Research and the Real World

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Schofield Memorial Lecture

Dr. Francis William Schofield (1889 - 1970) served on the staff of the Ontario Veterinary College (1921 - 1955) initially as Director of Veterinary Hygiene and Research and subsequently as Professor and Head, Department of Pathology. He was an outstanding teacher but it was in the field of research that he made his greatest contribution, including the discovery of dicumarol in mouldy sweet clover as the cause of hemorrhagic disease in cattle. Upon his retirement in 1955 he returned to Korea to teach veterinary science and Christian beliefs.

Dr. Blood's lecture on Research and the Real World was delivered at the University of Guelph, October 8, 1981.

In Memory of Frank Schofield

Choosing a title for this lecture seemed to me to require that I should choose a subject, and from that subject select a topic, that would bear some relationship to Schofield the man, and his life's work, and which would also be provocative as he so often was.

My personal contact with Dr. Schofield was very brief and consisted of two or three casual meetings when he was visiting the College from Korea. The meetings were brief but they were memorable. They were long enough for me to sample his acerbic wit, his critical mind, and his willingness to be brief and to the point. His achievement record is characterised by research of a particular quality - innovative research about real problems, problems that occurred naturally in animals in the field and in the barn. Mouldy sweet clover poisoning and blood coagulability is one example, hepatic dysfunction in poisoning by alsike clover is another. They were problems that were not really resolvable with the technology of the day, and with the resources that Frank Schofield had they were problems that could be researched only at a modest level and with little opportunity for experimentation.

This attempt to honor Frank Schofield's memory sets out to be provocative, to avoid pretense by setting out the problem clearly and honestly, and finally attempting to be constructive as well as being critical. He would, I am sure, have been provoked by and probably would have agreed with the principle of the comments I make on the status of veterinary research today. How important I think the subject is can be judged by the fact that I have chosen this honorific occasion to air it.

The Problem - Detachment from Reality

The course of action which I think is leading the veterinary profession away from its proper path, and which is doing more than anything else to frustrate our full development as a profession, is our increasing detachment from reality, from the real world. That judgement applies to both of our professional activities, to the professional services that we supply direct to the community, and to the research that we do as a back-up to enable us to provide an even better service. I refer to the general direction of the research, and in particular to the balance between basic research on the one hand and applied research on the other.

For the veterinary profession, the real world is easily recongisable and definable. Reality for us is our responsibility to provide the best possible services to maintain the health, welfare and the productivity of the animals in the community. Productivity in food animals means economic profitability of the enterprise, in racing animals it is measured by performance, and in companion animals the provision of happy, trouble-free, psychological support and emotional comfort for the owner. I suggest that we are losing touch with those objectives. It is easy enough to do that; for example, by deriving satisfaction from our research solely because of a successful proving of a scientific hypothesis, by doing the wrong kind of research, in the wrong mode, for the wrong motivational reason, or with the wrong objectives. I propose to touch on some of these.

To prove that his tendency does, in fact, exist would require a computer simulation model of great complexity to produce a result with any validity. The alternatives of estimating the proportions of papers published on "real" as agains "unreal" research, or of counting the numbers of graduate theses in the output of a university which are in these categories, or assessing the amounts of research funds invested in the two areas have been used for such comparisons and all have such serious shortcomings that resorting to subjective impressions seems as valid. The following arguments are based on my own impressions, but I think they reflect a good proportion of professional opinion.

I propose to draw attention to three fairly obvious gaps in our knowledge which are of vital importance to what I have described as our professional reality. These relate to profitability in food animals, physical performance in racing animals and psychological profiles in companion animals. They are:

(1) *in Animal Disease* - the absence of a central, generally available data base on which to base decisions in treatment and control

(2) *in Animal Production* - the absence of fundamental and other performance data on which decisions about sub-optimal activity can be based.

(3) *Predictive Profiles* in biochemical, biophysical, endocrinological and other parameters.

These subjects more or less select themselves by their generally poor appearance record in periodicals and in research grant applications.

1. Animal Disease of the Great Data Bank Deficiency

I want to take as the first example the state of the generally avialable store of information on animal disease on which we base our diagnoses, our assessment of response to treatment and control and our predictions about outcome. My conclusion is that, apart from the communicable, notifiable diseases, we know very little as a profession about the prevalence of the other common diseases, nor about the effects of what we do on them. We may know this as individuals but there is no central data bank which incorporates all the data.

For example the reality for the horse and the horse owner with respect to strangles is that it is necessary to know what effect vacination will have on the course of an outbreak of the disease, and what effect treatment will have on the course of the disease in individual animals. There is no hard data on which to properly base an accurate opinion common to all members of the profession about either of these matters. Neither has there been any assessment of the rate of return to normal function, not survival for one week, but return to profitable function, of cows treated successfully for coliform mastitis. The recovery rate six months after the event is very low indeed and an accurate figure is vitally important to an owner who is about to invest $300 in having his $2,000 cow treated.

Decision-Making in Treatment and Control

Until we have that sort of information it is not possible, when dealing with a sick animal, to predict the outcome nor the profitability of a particular course of action. However, what the client requires from us is authoritative advice, and not in any abstract sense either. Courts are increasingly inclined to insist that professional advisors have a financial responsibility for the outcome of the recommendations that they make. This is not a question of negligence but one of responsibility.

As an example, I show you a formula (Fig. 1) which we would so much like to use in our everyday work advising clients—and half of what we do—half of our importance to clients is advising them what to do. The client may want to know incidentally why his dog is paraplegic but his real need is to know the percentage probability of its achieving comfort (happiness) and some function and whether he should choose treatment (a), among the possible choices of (a), (b), or (c). We can and do advise in a general way about the probability of the outcome but without the authority of a large statistical backing which would enable us to say - in the last 1000 cases of that disease, in dogs of that age, and that breed with that course, a favourable outcome with treatment (a) was 5%, with treatment (b) was 15%, and treatment (c) was 35%. The only way that we can answer the question accurately is to have a large data base of actual results, rather than a personal opinion, perhaps biased by one's own inclinations as to which treatments should be used.

Field Data in Etiology and Pathogenesis, and in Diagnostic Decision Making

Figure 1.

COST EFFECTIVENESS IN THERAPY & CONTROL

for Survival

$$\left(\text{Value of patient as a survivor (salvage or emotional)} \times \text{Percentage probability of survival with this technique}\right) - \text{Cost of technique and aftercare} = \text{Net present worth}$$

for Return to Full Function

$$\left(\text{Value of patient as a functioning unit} \times \text{Percentage probability of return to function}\right) - \text{Cost of technique} = \text{Net present worth}$$

Besides the need for a statistical appraisal of the response to treatment and control we also need to know more about etiology and pathogenesis of most diseases, and thus provide a better basis for their treatment and prevention. However, in that interval, before etiology and pathogensis of a disease can be proven, we can make a presumptive, preliminary hypothesis about it and collect epidemiological data to support it or refute it. In many instances it will be the only hypothesis we ever have because there isn't enough time or money or people to fulfill Koch's postulates for all diseases, especially the non-microbial ones, and especially those which are caused by a combination of agents, the multi-causal diseases.

In the same way the probability of a particular disease being the correct diagnosis can be determined with greater accuracy if the decision is based on the rate of occurence of the clinical signs and other evidence observed in the case at issue, in a large number of previously identified cases recorded in a central data bank.

Unused Clinical Records

Our poor achievement in accumulating a clinical data base is largely due to our failure to conduct our clinical work as a research program - as a case control study. In many ways clinical work is one long experiment - one examines an animal and on the observations made constructs a hypothesis as to cause. The hypothesis is then tested by treatment. If we conduct so many experiments we should record the results, consolidate and analyse the data, if possible prospectively rather than retrospectively.

There is undoubtedly great value for the individual animal or herd in being able to consult its previous clinical record provides data for clinical research and if they are not used they are a notable waste. The sad part about this data bank deficiency is that we could now have many of the answers we need, or most of the data to provide the answers to questions of therapeutic and preventive decision making, and to many of the questions concerning management techniques and disease occurrence if we had in the past consolidated and analysed our clinical records.

2. Animal Production

Even in food animal medicine the available information on optimal production levels is limited and in many, if not most areas, what information is available does not have cost/benefit analyses to go with it. That is to say, we have numberical answers, but not the ones that reality demands - the economic answers. However a start has been made in that nowadays we talk in terms of reproductive efficiency, optimum body weight at first mating, daily rate of gain, food conversion efficiency and so on and what the wastage in these parameters costs.

Similar production data is not nearly as extensive in the other classes of animals. There are no growth tables for horses, based on the actual population, on which we can base a diagnosis of slow growth and weight loss. Nor are there performance tables graded for weight and age with which we can compare the speed or endurance of a racing horse, and this may be a desirable comparison in a clinical sense or when monitoring an animal's development; when it is desirable to know if a horse has good exercise tolerance, or whether it is responding well to training. Such data is essential. There are no standardised test diets which can be used to determine the rate of body weight gain for an individual horse. Nor are there tables of mean body condition scores to determine whether a horse is being adequately fed. The horse is the veterinarian's exclusive domain and there is a gap in the horse husbandry area that is more in need of development than further refinement of techniques to assess a horse's acid base balance.

In dogs and cats and other companion animals the problems and deficiencies are just as great but the solutions are not as obvious. Information on growth rate, body weight and exercise tolerance would be of value in many situations but it is psychological information which is the big deficiency. If we are to fulfill our obligations in companion animal work, and in all that companion means, it is essential that we have more information than we do in helping to match a client's psychological needs with the psychological profile of the companion animal most suited to those needs. We do it now in an amateurish way but the area deserves at least as much intellectual input from us as a profession as does the development of new surgical techniques for correcting inherited skeletal defects. There is developing a greater interest in canine psychology related to obedience classes, and an interest also in pet-assisted-therapy, so that perhaps the beginnings of what I suggest are there. It is conceivable that the contribution that the veterinary profession could make in this area, and that of animal welfare, and both of them are our sole prerogative, could be as important as our contributions have already been to the diseases of these animals. It would take us out of the area of salvage with its connotation of exploitation into the positive, productive area of human-animal relationship.

My view is that we have selectively neglected this field in favour of what has become known generally as comparative medicine. There are good reasons to doubt that most of what is done under that label has any real impact on human medicine, with the exception of those diseases which are exact biochemical or structural/neoplastic replicas in the two species. There is no doubt that most of the advances in companion animal medicine and surgery originate in human medicine but that is a different consideration. **To summarize this point I think that the comparison of the relative importance of the two areas, when assessing research priorities, is a comparison between the diseases of dogs and cats (unless there is a specific comparative medicine connotation) as against their emotional and psychological usefulness to their owners.**

3. Predictive Profiles in Biochemical, Biophysical, Endocrinological Parameters

This is the pipe-dream to which we are all susceptible, the philosophers' stone that will turn all our predictions into material gains. There have been quasi-attempts, especially in racehorses, to predict performance based on heart size, on hematological and blood biochemical data, but with very little real information available on which to base judgements. The Compton Metabolic Profile is the most famous in food animal medicine, and the study of cytogenetics in all species has something of this applicability. No concerted effort has been made to evaluate any of these profiles. It would fill a very obvious and logical gap to do so. It would then be possible to measure health and production status and then predict the future of them.

The Causes

Set out below are some of the reasons why the error of detachment from reality is occurring. It is a multifactorial etiology as one would expect.

The Late Coming of Veterinary Epidemiology

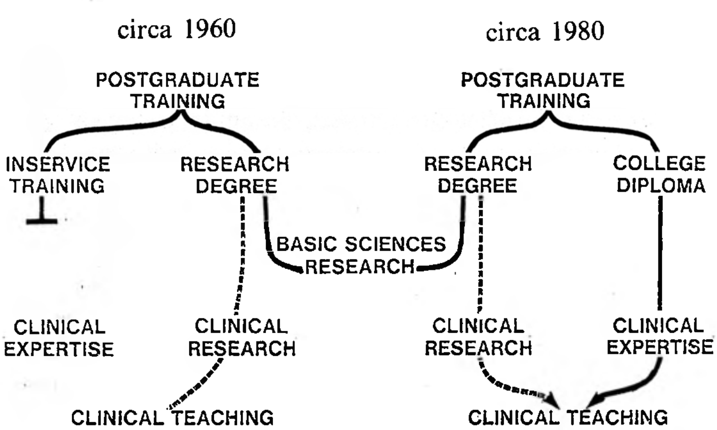
A big area of deficiency is the province of the field veterinarian, the clinician, and his/her collaboration with the epidemiologist. Until recent years there has been a dearth of epidemiologists and their orientation had been largely microbiological. Nowadays our activities are so much concerned with diseases caused by management, by production excesses, by environmental insults, many of them man-made, and a whole new epidemiological technology is required.

The Divided Responsibilities of Field Veterinarians

The problem begins in the University veterinary schools and although the schools do not dictate the profession's activities, they dominate them by having all the profession in their hands during their impressionable years and they tend to set the patterns of behaviour. Amongst the staff the clinicians have a serious responsibility that is peculiar to them - the need to establish authority in their teaching by clinical expertise, by a big caseload. So that their research time, compared to other staff, is limited. As a consequence the leadership in clinical sciences for research tends to be limited.

Appointment and promotion for academic clinicians has been a problem area ever since professional schools joined the universities and threw in their lot with basic sciences and humanities. It is only 15 to 20 years since the American College of Clinical Sciences (and the Australian College of Veterinary Scientists) opened their doors and began to award professional qualifications without a research component. This provided an alternate pathway for advancement and an escape route from the portal requiring research. Both portals have been used, with a preference I would think to Board Certification. In either case the research thrust has been further diluted. This is set out graphically in Fig. 2.

Figure 2. Pathways to Clinical Teaching



Board Certification versus Research Degree

A sequel to the development of the alternate portal of Board Certification for academic clinicians (and paraclinicians) has been a neglect of the research path and the deficiency is obvious as I have said. I have been a protagonist of the dual portal for many years but nowadays I incline to the view that all university staff should be required to add to our accumulated knowledge - should participate in research, perhaps only at the pilot trial or extended field trial level, possibly in collaboration with other disciplines working at other levels.

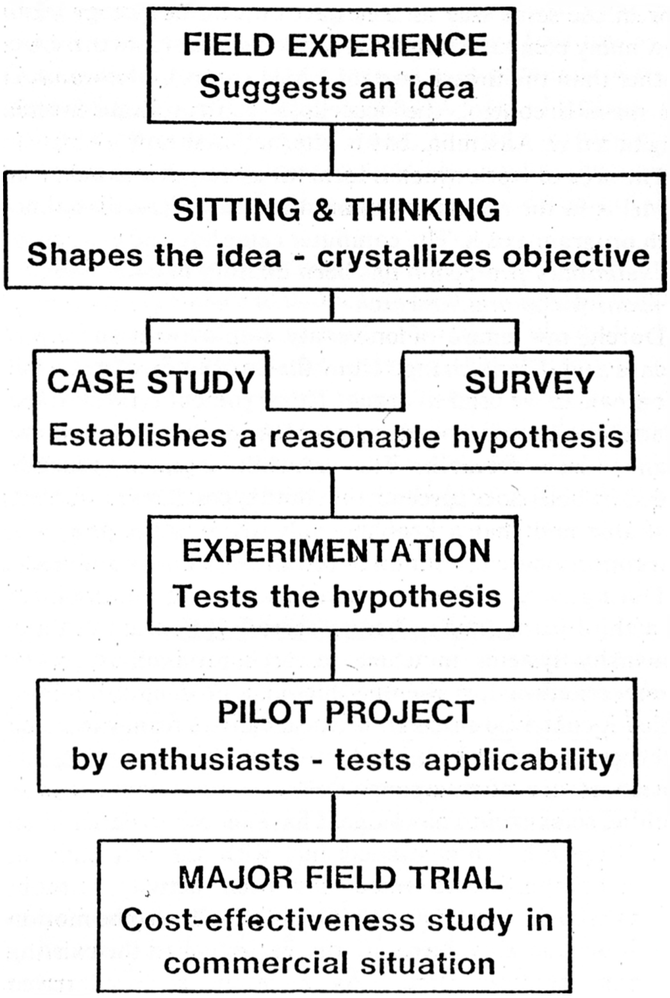
Imbalance in the Scope of Research

One of the reactions to a relative shortage of clinical research activity and leadership has been the turning away of persons who did want to follow a research career while in a clinical department, to paraclinical (pathology, microbiology and parasitology) and preclinical (anatomy, physiology and biochemistry) areas for research guidance. Inevitably some of these people become graduate students and/or research workers in their adopted discipline and never come back to clinical sciences. Others do and add a great deal of microbiological or physiological expertise to clinical departments. There is nothing wrong with this provided there is some balancing input into aspects of clinical medicine, preventive medicine and epidemiology, surgery or theriogenology. It was largely in response to this pressure that herd health programs were initiated.

One of the most important effects of this movement of clinical personnel to paraclinical pursuits has been a change in the type of research done. This is a large subject and one which I can afford to touch only briefly here. I do so by referring to Fig. 3 which sets out an idealised program for the development of a research project or more properly a research program. The path begins as an idea, passes through the development and testing of a hypothesis by experimentation, to the particular and then general stages of adaptive research to the testing of the applicability of the new knowledge to the real world. I do not suggest that research must follow this path. There is no reason why some research should not start in the middle and end in the middle, at experimental research, without ever attempting to proceed to a field trial. However, experimental research is not the only kind of research and just as experimental research can continue at great length without reference to other levels of research, so can each of the other levels. For example, the research that my group has done in herd health programs in the past 20 years has all been in the area of pilot projects and field trials. Also the Ontario Veterinary College, of all the veterinary schools in North America, conducts much research and has a prominent reputation for researching in this same area which is closest to the "real world" of food animal medicine.

When the professional faculties joined the Universities they accepted, to their advantage, the constraints applied by pure science. However, even science found it necessary to establish schools of applied science to escape from the

Figure 3. **PATTERN FOR AN IDEALIZED RESEARCH PROGRAM**



narrowing influence imposed by an overstrict adherance to the need for proof by experiment. There are many subjects in medical science, and the social sciences that are not susceptible to experimentation, for example, performance and behaviour in a community. Research on factors affecting these matters can only be done by observing the community in operation in natural circumstances. The definitive research on estrus detection in cattle was done in this way.

This sort of research is of course done all the time in human epidemiology and is widely accepted in veterinary science but tends to be carried out by governmental and industrial organizations. Any suggestion that universities should restrict their activities to pure research and detach themselves from applied research, in fact the real world, is repugnant because the development of new techniques in all fields of research is an important function of universities. However, it is an attitude of mind which is not uncommon in science faculties who may wish to impose their will on the applied sciences.

Early Sequestration of Potential Research Leaders

The leadership in all University departments is an area of increasing wastage because the best brains, the most active people, who should be keeping in touch with the real world of the animals, are taken out of the arena to administer. The problem today is that the transfer from real work to administration for the bright people comes much earlier, and at the time when they should be making a maximum contribution to scholarly affairs they are consumed by affairs that could be handled by others. The enormously increased administrative load imposed by the much larger budgets that we have to handle, and supplemented by the constraints of industrial relations, the expansion into graduate work of greater numbers and more courses and continuing education of all sorts, the greater supply of research money and pressures to do more research and so on and so on has been absorbed by academicians. These people are then taken out of the teaching and research line (largely) and replaced by young (usually) less experienced (mostly) and less capable (sometimes) people. The administrative pressures in clinical departments are the greatest of all because of the additional demands of maintaining a supply of clinical material, the consuming demands of the hospital clientele.

The Unconscious Bias of Granting Agencies

There are pressures which affect the volume of research carried out and some of them are included in the list above. There are other pressures which, probably more importantly, apply constraints to the type of work being done. Most important of these is finance. Money provided by granting bodies has to be very carefully allocated and the pressures that they are under to maintain solvency, to avoid being plundered, and to keep providing for the newcomers has led to a pattern of behaviour which includes:

- short projects preferably lasting one year, with a maximum of three.
- to avoid indexation, escalation and continuation implied in providing salaries, by avoiding salaries where possible.
- partiality for projects calculated to produce one or more graduate students.
- maintenance of a keen watch on the interests of the donor body, if there is one.

The net effect is to encourage submissions for projects that are bound to terminate, with a finite answer in two years and produce a PhD or MVSc. Long term projects, especially in the area that I am anxious about, the epidemiological and economic assessment of diseases, especially the non-infectious ones, that require a population to be kept under surveillance for ten years, find it hard to attract support.

The Costs of Large Animal Research

The costs of research can be immense especially if one is investigating problems of productivity in say dairy cattle under various combinations of housing, nutrition and general management. The cost of purchase of animals and their subsistence alone is enormous. In order to keep numbers down and reduce costs it is usually necessary to interfere with the natural environment in order to limit the effects of uncontrolled variable. In doing so we can greatly reduce the generalisibility of the results. In other words, the results of the research station are not necessarily applicable to the commercial farm, and the results of the enthusiastic university research workers are not necessarily achievable by commercial farmers.

So that applied research really has to be done in the "real world". However, the problems of doing so are very great. The farmers are not completely controllable, experimental research is not possible except in the most limited way, confounding by unpredictable changes in management and environment play havoc with plans. In spite of these difficulties, the work must be done to test the effectiveness of say a new control program under field conditions. "On farm" research is unavoidable and evading it because it is difficult is no longer tenable.

One answer to this almost insoluble problem is the development of computer simulation models, a not inexpensive exercise which requires masses of natural and if possible experimental data. Much of the production data for such a model is already available but there are great gaps in the disease data. Those gaps represent forty years of neglect, or more charitably evasion, and of inordinate preoccupation with other facets of veterinary research. They need to be filled in quickly if this aspect of veterinary research is to be brought into step with the rest. The problem of course is the same as that of all low probability investigation - especially systems containing so much noise, so much uncontrolled variables - the need to collect masses of data, to have masses of observers, and the costs of handling such volumes of data. These are problems which will be very familiar to those of you involved in this kind of work. I trust that they will be recognized as a pressing concern by those of you who do not.

The Latecoming of the Electronic Computer

Research projects based on prospective surveys, which contribute a great deal to epidemiological knowledge, were very time consuming when all the work had to be done manually. If routine observations can be entered into a case record so that its recording can be carried out automatically by machine such surveys become routine and very feasible. Also, biological systems which are so complex that it is not possible to dissect them by manual means can be dealt with simply and routinely once a computer simulation model has been created. This is not a simple task, nor a cheap one but it does make possible the analysis of the performance of a dairy herd when it is responding to a number of variable influences at one time.

The computer has made possible much of the work which has not previously been possible, mostly because it can handle repeatable tasks of computation much faster and more accurately than office staff can do. As a new technology of very great value it is surprisingly slow to assert itself in almost all areas of veterinary medicine. For the clinical sciences if offers an almost complete release from the bondage of research deprivation. To support the contention that computerisation is slow in developing one has only to count how many veterinary schools have data processing labs in the same way as they have clinical pathology labs; how many computer simulation models of diseases there are - other than the British one for FMD control, Melbourne's for mastitis control, the brucellosis control model which originated in Australia, but is international now.

The area of work which is ideal for a computer simulation model is in the evaluation of herd health programs, but no such program exists. The computer came late and as a group the veterinary profession has been dilatory in embracing it.

Reasons for Doing Research

During my tenure of university employment there has been a significant change from the two kinds of research about which we used to argue. The argument related to the relative merits of applied, or problem-oriented research as against basic research. The erstwhile argument usually ended by both sides agreeing that both types of research were desirable and that a problem only arises when there is a disproportionate utilisation of resources in one or the other.

During the past 20 years there has been the development of a third form - "contrived research". It appears in those university systems in which research productivity is the prime criterion for appointment and promotion. I have called it contrived research because it arises from simulated intellectual curiosity, is conducted for ulterior motives, its objectives are often wrong, and it is very likely to add nothing to existing knowledge. I have set out to define it as:

> "Research work carried out with the objective of establishing that there has been activity without satisfying the demand that there should also be motion - forward movement by way of adding to the existing sum of knowledge." As a result, such contrived research:

- creates problems to solve rather than attempting to solve naturally occurring ones (or if the real questions are too difficult, ask others).
- the research is inane because the question to be answered has not arisen from another question or proposition.
- the intellectual output per unit of input is small.
- has a natural tendency to cease at the age of 30 years.
- fills a large part of veterinary literature.
- may exist only in the eye of the beholder.

The problem created by contrived research is that it is a negative approach and contributes to the gap created by the absence of statistical data on prevalence, treatment response and so on. It creates irrelevant problems when natural problems are too difficult.

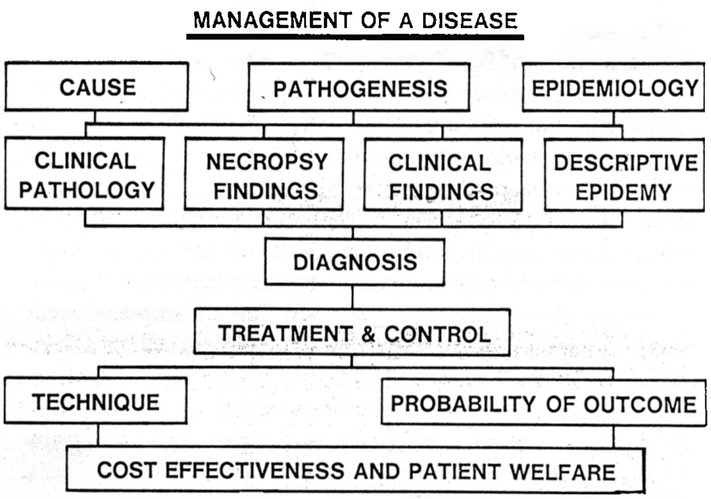
The Wrong Objectives in Research

It is not a difficult thing to do, to have the wrong research objectives, usually by way of omission, so that for example, the objective becomes a scientific one and omits the financial answer that should be added to make the project meaningful to the end-user of the results. For example, a mastitis control project of which the objective is to detemine the effect of selective dry period treatment of cows on the quarter infection rate in the next lactation would be improved by the addition of another objective of determining also what the effect of the treatment had on productivity. That was as it should be. The project would then have an economic as well as a bilogical end point. Similarly, the objectives in companion animal work may include biological advancement but they must include also, and at the top of the priorities, the satisfaction of the emotional needs of the owner.

Lastly, a problem to which I referred earlier and because of my own experience of it, one that I tend to bring up often - that of narrowing the area of application of results by controlling so many of the variables that the results are inapplicable to any known real situation. Dissipation of generalisibility I think expresses it well - a fault of objective.

Research by enthusiasts is much the same thing but a matter of technique rather than objective - the situation

Figure 4.



created when a research worker who is enthusiastic about the success of a program puts in a great deal more thought and planning than a working veterinarian or farmer can possibly do.

The Bottom Line

In Fig. 4, I have set out the anatomy of the study of any disease, or if you like of all diseases. I have done this so that I can say again that although there is much knowledge already available about the cause, clinical pathology, clinical findings, treatment and control of most diseases the science of epidemiology has been late in starting so that the epidemiological aspects of diagnosis and control are still behind, especially the economic epidemiology concerned with the relationship between management and the occurrence of the disease, and the economic epidemiology which assesses value of wastage and cost of control.

It is this latter consideration which brings me to my main thesis in this area—the pre-eminent importance of making the correct decision on treatment and control. As set out in this diagram it is the bottom line. It is making the correct decision based on cost-effectiveness and animal welfare. We have the techniques but our knowledge of probability of outcome is meagre, and that keeps us out of touch with reality.

The need to avoid inflicting pain on animals in both the companion animal and agricultural sectors has been emphasized a great deal by the community at large, and this has now been joined by prevention of harrassment or as some put it - protection of the natural dignity of animals. This is a contentious and divisive subject within the profession as it is amongst the community at large - it polarises a community into two camps and there appears to be little common ground. Although the argument cannot be resolved, the problem cannot be ignored and must be included in al decisions on animal matters. Cost effectiveness is not all. The veterinary profession is the final defense for animals against the importunings of man and we must accept that responsibility.

In Summary

If our research effort as a profession is unbalanced as I think it is, part of the fault may lie within the Universities where the research begins and that's why I raise the matter with you. If there is a shortfall the probable causes include:

- Universities tend to favour basic research and not enough is done to redress that balance in professional faculties where applied research does need to be done because academic clinicians are preoccupied with a heavy case load.
- by this default, amongst other things, the overall leadership of research in Universities tends to be in the basic disciplines with obvious effects on the kind of research done.
- the basic disciplines are more inclined to be experimental and easier to plan as short term projects or short term modules of a larger problem - whereas most present day disease problems, especially those related to nutrition and metabolic errors, and their epidemiology, are complex and require long term projects which are much more difficult to fund than short term ones.
- the larger problems present much more of a problem in data handling because of its volume, and because in survey and case control studies much of the data is of varying degrees of reliability short of good.
- the complexity of these multicausal diseases which appear at a variety of severity, and the need to limit their wastage to varying levels depending on what the economics of the situation demands, requires the use of a computer and a computer analyst and programmer and his/her software and a well-designed computer model.

I think it is a reasonable statement that as a profession, we have neglected mathematics, statistics and biometry. Quantitative biometry is almost non-existent. We have been refugees from computation for too long and it's time for us to return to the promised land, not as mathematicians any more than most of us are hematologists or tissue pathologists, but as interpreters of the statistical analysis. In the same way as we have become adept at interpreting a microbiological report.

The Solutions

I foreshadowed that I would offer some solutions which consist almost entirely of correcting the shortcomings in our profession's activities which I have already set out.

The identifiable shortfall in applied research could be aided by the following items:

1. Repairing the deficiency of contact with reality by promoting research participation by those arms of the profession which are most in contact with the real world. This includes particularly the private practitioner and the government field officer.
2. Promotion of those research modes which are neglected resulting in an imbalance of the total effort, and which are designed to translate the real world's problems to the experimentalist and at the other end, to translate the experimentalist's results to the farmers and animal owners who are to use them.
3. Fostering the development of long term research because the observations must continue over time. A natural corollary will be a return to the long-term, in service research degree, almost forgotten these days.
4. The size and complexity and duration of this sort of research will place more emphasis on the need for the silent wreaker of miracles with routine tasks, that servant with no industrial legislation to constrain it - our new-found friend, the computer.
5. A particular specific need in on-farm research is the devising of protocols which do as much as possible to overcome the difficulties inherent in a commercial farm over which one has virtually no control, and at the same time arrive at the end of a project with results which can influence the farmer's decision on which choice of alternative paths to follow or treatments to use. At the present time we are following the route of the computer simulation model and I wish us luck.
6. Another necessity for the development of "on-farm" research is a means of avoiding total commitment to orthodox experimental research procedures and classical statistical techniques which are designed to establish principles and not to resolve problems concerning detail and which are not principles but tactics of what to do this time in this particular set of circumstances. So that the task is to conduct an investigation that does not intend to fulfill Koch's postulates, nor to prove that there is a highly significant numerical difference between the results of treatment A and treatment B, but to decide which of the two is more likely to produce a better profitability and has greater practicality in terms of an individual farmer's managemental skills and financial and land resources. Such statistical theorems exist and are used in business and industry. They need adapting to agricultural industry. You will feel the need for such techniques most acutely when you file a grant application and have it refereed by a statistician of classical lineage. In my experience this is one of the most serious impediments in the way of progress I have outlined but it may be my personal experience rather than a general one.
7. Soliciting funds from the relevant industries to support research into them.

Conclusion

My purpose has been to add something to the stature of the veterinary profession and to use the medium of this lecture to be critical of one of its shortcomings. One of the policies I have proposed as a corrective measure is the participation of private practitioners in a planned research program. This is based on the view that this is the only means we have of accumulating a data bank of information on prevalence of individual diseases, outcomes of particular treatments and preventive programs, and the reliability of clinical signs and clinico-pathological tests which is essential if we are to have a detailed knowledge of our area of professional liability.

There is a further motive in proposing that a research activity should be included in a private practitioner's ethical code, that it should be as much an obligation for him/her as it is for the university staff member. If the profession is to escape the criticism that it is a "conspiracy against the laity" as is suggested by the proponents of consumerism, it might well consider additions to its only existing claim to be a self-governing profession, that of abiding by a self-imposed code of ethics which places the needs of the patient/client above those of the practitioner. Obligatory continuing education and self-assessment are obvious ways of doing this, but difficult to implement. However, participation in a program of acquiring new information to the benefit of our clients would be an admirable way of achieving this objective. There is an inclination to decry the contributory motivation of private practitioners and a general reaction to this proposal is unlikely to be favorable. My view is that if a lead was provided, and it must come from university clinical departments, there is sufficient desire amongst practitioners to participate in research programs that will tip the balance of the total output towards the real world.

I appreciate that what I have said, the cause I have espoused, is not a commonly held view. That does not concern me - to be a lone voice - and I am sure it would not have worried Dr. Schofield. I am sure that the diagnosis that I have set out, and the assessment I have made of the causes of our research shortfall are accurate and perhaps you may see some merit in them - especially if you view them not as a criticism of past research policy but as a call for a lift in performance in an area that lags behind the rest, the one that is the veterinary profession's half of the interface with the real world.