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Research in Veterinary Medical Science

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It is my objective to discuss briefly some of the factors that are important to productive research in veterinary medical science. This discussion is directed primarily to those who now are entering research, and those still contemplating the wisdom of their decision to enter research. It also is designed to help the beginning researcher determine his strengths and weaknesses in the field of scientific investigation and to provide a guide in preparing for a research career. I will not get into the philosophical aspects of research, but confine myself to the superficial "what is involved" aspect.

Webster defines research as "studious inquiry or examination; specific and usually critical and exhaustive investigation or experimentation having for its aim the discovery of new facts and their correct interpretation, the revision of accepted conclusions, theories or laws, in the light of newly discovered facts, or the practical applications of such new or revised conclusions." Research is man's search for new knowledge, in its broadest sense. The search may be systematic or it may result from chance coupled with keen observation by the prepared mind. In all events, however, research is conducted in the minds of men. It is not conducted by apparatus, elaborate laboratories, or technicians running analyses. Research is conducted by the human mind, synthesizing, associating, correlating, with the end result of developing new knowledge. This we must understand from the onset.

There is no universal formula which explains how to conduct research. This is true because scientific investigation is an art, i.e., the art of creating new knowledge. It can be attained by many different and equally successful means. However, there are some basic ingredients and principles to good scientific investigation that have been shown pragmatically to be important. I will try to point out some of these factors.

I am not sure how much of the material is original and how much has been drawn from my co-workers and research colleagues throughout the world. I have drawn heavily from Dr. W. I. B. Beveridge's "The Art of Scientific Investigation." This is an excellent scholarly treatise of this subject. I recommend it highly. The writings of Hans Selye, W. B. Cannon, Rene' Dubos, and F. M. Burnet also have been used extensively. I will discuss the research worker, the research and what kind of research.

THE RESEARCH WORKER

What are the qualities in a person that best qualify him to be a productive research worker? This is a question that has concerned educators, research administrators and psychologists for a long time. Much time and effort has been expended in studying the various attributes of research workers in order to see what has made them successful. The job is made easier by the fact that approximately 90% of the scientists that the world has pro-
duced are alive today. One conclusion can be drawn, there is no objective means to evaluate a research worker. We only can subjectively point to certain characteristics that seem to be present in many successful reasearchers.

Natural Attributes

Intelligence.

Scientific investigation is an intellectual pursuit. It requires precise and vast knowledge of many fields. It requires that many ideas be created, tested, and proved to be valid or invalid and accepted or rejected. It requires deep understanding of principles and facts. The ultimate objective of research is the intellectual task of creating new knowledge, hence, one of the most valuable attributes of a research worker is exceptional native intelligence. All research workers need not be geniuses. However, if a person is not highly intelligent, he probably will not do well in research.

Curiosity.

The young of all species are curious. Usually by the time a person reaches adulthood, his curiosity in his environment has been satisfied. A scientist's curiosity is directed to intellectual matters. He is curious about relationships and mechanisms about which there is no satisfactory explanation. Curiosity is one of the most valuable motivating forces of a scientist. Basic to curiosity is the awareness that a problem exists. One learns of a problem by asking how something functions or why a certain result occurs? The non-curious never become aware of problems because they are not obsessed by why or by how. The number of times these words appear in one's consciousness is a measure of one's innate curiosity.

Imagination.

When the mind is aware that a problem exists, this awareness acts as a stimulus for the formation of possible solutions. After a possible solution is developed, it can be subjected to analysis to determine its value. If it is rejected, other solutions may come into consciousness. The ease in which these solutions occur is called imagination. It varies markedly between individuals. Some people are inherently so unimaginative that they seldom get an idea while others bubble over with ideas. i.e., are full of imagination.

Ideas are formed in the mind mainly by associations. Some are very obvious while others are more subtle and hence, more imaginative. The validity and complexity of the solutions are dependent upon the facts stored in the mind. New ideas and associations are more likely to come out of a varied store of memories than a collection of the same kind. A background of facts and problem solving experience in another field, i.e., physics, chemistry and mathematics is helpful to the profusion and nature of ideas that one has. We often look to the before mentioned areas for methodology but their greatest value probably lies in their ability to stimulate the imagination. Fortunately, the imagination of intelligent people can be stimulated by being conscious of “problems” and their solution. One should not neglect this type of mental gymnastics.

Scholarship

A research worker who places a high priority on increasing his knowledge for no other reason than that he wants to know as much as possible generally is much more productive and successful than one who does not. A propensity toward scholarship makes it easier for a researcher to remain a student for a lifetime — which, of course, is indispensable. Scholarship ordinarily is a reflection of environment in the formative years, but it can be cultivated and developed throughout life. One also must have an intense love for science to be a productive researcher. A scientist should obtain deep personal pleasure from participating in scientific investigation.

Tenacity

Research is laborious. Far more experiments are unproductive than ever succeed. Because of this, most successful scientists are characterized by extreme perseverance. Most worthwhile achievements have required persistence and courage in the face of repeated failure. Most
truly great scientists have great internal drive and tenaciousness of purpose. Pasteur said, “Let me tell you the secret that has led me to my goal. My only strength lies in my tenacity.” A research worker must develop a singleness of purpose and an ability to weather the misfortunes of everyday adversity to make significant contributions to science.

Cooperativeness

Meaningful research is seldom the result of the efforts of a person working entirely alone. Many individuals contribute to the synthesis of an idea and many to testing it. A research worker should realize his dependence on others and be as cooperative as possible. Far more progress can be made by all if people working in the same research sphere, anywhere in the world, develop a spirit of helpfulness and confidence. One should not feel that he is giving up valuable ideas because in the long run he probably will receive far more than is given. Avoid senseless races for publication. If one constantly is plagued by other workers publishing material that he is working on, he should alter his research, dig deeper and use a less obvious approach.

One should be cooperative and helpful to other workers in his laboratory. This is a profitable investment because productive ideas do not flow freely if there are tensions and antagonisms among people working together. This is so important that one should place a very high priority on getting along with co-workers. Nothing is so senseless as to let petty jealousies and frictions destroy a productive working unit. This spirit of friendliness and appreciation should extend to technical assistants, secretaries, animal caretakers and service personnel.

If a person’s personality is such that he has trouble getting along with people, he should give special attention to correcting his deficiency. Much of his future accomplishment in research will depend upon how well he and his co-horts can get along. If these traits cannot be corrected, the research worker should get into a situation where he can work alone or into a different type of work.

Modesty

One must be modest regarding his accomplishments. Progress in research is slow and, each of us contributes only a little. Ones co-workers generally are more competent to judge our contributions than we are ourselves. Do not let what you believe to be personal accomplishment give you license to criticize a fellow scientist. You are always free and welcomed to challenge his work, but you have no right to attack the researcher. In general, those researchers who have accomplished the most are the most modest.

Skepticism

A scientific investigator must be skeptical by nature. He must submit each idea to the mental test of reasonableness and all of his conclusions to the test of the experimental method. Only by maintaining an objective and skeptical mind can progress be made. The key is submitting all things to the test of reasonableness no matter what the source. Good tests for any alleged fact is, “what is the evidence?”, “what are other reasonable alternatives?”, and “why must this be true?”. Do not be afraid to ask, “why.” If one does not often feel compelled to ask “why,” he should wonder about his fitness for research.

Scientific Training

Undergraduate education generally is the foundation for a scientific career. In the medical sciences, however, veterinary, medical and dental education is a mixture of basic and applied science. Most veterinarians, physicians and dentists should have more training, particularly in the pure sciences, if they choose careers in research.

Graduate education is a guided tour to the frontier of knowledge in a narrow area with practice of the art of scientific investigation. It is training for research in one of the specialties. It is different training than professional education — not necessarily more or less advanced or with more or less status. The tools for research are different from those needed for practice, hence, graduate training is needed. There are two important aspects of graduate training: (1) the accumu
lation of facts which are necessary to formulate and test ideas, (2) conducting research under the close supervision of an expert; the major professor. Graduate education also serves as a testing period to determine whether or not a person is qualified for a career of scientific investigation.

Post-doctoral education consists of doing research under the guidance of an active and accomplished man in the field. Post-doctorals are the rule for Ph.D.'s in most basic biomedical sciences today. They also are the rule for M.D.'s who go into research. This training mechanism is becoming more popular in veterinary medicine.

Because research is an intellectual pursuit it is necessary that the mind continually be fed with the facts from which creative ideas evolve. The most successful research workers are students during their entire productive lives. This means that one must stay abreast with the developments in his research specialty as well as the major developments in science. This is a very difficult and time consuming task. It means that each of us must read deeply or superficially each issue of 10 to 50 research journals and 5 to 10 books per year. If we do not do this, we quickly fall behind and no longer are able to create meaningful new knowledge in our specialty.

An active research worker should attend and participate in the major research meetings of his specialty to hear the reports of research given personally by contemporary research leaders, give research reports and enter into discussions. He also should attend and participate routinely in lectures, seminars and discussion periods within his specialty. Periodic leaves of absence such as Sabbaticals should be utilized to spend 6 months to a year in an active research laboratory in an important research center. This will help one keep up-to-date and also remotivate a person who has tended to get into a rut.

THE RESEARCH

The process by which new knowledge is created varies with the investigator, the problem and the research center in which it is conducted. The course followed must be varied to meet specific situations, however, one's chances of success are increased markedly if some organized plan is followed. I will discuss a commonly used scheme composed of the following steps: (1) selection of a problem, (2) preparation for the solution, (3) development of the hypothesis, (4) testing the hypothesis, (5) collecting the data, (6) synthesis of the solution and (7) communication of the results.

Selecting a Problem

The matter of selecting a research problem is something that most research workers do only once or at best a few times in a lifetime. Usually it is done when one selects the subject of his doctoral dissertation. Because good research work uncovers many additional unanswered questions, a productive researcher often spends a lifetime working in the general area in which he did his doctoral research. It is important, therefore, to choose a problem carefully.

It is well established that if a research worker is mainly responsible for choosing his research project, he is more likely to be successful. It is not always possible for all of us to have a limitless choice of problems because many of us function in applied research organizations with definite responsibilities. However, no matter what subject is assigned, there usually is almost unlimited latitude to select a problem within the subject. Generally only the disease or problem is assigned, not the exact research.

A curious research worker generally has no difficulty in selecting a suitable problem, because so much is missing in our knowledge of so many things. It is often wise to choose a problem involving commonplace diseases and things because material is much more available. A new research worker should select a problem within the general sphere of the research interest of senior researchers in the laboratory in which he is working. He will need infinite guidance and this can be done well only by people actively engaged in research on the same general problem. I
would consider this to be so important that it should be considered mandatory for doctoral research.

One should select a problem which falls within his specialty or the specialty that he is developing. He should never make the mistake of choosing an area of research for which he is ill prepared.

Preparation for the project

After a problem has been selected, the next step is to determine as far as possible what is known about the problem. One first starts with textbooks and review articles until he is intimately acquainted with the general nature of the problem. Next, he should read the most important, if not all the research publications on the subject. Now he should discuss the problem with experienced researchers in his laboratory, and if none are present, with other active researchers in this field. If feasible, i.e., if this is a disease state, the next step should be to collect as much information on the syndrome as it occurs naturally. Get out on farms and observe occurrences of the disease, talk to farmers, practicing veterinarians, and others who are familiar with it. Do not minimize the importance of observations made by lay personnel, and consider them with proper skepticisms as you would even your own observations.

During this stage of preparation, one's mind should be taking in specific facts and storing them away for future use. In addition, the mind should be submitting each fact to the test of reasonableness and looking for gaps or inconsistencies in the knowledge of the problem. Also, the researcher should try to determine what areas might be the most fruitful to investigate.

The investigator should avoid a premature plunge into testing ideas, remembering that the success of the study will depend to a great extent upon the thoroughness and care of the preparation. Next, one should resolve the particular problem into crucial questions and then design experiments to answer these questions. A crucial experiment is one which gives a result consistent with one hypothesis and inconsistent with another.

A quotation describing Charles Nicolle's careful preparation for an experiment attributed to Hans Zinser by Beveridge points out the importance of careful preparation to productive research. "Nicolle was one of these men who achieve their successes by long preliminary thought before an experiment is formulated, rather than by frantic and often ill-conceived experimental activities that keep lesser men in ant-like agitation. Indeed, I have often thought of ants in observing the quantity of 'what-of-it' literature from many laboratories. Nicolle did relatively few and simple experiments, but every time he did one, it was the result of long hours of intellectual incubation during which all possible variants had been considered and were allowed for in the final tests. Then he went straight to the point without wasted motion. That was the method of Pasteur, as it has been of all the really great men of our calling, whose simple conclusive experiments are a joy to those able to appreciate them."

It is sad, but never-the-less true, that so many present day research workers are so busy running analyses and collecting data that there is little time left for research.

Development of a hypothesis

A hypothesis is a supposition that if true will explain the occurrence of a given set of facts or phenomena. It is a problem boiled down to a single question. Its function is to indicate new experiments and observations. It is a useful tool in research even when proved wrong because often the true facts result from the experiments that disproved the hypothesis. The literature is full of examples of important discoveries that have resulted from disproved hypotheses.

The use of a hypothesis is quite necessary to productive research. It is a necessary tool in setting up experiments that will provide answers not just more observations. One should devise an experiment to prove or disprove a hypothesis and reap the additional harvest of new facts that are uncovered. Do not constantly do something merely "to see what happens."
One should be slow in accepting an idea as a hypothesis until it has been given the most careful scrutiny. The danger is that once a researcher sets out to prove or disprove a hypothesis, he unconsciously identifies himself with the hypothesis and it is more difficult to think of alternatives. Furthermore, there is danger that one may become unconsciously biased because he created the hypothesis. When one's evidence is sufficient to show that a hypothesis is not valid, he should not be slow in abandoning it. The best possible position at all times is to remain as objective as possible about your hypotheses. Try not to identify yourself with the possible outcome.

Testing the hypothesis

The controlled experiment is one of the most important concepts of research. It is composed of two groups, the principals and the controls. They are assembled by randomization and should be as similar as biological material can be except for the variable to be tested for. This is the usual form of the productive experiment. However, if statistics are utilized fully, several variables can be tested at one time. The controlled experiment is the basis for most biological research. The real problem arises when one tries to limit the variables. It is the essence of an experiment that it should be reproducible. If it is not, it is of no great significance.

In setting out on a project, it is best to first conduct a few simple crucial experiments to prove or disprove the main hypothesis. In a study of an unknown disease, one would want to conduct a simple transmission study before trying to isolate a causative agent. If one were studying a toxic agent, he would want to bracket the response first by low, intermediate and high doses with a few animals before an elaborate experiment with many animals is conducted. If one has a new drug, he might want to quickly screen it against many infections before conducting detailed studies on any one. In any event, plan the experiment so that it will give a useful result. Never perform an ill-planned experiment because you will be tempted to believe it. Never conduct the preliminary or sketchy experiment except as a prelude to a more detailed experiment.

In conducting an experiment, it is of extreme importance to take great care with the essentials of the procedure. However, do not waste time with unnecessary details on the unimportant aspects of the work. Research protocols are important aids in experimental design. One should work out all the details in the protocols before embarking on the experiment.

You should carefully record all details of an experiment. One often needs details that he did not think important at the time the experiment was conducted. Be sure to record in detail the findings that run contrary to your hypothesis. It is easier to unconsciously forget negative findings than positive ones. Note taking is also helpful in that it forces careful observation.

The researcher must be completely familiar with technical methods before he uses them. He must realize their limitations and accuracy as well as what they actually measure. Most methods sometimes go wrong and give misleading results and the experimenter must be able to detect trouble immediately. Crucial measurements should be made repeatedly and if possible, by a second method. Avoid technique fads. Do not conduct a study merely to use a certain technique. How often have we heard people say, we have to conduct studies with electrophoresis, electron microscopy or tissue culture? Techniques merely extend the researchers powers of observation. It is naive to consider methodology as the research itself. Finally, one must understand that experimentation is not infallible. If one is not able to demonstrate a supposition experimentally, this does not prove that it is incorrect.

Collecting the data

After a hypothesis has been established and an experiment set up to determine its validity, one must collect data that is sufficiently valid to prove or disprove the hypothesis. Ordinarily we insure the validity of these data by making objective measurements and we check and recheck.

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our results. We repeat the experiment. We may use an alternative method. Generally, the good research worker using all these things is able to collect reliable data. From the data he must determine whether or not the hypothesis has been proved.

The value of an experiment is twofold. In addition to proving or disproving a hypothesis it provides an opportunity for the scientist to make observations on other happenings with his experimental system. It is in how we capitalize on this bonus of information that separates the keen researcher from the common garden variety of scientist. Psychologists have pointed out the inherent difficulty and inaccuracy of human observations. Every court trial in which witnesses attest to the facts of a legal case is a living example of the frailty of human observational ability. This is an area in which we must condition our senses to be more active and receptive.

One cannot observe everything closely, therefore, one must try to select the significant. As Alan Gregg of the Rockefeller Foundation has said, “Most of the knowledge and much of the genius of the research worker lie behind his selection of what is worth observing. It is a crucial choice often determining the success or failure of months of work, often differentiating the brilliant discoverer from the plodder.” One should train himself in the techniques used, the animals, and the syndrome studied so that he will be able to observe both the expected and even more important, the unexpected, with a great deal of accuracy.

Synthesis of the solution

After all the observations have been made and all the data collected, if you are lucky you can determine whether or not your hypothesis was proved or disproved. If the results are positive you undoubtedly will want to repeat the experiment, probably several times. You also may want to prove your hypothesis by an alternative method. If the hypothesis was disproved by the experiment, you may want to repeat the experiment or perhaps vary the procedure somewhat to better conform to some of the results of the completed experiment. Do not be too hasty to abandon a hypothesis except, of course, if further observations prove it to be untenable.

You now will want to begin the process over again by developing another hypothesis, proving it, and further adding to the solution of your problem.

Communication of the results

Research is not completed research until it has been communicated. In most cases this means published in a research journal. Until an idea has been tested by experimentation and then placed before the world to be criticized and substantiated or disproved it is not new knowledge created. One should plan every experiment with the thought that someday part of this will be published. Until it is published, research is the playing of the researcher; after publication it is the property of everyone.

One must not think that every experiment conducted is worthy of publication. Publish sparingly but publish all valuable experimental data. Remember that once a paper is published it remains published forever. Be sure that you will be proud to refer to a paper you have published at a later date.

The Role of Chance in Research

Chance plays an important role in research. Many of the important discoveries in biology and medicine might be classified as unexpected or chance discoveries. Beveridge lists 19 illustrations of important chance discoveries in his book. It is important for all of us to realize how important chance is to research. However, it is equally important to realize that this is only partly true because chance must be coupled with the prepared mind of the researcher. As Pasteur said, “Chance favors the prepared mind.” Chance or accident affords the opportunity. The scientist must recognize it and give it significance by relating it to other knowledge. One can prepare himself to take the fullest advantage of chance or accidental happenings.

WHAT KIND OF RESEARCH?

At the onset of a research career, one
wonders what kind of research he should engage in? What will be the most profitable line to follow? These questions can only be answered by the researcher himself. In general, however, it doesn't really matter so long as one works in an area in which he is intensely interested and works at the frontier of his field. If this is done, there is no doubt that one has a high likelihood of being successful. To be on the frontier one must conduct really deep and searching research. He cannot be satisfied just to fill in the gaps. He must create real steps forward.

**Basic or Applied Research**

Many new researchers wonder whether they should participate in basic research or applied research. This is not as important a decision as one might think because there really is little fundamental difference in basic or applied research. Research is basic if it has no foreseeable application or usefulness. It is applied if it is done primarily because it may in some way help solve a problem. If this is true, whether or not research is basic or applied depends upon the intellect of the researcher as much as on the research. Early workers in the area of atomic physics may or may not have visualized atomic power plants. Were they conducting basic or applied research? Dr. Hans Selye covered this subject very ably in the Saturday Evening Post's Adventure of the Mind series "What makes Basic Research Basic?"

**Descriptive or Mechanistic**

In the biomedical sciences research may be divided into two very large categories; descriptive, and mechanistic or quantitative. The former describes the occurrence of various phenomena while quantitative research determines and measures how things happen. Most segments of biology and medicine have passed through the period when most of their research is descriptive and now are fully occupied with the determination of mechanisms and quantitating these systems. Because research in veterinary medical science is younger and fewer research workers have been available to work on many diseases, we are in many areas still in the midst of describing various syndromes and disease states. This must be done, but we should also get about the business of quantitative research as rapidly as possible. Let us not be shackled by the habit of describing, let us move into research on mechanisms and describe as we go along.

**Veterinary Medical — or Medical Research?**

A young man beginning a career in biomedical research may well ask which branch should he enter—veterinary medical, human medical or purely biological research. This is a question that is very difficult to answer because there is no clear line where one branch leaves off and the other begins. The principles of physiology, pharmacology, microbiology, immunology and pathology are essentially the same in man as they are in other animals. Even when dealing with specific disease problems, it is difficult to separate veterinary medical from human medical research. As Sir William Osler said, "There is only one medicine." Medical research may be animal oriented or people oriented. That is, it may be directed toward application to animals or to man. Only at this stage is there any difference in veterinary medical and human medical research.

Some of the greatest future opportunities in biomedical research will be open to the well prepared veterinary scientist. The training in basic and clinical sciences required of the veterinarian will better equip him for many kinds of research in the biomedical sphere than the more specialized training of the biologist provided, of course, the veterinarian is trained for research particularly in the pure sciences. The intimate knowledge of many kinds of animals provides the veterinarian with many advantages over the physician in research. I am sure that a realization of the tremendous future in research for the veterinarian has attracted many research minded biologists to veterinary medicine. With the problems of men, money and facilities now capable of solution, I predict that within a decade veterinary researchers will be recognized as leaders in many aspects of biomedical research.

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Ethics of Research

There are certain ethical considerations that seldom are discussed but are understood among scientists. I will mention only a few.

The author of a publication is under an obligation to give due credit to anyone who has helped in the investigation. All types of assistance should be acknowledged in appropriate footnotes. If there is any question in your mind regarding co-authorship of a paper which you are the principal author, err on the side of adding a co-author. If your name is included as a co-author of a paper and you know you did not contribute to the extent that should justify co-authorship, it is obligatory for you to remove your name as an author. It is unethical to accept co-authorship if one has not made an adequate contribution.

The author of a publication is under an obligation to cite pertinent references to research on the subject of a research report. Furthermore, as a scientist he is under an obligation to be aware of all published research on the subject of a research paper. It is the gravest of errors to fail to discover and cite, if appropriate other published research in the area of the research reported. One also is under an obligation to cite the research of another scientist in its most favorable light. It is a very serious error to misrepresent the reports of another author or not to cite them in their best light. This, I am sorry to say, is a very commonly committed unethical practice.

It is a serious breach of ethics to steal someone’s idea and to work on it and report it without permission to do so. This is out and out thievery, but it frequently occurs. It often results from an innocent visit to a laboratory where work in progress is discussed in detail. Later, because of poor memory, a scientist may innocently think he thought of some of the ideas he picked up in the course of his visit. If you are one who likes to visit laboratories, be sure you do not leave with ideas that you later may unintentionally consider your own.

It is unethical to draw from the great pool of accumulated scientific knowledge if one does not contribute to the pool by publishing the results of his own research. Research that is not published is unfinished research and wasted for all time. All scientists are under a moral obligation to communicate the fruits of their research by publication in a proper scientific journal. One should never, under any circumstances, publish research that is unworthy of publication. It is well to remember that the scientific community weights publications by the quantity of thought they contain rather than the weight of the paper on which the publication is made. Respect your fellow scientists by condensing all publications as much as possible.

One should not present the evidence for his own conclusions only in their best light because the truth will eventually appear anyway. It is much better to objectively weigh the probity of your evidence and point out weak points in your proof as well as the strong points. If a researcher publishes a paper and later learns that a mistake has been made, he should publish a correction to help others. It is much better for the perpetrator of the error to point it out rather than to have it done by someone else, which is inevitable.

A scientist has no moral right to question the motives of a fellow scientist. He may question his knowledge, his facts and the validity of his conclusions, but it is essential to the spirit of science that the motives of ones fellow worker not be open to question. You may think privately that your colleague is a fool; however, you should not permit yourself to think that he is a knave. The few who mask skulduggery as research soon are passed by the march of science and their efforts have no influence on scientific progress.

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