESA - Seminar on the Presentation of Scientific Papers

22nd October 1983.

Basic Research Methodology, and Analysis and Interpretation of data.

by

Winston E. Ratnayake, Professor and Head, Department of Zoology,
University of Sri Jayewardenepura, Nugegoda.

(9.30 am. - 10.30 am.)

1. Introduction

I have to thank NARESA for inviting me to give a talk on the above subject at this Seminar. This has given me the opportunity to read a few books on the subject and to mull over in my own mind my experiences with regard to research and thereby concretize the inchoate feelings and thoughts I myself have on the Science of Science, that is, the philosophy and politics of science.

It is best that I first quote some passages from a few great scientists - who have with their greater experiences and deeper insights put down their thoughts so succinctly and clearly. I would not be able to better them in any way at the moment.

1.1 I will first quote at some length from John Desmond Bernal, the eminent Professor of Physics from Birkbeck College, University of London from his book Science in History (Watts and Co. London, 1954). This section is titled the Methods of Science and I think is very appropriate to start my talk with.

"The institution of science is a social fact, a body of people bound together by certain organizing relations to carry out certain tasks in society. The method of science is by contrast an abstraction from these facts. There is a danger of considering it as a kind of ideal Platonic form, as if there were one proper way of finding the Truth about Nature or Man, and the scientists' only task was to find this way and abide in it. Such absolute conception is believed by the whole history of science, with its continual development of a multiplicity of new methods. The method of science is not a fixed thing, it is a growing process. Nor can it be considered without bringing out its closer relations with the social and particularly the class, character of science. Consequently, scientific method, like science itself, defies definition. It is made up of a number of operations, some mental, some manual, that in the past have been found to lead to the formulation, finding, testing, and using of the answers to the general questions that are worth asking and can be answered at any stage of social development. In the distant past the questions that could usefully be answered were mostly in the fields of the mathematical sciences, such as astronomy and physics. In all other fields there were only particular results found by experience and guaranteed by technical usefulness. Later, the scientific method came to be applied and modified in the fields of chemistry and biology, and now, in our owntime, we are just beginning to learn how to apply it to problems of society.

Now the study of the method of science has proceeded much more slowly than the development of science itself. Scientists find out things first, and then, rather ineffectively, muse on the way in which they were discovered. Unfortunately, most of the books written about the methods of science have been by people who, though philosophically or even mathematically gifted, are not experimental scientists and strictly speaking do not know what they are talking about.

Observation and Experiment.

The methods used by working scientists have evolved from a separation of methods used in ordinary life, particularly in the manual trades. First you have a look at the job and then you try something and see if it will work. In more learned language, we begin with observations and follow with experiments. Now everyone, whether he is a scientist or not, observes; but the important things are what to observe and how to observe them. It is in this sense that the scientist differs from the artist. The artist observes in order to transform, through his own experience and feeling, what he sees into some new and evocative creation. The scientist observes in order to find things and relations that are as far as possible independent of his own sentiments. This does not mean that he should have no conscious aim. Far from it; as the history of science shows, some objective, often a practical one, is almost an essential requirement for the discovery of new things. What it does mean is that in order to achieve its goal in the inhuman world, deaf to the most emotional appeal, desire must be subordinated to fact and law.

Classification and Measurement

Two techniques have in time grown out of naive observation: classification and measurement. Both are, of course, much older than conscious science, but they are now used in quite a special way. Classification has become in itself the first step towards understanding new groups of phenomena. They have to be put in order before anything can be done with them. Measurement is only the further stage of that putting in order. Counting is the ordering of one collection against another in the last resort against the fingers. Measuring is counting the number of standard collection that balance or line up with the quantity that is to be weighed or measured. It is measurement that links science with mathematics on the other. It is by measurement that numbers and forms enter science, and it is also by measurement that it is possible to indicate precisely what has to be done to reproduce given conditions and obtain a desired result.

It is here that the active aspect of science comes into the picture - that characterized by the word "experiment". An experiment, after all, as the word indicates, is only a trial, and early experiments indeed were full-scale trials. Once measurement was introduced it was possible not only to reproduce trials accurately, but also to take the somewhat daring step of carrying them out on a small scale. It is that small-scale or model experiment that is the essential feature of modern science. By working on a small scale far more trials can be carried out at the same time and far more cheaply. Moreover, by the use of mathematics, far more valuable results can be obtained from the many small-scale experiments than from one or two elaborate and costly full-scale trials. All experiments boil down to two very simple operations: taking apart and putting together again; or, in scientificc language, analysis and synthesis. Unless you can take a thing or a process to bits you can do nothing with it but observe it as an undivided whole. Unless you can put the pieces together again and make the whole thing work, there is no way of knowing whether you have introduced something new or left something out in your analysis.

Apparatus

In order to carry out these operations, scientists have, over the course of centuries, evolved a complete set of material tools of their own—the apparatus of science. Now apparatus is not anything mysterious. It is simply the tools of ordinary life turned to very special purposes. The crucible is just a pot, the forceps a pair of tongs. In turn, the apparatus of the scientist often comes back into practical life in the form of useful instruments or implements. It is not very long, for instance, since the modern television set was the cathode-ray tube, a purely scientific piece of apparatus devised to measure the mass of the electron. Scientific apparatus fulfils either of two major functions: as scientific instruments, such as telescopes or microphones, it can be used to extend and make more precise our sensory perception of the world; as scientific tools, such as micro-manipulators, stills, or incubators, it can be used to extend, in a controlled way, our motor manipulation of the things around us.

Laws, Hypotheses, and Theories

From the results of experiments, or rather from the mixture of operation and observation that constitutes experiments, comes the whole body of scientific knowledge. But that body is not simply a list of such results. If it were, science would soon become as unwieldy and difficult to understand as the Nature from which it started. Before these results can be of any use, and in many cases before they can even be obtained, it is necessary to tie them together, so to speak, in bundles, to group them and to relate them to each other, and this is the function of the logical part of science. The arguments of science, the use of mathematical symbols and formulae, in earlier stages merely the use of names, lead to the continuous creation of the more or less coherent edifice of scientific laws, principles, hypotheses, and theories. And that is not the end; it is here that science is continually beginning, for, arising from such hypotheses and theories, there come the practical applications of science. These is turn, if they work, and even more often if they do not, give rise to new observations, new experiments, and new theories. Experiment, interpretation, application, all march on together and between them make up the effective, live, and social body of science.

The Language of Science

In the process of observation, experiment, and logical interpretation, there has grown up the language, or, rather, the languages, of science that have become in the course of time as essential to it as the material apparatus. Like the apparatus, these languages are not intrinsically strange, they derive from common usage and often come back to it again. A cycle was once kuklos, a wheel, but it lived many centuries as an abstract term for recurring phenomena before it came back to earth as a bicycle. The enormous convenience of making use of quite ordinary words in the forgotten languages of Greece and Rome was to avoid confusion with common meanings. The Greek scientists were under the great disadvantage of not having a word - in Greek - for it. They had to express themselves in a roundabout way in plain language - to talk about the submaxillary gland as "the acorn-like lumps under the jaw". But these practices, though they helped the scientists to discuss more clearly and briefly, had the disadvantage of building up a series of special languages or jargons that effectively, and sometimes deliberately, kept science away from the ordinary man. This barrier, however, is by no means necessary. Scientific language is too useful to unlearn, but it can and will infiltrate into common speech once scientific ideas become as familiar adjuncts of everyday life as scientific gadgets.

The Strategy of Science

This discussion of the method of science has been limited so far to what might be called the tactics of scientific advance. This is primarily a method of solving problems and being reasonably s sure that the solutions are satisfactory. It is clearly insufficient by itself to explain the advance of science as a whole over long periods of time. To complete the picture it is necessary to say something of what corresponds to the strategy of science. Now, of course, there is no absolute need for science to have a conscious strategy in order to advance, and indeed in earlier times it certainly was not directed with any long-term ends in view. Nevertheless, as we shall see, the path of advance of science was by no means a random one, and all the time something like a strategy must have been operating, unconsciously for the most part, but sometimes consciously as well.

The essential feature of a strategy of discovery lies in determining the sequence of choice of problems to solve. Now it is in fact very much more difficult to see a problem than to find a solution for it. The former requires imagination, the latter only ingenuity. This is the sense of Kosambi's definition of science as the cognition of necessity. The general advance of science has, in fact, taken place in following out the solutions of problems set in the first place by actual economic necessity, and only in the secomd place arising out of earlier scientific ideas. At any given time there are usually a set of challenging problems like the doubling in bulk of the cubic alter at Delphi, which involved extracting a cube root, or the finding of the longitude, which led to Newton's laws, or the curing of the silkworm disease in France, which helped Pasteur to arrive at the idea of the germ theory of disease. The danger in science is that the number of such recognized classical problems tends to be limited. The efforts of scientists, generation after generation, are concentrated on solving them and on elaborating on the solutions.

It is this tendency that has kept science for long periods of its history within narrow bounds. It is by breaking with it and finding new problems in outside life that it expands into new fields. Some of the greatest scientists of the past, like Newton, Darwin, and Faraday, set themselves to find and solve problems according to a plan of their own. Faraday, for instance, early in his career set himself the general problem of finding the connections between the separate forces of physical Nature—light, heat, electricety, and magnetism—and taking them p pair by pair, nearly completed the programme.

Now we are beginning to see that what could be done consciously, though on a small scale, by such great individuals is an essential part of the growth of science, and we are finding it possible to plan science consciously on a collective rather than a purely individual basis. Here the wider problem comes from the need to reconcile and combine the questions arising from the social and economic requirments on the one hand, and the intrinsic developments of science on the other. This, however, involves, for its full advantages to be discovered and used, a far greater control over the economic life of the country than is to be found outside Socialist countries. These advantages are, nevertheless, so great in the long run that no nation will be able to hold its own in the world without making positive and planned use of science. Consequently, the advance of science and its increasing utilization in social life are likely in the future to take a far more rational and less accidental course than in the past.

Viewed in the perspective of evolutionary history, science marks a conscious elaboration of the experience provided by the sensory and motor organs of the body. It extends consciously and socially, the unconscious processes of learning, common to all higher animals. An animal can learn by experience; man in using science goes beyond this and experiences to learn. In the same sense the scientific method itself, with its codified processes of comparison, classification, generalization, hypothesis, and theory, is an extension of the mechanism of the brain, which had already evolved in the higher mammals the capacity of dealing with highly complex situations, such as those involved in hunting. The essential difference, however, between these animal performances and the achievements of human science is that the latter is no longer an individual but a social achievement. It arises from the co-operative effort of work and is co-ordinated by language.

Science and Art

The extension of the physical powers of man through science is no longer, as in animals, a continuous, almost automatic, evolutionary process. It comes about as a necessary correlative of social changes and is marked by the same internal struggles and conficts of successively emerging classes. Bearing always in mind the inseparability of science from society, it may yet be useful to abstract still further and to consider the features of science which distinguish it from other aspects of human social activity, such as those of art or religion. The major grounds for the distinction of the scientific aspect are that it is concerned primarily with how to do things; that it refers to a cumulative mass of knowledge of fact and action; and that it arises first and foremost in the understanding, control, and transformation of the means of production—that is of techniques for providing human needs.

The first of these distinctions can be expressed by saying that the mode of science is indicative, in that it can indicate or show people how to do what they want to do. In itself the scientific mode does not attempt to make people want to do one thing rather than another. That is more properly the task of the artistic mode, a mode equally social, one of whose functions it is to generate first the wish and then the will for specific action. Neither of these modes is complete without the other and, in fact, neither in science nor in art is one to be found without the other. Nor between them do they exhaust the significance of art of science for the individual. Beyond them, and common to all forms of human achievement, is intrinsic pleasure produced in the contemplation, or still more in the creation, of new combinations of words, sounds, or colours, or in the discovery of combinations already existing in Nature. This pleasure, though felt individually in the first place, is by no means a private emotion. As the first interest derives from society, so the contemplative act is social at one remove, as is shown by the intense desire, common to artist and scientist, to communicate it.

Every work of science has a purpose and generates a further purpose, but that purpose is not the characteristically scientific aspect of the work, neither is it the beauty of pleasure to be appreciated in the work of science. In its purely scientific aspect, it is recipe: it tells you how to carry out certain things if you want to do them. Nor, on the other hand, is a work of art something that merely moves or pleases. Works of art themselves contain invaluable information about the world and how to live in it, especially when, as in the novel, they deal with social problems.

In stating these abstract characteristics of science, there is always the danger that the abstract may be taken as the ideal, that is, what science should be if only all the unessential aspects of social morality or usefulness could be removed. Indeed, the ideal of pure science—the pursuit of Truth for its own sake—is the conscious statement of a social attitude which has done much to hinder the development of science and has helped to put it into obscurantist and reactionary hands. It should always be remembered that science is complete only if the indications are followed. Science is not a matter of thought alone, but of thought continually carried into practice and continually refreshed by practice.

practice. That is why science cannot be studied separately from technique. In the history of science we shall repeatedly see new aspects of science arising out of practice and new developments in science giving rise to new branches of practice. The professions of the modern engineer are very largely directly due to scientific progress. The very names of the different kinds of engineers there are today, electrical engineers, chemical engineers, radio engineers, indicate that were all originally branches of science that have now become branches of practice.

Scientist and Engineer

But the fact that the engineers have arisen from the scientists, and are continually and closly linked with them, does not mean that the two professions are indistinguishable. In fact, the functional aspects of the scientist and the engineer are radically different. The scientist's prime business is to find out how to do things, the engineer's business is to do them. The responsibility of the engineer is much greater, in the practical sense, than that of the scientist. He cannot afford to rely so much on abstract theory; he must build on the traditions of past experience as well as try out new ideas. In certain fields of engineering, indeed, science still plays a subsidiary role to experience. Ships today, although full of modern scientific devices in their engines and controls, are still built by men who have based their experience on those of older ships, so that one may say that the building of ships, from the first dug-out canoe to the modern liner, has been one unbroken technical tradition. The strength of technical tradition is that it can never go far wrong-if worked before, it is likely to work again; its weakness is that it cannot, so to speak, get off its own tracks. Steady and cumulative improvement of technique can be expected from engineering; but notable transformations, only when science takes a hand. As J.J. Thomson once said " Research in applied science leads to reforms, research in pure science leads to revolution". At the same time engineering successes, and even more engineering difficulties, furnish a continually renewed field of opportunity and problems for science. The complementary roles of science and engineering mean that both need to be studied to understand the full social effects of either."

1.2 Francis Bacon (1561 - 1626)

His basic contention as dilineated in his Books of Aphorisms was that negative observations should be used to correct superstitious beliefs.

1.3 Karl Popper

Professor of Logic & Scientific Method in the London School of Economics until 1969 proposed in his 1934 book "Logic der Forschung" (the Logic of Scientific Discovery) ~~theore~~ the now well known concept of the falsification of hypotheses (on his hypothetico - deductive model).

1.4 P.B. Medawar (from the Art of the Soluble, Penguin, 1969)

"According to the first conception, Science is above all else an imaginative and exploratory activity, and the scientist is a man taking part in a great intellectual adventure. Intuition is the mainspring of every advancement of learning, and having ideas is the scientist's highest accomplishment; the working out of ideas is an important and exacting but yet a lesser occupation. Pure science requires no justification outside itself, and its usefulness has no bearing on its valuation. 'The first man of science', said Coleridge, 'was he who looked into a thing, not to learn whether it could furnish him with food, or shelter, or weapons, or tools, or ornaments, or play-withs, but who sought to know it for the gratification of knowing..."

"The alternative conception runs something like this: Science is above all else a critical and analytical activity; the scientist is pre-eminently a man who requires evidence before he delivers an opinion, and when it comes to evidence he is hard to please. Imagination is a catalyst merely: it can speed thought but cannot start it or give it direction; and imagination must at all times be under the censorship of a dispassionate and sceptical habit of thought. Science and poetry are antithetical, as Shelley so rightly said. Scientific research is intended to enlarge human understanding, and its usefulness is the only objective measure of the degree to which it does so; as to freedom in science, two world wars have shown us how very well science can flourish under the pressures of necessity. Patrons of science who really know their business will support projects, not people, and most of these project will be carried out by teams rather than by individuals, because modern science calls for a consortium of the talents and the day of the individual is almost done. If any scientist should spend five years getting nowhere, his ambitions should be turned in some other direction without delay."

"There is no paradox here: it just so happens that what are usually thought of as two alternative and indeed competing accounts of one process of thought are in fact accounts of the two successive and complementary episodes of thought that occur in every advance of scientific understanding. Unfortunately, we in England have been brought up to believe that scientific discovery turns upon the use of a method analogous to, and of the same logical stature as deduction, namely the method of Induction - a logically mechanised process of thought which, starting from simple declarations of fact arising out of the evidence of the senses, can lead us with certainty to the truth of general laws. This would be an intellectually disabling belief if anyone actually believed it, and it is one for which John Stuart Mill's methodology of science must take most of the blame. The chief weakness of Millian induction was its failure to distinguish between the acts of mind involved in discovery and in proof. It was an understandable mistake, because in the process of deduction, the paradigm of all exact and conclusive reasoning, discovery and proof may depend on the same act of mind: starting from true premises, we can derive and so 'discover' a theorem by reasoning which (if it has been carried out according to the rules) itself shows that the theorem must be true. Mill thought that his process of 'induction' could fulfil the same two functions; but, alas, mistakenly, for it is not the origin but only the acceptance of hypotheses that depends upon the authority of logic."

1.5 T.S. Kuhn (from I. Lakatos and A. Musgrave - eds - Criticism and the Growth of Knowledge - Cambridge University Press 1970)

"Among the most fundamental issues on which Sir Karl (Popper) and I agree is our insistence that an analysis of the development of scientific knowledge must take account of the way science has actually been practised. That being so, a few of his recurrent generalisations startle me. One of these provides the opening sentences of the first chapter of the Logic of Scientific Discovery: 'A scientist', writes Sir Karl, 'whether theorist or experimenter, puts forward statements, or systems of statements and tests them step by step. In the field of the empirical sciences, more particularly he constructs hypotheses, or systems of theories, and tests them against experience by o observation and experiment'. The statement is virtually a cliche, yet in application it presents three problems. It is ambiguous in its failure to specify which of two sorts of 'statements' or 'theories' are being tested. That ambiguity can, it is true, be eliminated by reference to other passages in Sir Karl's writings, but the generalisation that results is historically mistaken. Furthermore, the mistake proves important, for the unambiguous form of the description misses just that characteristic of scientific practice which most nearly distinguishes the sciences from other creative pursuits.

There is one sort of 'statement' or 'hypothesis' that scientists do repeatedly subject to systematic test. I have in mind statements of an individual's best guesses about the proper way to connect his own research problem with the corpus of accepted scientific knowledge. He may, for example, conjecture that a given chemical unknown contains the salt of a rare earth, that the obesity of his experimental rats is due to a specified component in their diet, or that a newly discovered spectral pattern is to be understood as an effect of nuclear spin. In each case, the next steps in his research are intended to try out or test the conjecture or hypothesis. If it passes enough or stringent enough tests, the scientist has made a discovery or has at least resolved the puzzle he had been set. If not, he must either abandon the puzzle entirely or attempt to solve it with the aid of some other hypothesis. Many research problems, though by no means all, take this form. Tests of this sort are a standard component of what I have elsewhere labelled 'normal science' or 'normal research', an enterprise which accounts for the overwhelming majority of the work done in basic science. In no usual sense, however, are such tests directed to current theory. On the contrary, when engaged with a normal research problem, the scientist must premise current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that, given sufficient brilliance, it can be solved. Of course the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. If it fails the test, only his own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than current theory which is tested".

"I suggest then that Sir Karl has characterised the entire scientific enterprise in terms that apply only to its occasional revolutionary parts. His emphasis is natural and common: the exploits of a Copernicus or Einstein make better reading than those of a Brahe or Lorentz; Sir Karl would not be the first if he mistook what I call normal science for an intrinsically uninteresting enterprise. Nevertheless, neither science nor the development of knowledge is likely to be understood if research is viewed exclusively through the revolutions it occasionally produces. For example, though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing. Or again, it is for the normal, not the extraordinary practice of science that professionals are trained; if they are nevertheless eminently successful in displacing and replacing the theories on which normal practice depends, that is an oddity which must be explained. Finally, and this is for now my main point, a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extra-ordinary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores."

Concept of paradigms (example or Model).

Paradigm —falsification→ Paradigm (Scientific Revolution)

within the paradigm lot of research work carries it forward. — Normal & Revolutionary Science. (Scientific evolution).

1.6 Science is Hypothetico – Deducto – Inducto

1.7 Pure (Curiosity oriented) and Applied (Mission oriented) Research.

1.8 Hard (Physics, Natural Chemistry & Bio.) & Soft (Social Science)

1.9 Social significance of Science.

2. Methodology in Science. (Deductive process)

Fig V

2.1 Idea emerges from past experience or from pilot experiment.

2.2 Hypothesis is formulated.

2.3 Experiments ~~densed~~ devised to test hypothesis. (Criticism)

2.4 Experiments performed and results evaluated to reach condusion. Intellectual Honesty

2.5 Hypothesis accepted or rejected.

2.6 Errors involved - a) methodological or
b) conceptual.

(Hypothesis accepted when it should be rejected and hypothesis rejected when it should be accepted)

2.7 Repetition of experiments by same person - replication in time (how many times is a bit of a question) and by others for corroboration.

2.8 Methods of Science - a) Observation (astronomy)
b) Survey (Sociology)
c) Experiment (Hard sciences).

can be either Qualitative or or Quantitative differences
(discontinuous) (continuous)
that are observed.

2.9 Controls (Positive, Negative etc.) and Blind (coded) experiments.

3. Processing of Data.

Fig VI

3.1 Concept of probability.

3.2 Measures of central Tendency . Mean, Mode & Median.

3.3 Measures of dispersion : Range, Variance, Standered Deviation.

3.40 Descriptive statistics : Tables, Graphs, Charts etc.

3.5 Analytical Statistics: Tests of significance, ~~test~~ Analysis of variance, χ^2 analysis, Regression and Correlation analyses etc. Null Hypothesis

3.6 Parametric & non-parametric.

Sampling
Design of Expts.

4. Conclusions

Prof of Maths King's College, London
Aug 1983 - Master of Churchill College Cambridge
chairman NERC (Nat. Env. Res. Council)

4. Conclusions.

4.1 Finding research problem - Professor Sir Herman Bondi - FRS
The Making of a Scientist, Royal Society of Arts Journal (1983) Vol: 5323 & pp. 405 - 406)

"Science is, above all, concerned with research. To do something new, to find out something that nobody knew before, is its prime aim. It is like a cart that can be rocked backwards and forwards and thus sometimes makes a little progress in its bed of mud. It is this progress that I see both as its ultimate attraction and the ultimate test in the making of a scientist. It is when you have done something new, however minor it may be, that you really joined the ranks of the profession. I am not sure that we always make it appear sufficiently exciting for young people when they first achieve something that has not been done before. Americans often say that there are no frontiers. You can always do something new andto have done so should be a tremendous moment. Of course, as I have indicated, reaching this stage is not easy, particularly if you try to do it within the three-year confines of a research studentship. The very essence of science is that one does not just look at the interesting problems, but at those where progress appears to be just possible. Peter Medawar once called science the art of the soluble, and I have the greatest respect for that terminology. I often say that if you walk along the street you will encounter a number of scientific problems. Of these, about 80 per cent are insoluble, while 19½ per cent are trivial. There is then perhaps only half a per cent where skill, persistence, courage, creativity and originality can make a difference. It is always the task of the academic to swim in that half a per cent, asking the questions through which some progress can be made. This is where greatness in science shows itself. It is no use thinking great thoughts about great problems if you make no impression on them. Your choice should always be for an area where you can make an impact. This is perhaps the most crucial feature in making a scientist: that his supervisor, professor, teacher chooses something soluble for him, something that can be done in a very limited timespan by somebody who is new to the topic. This is not an easy task for the teacher".

4.2 Predictability.

4.3 Creativity.

4.4 Funding - Social relevance etc. N of Scientists etc.

4.5 Research Paper. - Is it a fraud?

5. Reference.

5.1 Aligarh Muslim University, ~~Asia Publishing House~~, ~~1963.~~
Science:Its Method and Outlook., Asia Publishing House., 1963.

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5.3 Jevons, F.R. , Science Observed., George Allen & Unwin, 1973.

NARESA

Seminar on Presentation of Scientific Papers

22 Oct. 83

ORAL PRESENTATION OF SCIENTIFIC REPORTS AND PAPERS

N. Vignarajah[1]

Introduction

Must effectively convey to audience what we want to be conveyed. Knowledge of the subject alone will not suffice.

A good speaker is often made and not born.

Sometimes, subject itself is interesting. Often, interest has to be created by speaker. A good speaker makes a difficult subject easy to assimilate and a bad speaker makes an easy subject difficult to assimilate.

Principles of good presentation

1. Aim - very clear and definite.
   Think - To whom I am talking to, what I intend to achieve, why should it be conveyed, what has to be conveyed, ~~conveyed~~, what aspects are important, what standards have to be reached, within what time I have to achieve my objective and finish the presentation.

2. Planning and preparation.

3. Create interest in audience and maintain it.

4. Use of five senses.

5. Simplicity.
   Language, dress, delivery.

6. Human factor.
   * Speaker must show leadership.
   * Consider audience as individuals and not as a whole.

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1 Deputy Director of Agriculture (Research),
Regional Agricultural Research Centre, Makandura, Gonawila (NWP).

* Approachable.
* No sarcasm, no bluffing.
* Patience and tolerence.
* Encourage and praise when deserved.
* Confident, modest and not boastful.
* Be natural and relaxed.
* See yourself through eyes of audience.

7. Personality.
   Dress, appearance, mannerism, posture

8. Command of the language.
   Simple, appropriate words.

9. Confirm by stages.
   Difficult in symposia and when presenting papers.
   Possible in working group meetings, lectures for students, courses.

<u>Presentation</u>

1. BACKGROUND INFORMATION, INTRODUCTION

2. OBJECT
   Limited objectives.
   Do not veer from objectives.

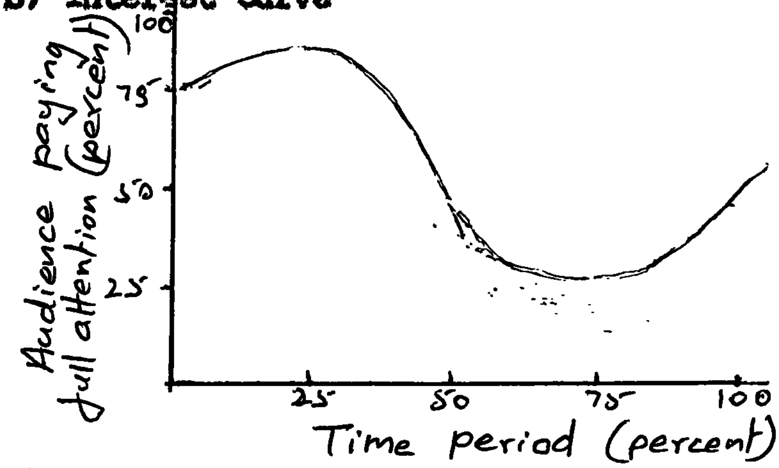
3. PLANNING AND PREPARATION
   "Time spent on planning is time well spent"
   - Napolean.
   Marshalling of facts, sequence, build-up of the subject matter (what audience must know, should know and could know).
   Seating arrangements, etc. for audience.
   Test communication aids (eg. slide projector must work and slides must be in order).
   Rehearse.
   Spare no pains.

4. HANDOUTS, PRECI, ABSTRACTS

5. TRANSMISSION AND RECEPTION

a) Use communication aids - blackboard, slide projector, charts, graphs, pictures, overhead projector, models, specimens. Must be clear and not confusing. Visible to all. Choice of colours.
"A good picture is worth a thousand words"
- Confucius.

b) Interest Curve



c) Use time allocated judiciously to achieve objective.

d) Dramatic sense.

e) Visibility.
Instructor as well as communication aids

f) Audibility.

g) Use your eyes!
Look at audience as often as possible, cast your eyes around.

h) Speech.
Don't rush, deliver slowly, don't swallow words, pause when appropriate, raise voice to emphasise, don't use outworn tags.

i) Posture and mannerism.

COMMON MISTAKES, DOs and DON'Ts

* Don't just read through script. Look at audience as often as possible, pause to explain or emphasize.
* Avoid unnecessary details.
* Don't waste time on apologies, dry jokes, stories, correcting mistakes in handouts.
* Break monotony - use variety of communication aids.
* Don't distract audience by exhibiting too many charts. Show charts, specimens, models at the appropriate time and then put them back.
* Explain charts, graphs clearly - scale used, what is on X and Y axes, key.
* Avoid repetitions.
* Stand clear of the board/chart.
  Use a pointer.
* Don't mess-up graphs, etc. on board and confuse audience.
* Keep to your time limit.