**TYPES OF RESEARCH-Lecture 9**

Research is broadly classified into two main classes:

1. Fundamental or basic research
2. Applied research

**A.** Basic Research

Basic research is an investigation on basic principles and reasons for occurrence of a particular event or process or phenomenon. It is also called *theoretical research*. Study or investigation of some natural phenomenon or relating to pure science are termed as *basic research*. Basic researches some times may not lead to immediate use or application. It is not concerned with solving any practical problems of immediate interest. But it is original or basic in character. It provides a systematic and deep insight into a problem and facilitates extraction of scientific and logical explanation and conclusion on it. It helps build new frontiers of knowledge. The outcomes of basic research form the basis for many applied research. Researchers working on applied research have to make use of the outcomes of basic research and explore the utility of them.

Research on improving a theory or a method is also referred as fundamental research. For example, suppose a theory is applicable to a system provided the system satisfies certain specific conditions. Modifying the theory to apply it to a general situation is a basic research.

Attempts to find answers to the following questions actually form basic research.

* Why are materials like that?
* What are they?
* How does a crystal melt?
* Why is sound produced when water is heated?
* Why do we feel difficult when walking on seashore?
* Why are birds arrange them in ‘*>*’ shape when flying in a group?

Fundamental research leads to a new theory or a new property of matter or even the existence of a new matter, the knowledge of which has not been known or reported earlier. For example, fundamental research on

1. astronomy may leads to identification of new planets or stars in our galaxy,
2. elementary particles results in identification of new particles,
3. complex functions may leads to new patterns or new properties associated with them,
4. differential equations results in new types of solutions or new properties of solutions not known so far,
5. chemical reactions leads to development of new compounds, new properties of chemicals, mechanism of chemicals reactions, etc.,
6. medicinal chemistry leads to an understanding of physiological action of various chemicals and drugs,
7. structure, contents and functioning of various parts of human body helps us identify the basis for certain diseases.

**B.** Applied Research

In an *applied research* one solves certain problems employing well known and accepted theories and principles. Most of the experimental research, case studies and inter-disciplinary research are essentially applied research. Applied research is helpful for basic research. A research, the outcome of which has immediate application is also termed as *applied research*. Such a research is of practical use to current activity. For example, research on social problems have immediate use. Applied research is concerned with actual life research such as research on increasing efficiency of a machine, increasing gain factor of production of a material, pollution control, preparing vaccination for a disease, etc. Obviously, they have immediate potential applications.

Some of the differences between basic and applied research are summarized in table I. Thus, the central aim of applied research is to find a solution for a practical problem which warrants solution for immediate use, whereas basic research is directed towards finding information that has broad base of applications and thus add new information to the already existing scientific knowledge.

TABLE I: Differences between basic and applied researches.

|  |  |
| --- | --- |
| *Basic research* | *Applied research* |
| Seeks generalization | Studies individual or specific cases without the objective to generalize |
| Aims at basic processes | Aims at any variable which makes the desired difference |
| Attempts to explain why things happen | Tries to say how things can be changed |
| Tries to get all the facts Tries to correct the facts which are problematicReports in technical language of the topic Reports in common language |

**C.** Normal and Revolutionary Researches

Basic and applied researches are generally of two kinds: *normal research* and *revolutionary research*. In any particular field, normal research is performed in accordance with a set of rules, concepts and procedures called a *paradigm*, which is well accepted by the scientists working in that field. Normal research is something like puzzle-solving: interesting, even beautiful, solutions are found but the rules are remain same. In this normal research sometimes unexpected novel results and discoveries are realized which are inconsistent with the existing paradigm. Among the scientist, a tense situation then ensues, which increases in intensity until a scientific revolution is reached. This is marked by a *paradigm shift* and a new paradigm emerges under which normal scientific activity can be resumed.

**D.** Quantitative and Qualitative Methods

The basic and applied researches can be *quantitative* or *qualitative* or even both. Quantitative research is based on the measurement of quantity or amount. Here a process is expressed or described in terms of one or more quantities. The result of this research is essentially a number or a set of numbers. Some of the characteristics of qualitative research/method are:

* It is numerical, non-descriptive, applies statistics or mathematics and uses numbers.
* It is an iterative process whereby evidence is evaluated.
* The results are often presented in tables and graphs.
* It is conclusive.
* It investigates the *what*, *where* and *when* of decision making.

Statistics is the most widely used branch of mathematics in quantitative research. It finds applications not only in physical sciences but also in economics, social sciences and biology. Quantitative research using statistical methods often begins with the collection of data based on a theory or hypothesis or experiment followed by the application of descriptive or inferential statistical methods.

Qualitative research is concerned with qualitative phenomenon involving quality. Some of the characteristics of qualitative research/method are:

* It is non-numerical, descriptive, applies reasoning and uses words.
* Its aim is to get the meaning, feeling and describe the situation.
* Qualitative data cannot be graphed.
* It is exploratory.
* It investigates the *why* and *how* of decision making.

We measure and weigh things in the study of substance or structure. Can we measure or weigh patterns? We cannot measure or weigh patterns. But to study patterns we must map a configuration of relationships. That is, structures involve quantities whereas patterns involve qualities. If one wishes to investigate why certain data are random then it is a qualitative research. If the aim is to study how random the data is, what is the mean, variance and distribution function then it becomes quantitative. Explaining how digestion of food takes place in our body is a qualitative description. It does not involve any numbers or data and quantities.

The detection of a particular compound is a qualitative analysis. This can be done by carrying out physical or chemical tests. Determination of exact amount of a particular compound present in a volume is essentially quantitative analysis. This can be done by volumetric, gravimetric and colorimetric methods or instrumental methods. Experimental and simulation studies are generally quantitative research.

In fact, qualitative methods can be used to understand the meaning of the numbers obtained by quantitative methods.

**E.** Other Types of Research

Other types of research include *action research* (fact findings to improve the quality of action in the social world), *explanatory research* (searching explanations for events and phenomena, for example finding answer to the question why are the things like what they are?), *exploratory research* (getting more information on a topic) and *comparative research* (obtaining similarities and differences between events, methods, techniques, etc.). For discussion on these types of research see refs.[3–5].



**Assignment:**

1. List out at least 10 theoretical and applied methods which you have learned in your UG, PG courses and write their features in two or three sentences.
2. Write at least 20 questions in your subject the investigation of which forms basic research. Then point out how many of them have already been solved and how many were found in applications.
3. Distinguish between theory and experiment.
4. Write a note on importance of theory in basic and applied researches.
5. Bring out the importance of inter-disciplinary research.



**IV. ENTERING INTO RESERCH**

*How do you enter into a research career?*

There are many ways to enter and start a research career. In India, one popular path is to appear for the National Eligible Test (NET) conducted by the National Education Testing Bureau of the University Grants Commission (UGC). This test is conducted twice in a year generally in June and December. The NET is conducted in humanities, languages, social sciences, forensic science, environmental sciences, computer science and applications and electronics. The Council of Scientific and Industrial Research (CSIR) conducts the UGC–CSIR NET for science subjects like mathematical, physical, chemical, life, earth, atmospheric, ocean and planetary sciences–jointly with the UGC.

One of the prime objectives of the NET is to ensure minimum standards for the entrants in the research. Those who have at least 55 percent of marks in their postgraduate degree are eligible for writing the test. Those who are appearing for the final-year qualifying examination can also apply for the test under the Result Awaited category. Age limit for JRF is 28 years. The upper age limit may be relaxed up to five years for SC/ST/OBC/PH and female applicants. For more details, visit www.csirhrdg.res.in.

Passing the test means one is eligible for the award of Juniour Research Fellowship (JRF) for a period of five years in a university or a research institution or a college.

Research facilities are availble in research institutions and CSIR laboratories for those who got good grades in the Graduate Aptitude Test in Engineering (GATE) conducted by the Indian Institutes of Technology (IITs). There is another possible path to enter research. Scientists working in research and educational institutes prepare research proposal and submit to government agencies like Department of Science and Technology (DST), CSIR, UGC, Department of Atomic Energy (DAE), National Board for Higher Mathematics (NBHM), etc. Generally, JRF and other higher fellowships are proposed by the proposer to carry out the proposed research work. Once the proposal is approved then advertisement will be given in newspapers to apply for the research fellowships. Many universities also provide limited number of fellowships. In the above routes a researcher will get fellowship to do research. Without fellowship also one can start a research career. However, since research period for Ph.D. degree is generally a 4–6 years of work, it is not advisable to start a research life without a fellowship.

**V. VARIOUS STAGES OF A RESEARCH**

Whenever a scientific problem is to be solved there are several important steps to follow. The problem must be stated clearly, including any simplifying assumptions. Then develop a mathematical statement of the problem. This process may involve use of one or more mathematical procedures. Frequently, more advanced text books or review articles will be needed to learn about the techniques and procedures. Next, the results have to be interpreted to arrive at a decision. This will require experience and an understanding of the situation in which the problem is embedded. A general set of sequential components of research is the following:

1. Selection of a research topic
2. Definition of a research problem
3. Literature survey and reference collection
4. Assessment of current status of the topic chosen
5. Formulation of hypotheses
6. Research design
7. Actual investigation
8. Data analysis
9. Interpretation of result
10. Report

In the following sections the above mentioned various stages of research are discussed in detail.

**VI. SELECTION OF A RESEARCH TOPIC AND PROBLEM**

The starting point of a research is the selection of a research topic and problem. History teaches the continuity of the development and progress of science. The point is that every age has its own problems, numerous in number, which the following age either solves or casts aside as profitless and replaces by new one. If we could obtain an idea of the probable development of scientific knowledge in the immediate future, we must let the unsettled questions pass before our minds and look over the problems which the science of today sets and whose solution we expect from the the near future. The deep significance of certain problems for the advancement of science and society must be taken into consideration in choosing a problem of research.

There are many ways to do research as there are scientists. The choice of a thesis research area and adviser has always been a very subjective process. Identifying a suitable topic for work is one of the most difficult parts of a research. Before choosing a research topic and a problem the young researchers should keep the following points in mind.

* Topic should be suitable for research.
* The researcher should have interest in it.
* Topic should not be chosen by compulsion from some one else.

Topic and problem can be fixed in consultation with the research supervisor. In our country often research supervisors suggest a topic and state a problem in broad view. The researcher has to narrow it and define it in an operational form. One may ask: Is it necessary that the topic of a Ph.D. should be different from M.Sc. project and M.Phil dissertation? The answer is not necessary. If a student is able to get a supervisor working in his M.Sc.project or M.Phil dissertation topic then it would save about six months in the duration of his Ph.D. work.

**A.** Can a Researcher Choose a Topic by himself?

A youngster interested to start a research career wishes to know whether he/she has freedom to do research in the topic of his/her own interest. The style of research in our country and various other factors like the infrastructure facility available in a research institute, time limit, our commitment to family and social set up hardly allow a young researcher to choose a topic by himself for his PG project, M.Phil. dissertation and Ph.D. thesis. However, many research supervisors give complete freedom to choose a problem in the topic suggested by him for a Ph.D. research work. Because the normal time duration of M.Phil dissertation is about 6-8 months, it is better to work on the problem suggested by the supervisor.

If a student wishes to do research (for Ph.D. degree) with fellowship then he cannot have freedom to choose a topic since he has to work on a project the goal of which is already defined by the project investigator. In the other way, after choosing a topic of his own interest he has to find a supervisor who is working in that topic or interested in guiding him. In this case one has severe limitation in our country for getting a fellowship and for registering for a research degree. If a student is not very much particular about the fellowship he has a chance to do research in the topic of his own interest. A researcher in India after two years of research experience with few (two or more) publications can apply for a senior research fellowship (SRF) to CSIR (for details see its and other relevant web sites). He can prepare a project under the direction of his Ph.D. supervisor which can lead to a fellowship. For details see the book *How to Get Scholarships, Fellows and Stipends* by K.D. Kalaskar (Sultan Chand and Sons, New Delhi).

Considering the above, a researcher should make-up his mind so as to work in a topic suggested by the supervisor. However, a research problem may be chosen by a researcher himself. This has several advantages. In this case

* the researcher can pursue his/her own interest to the farthest limits,
* there is an opportunity to spend a long time on something that is a continuous source of his pleasure and
* the results would prove better in terms of the growth of the investigator and the quality of the work.

If the researcher is not interested in the topic and problem assigned to him but he is working on it because of supervisor’s compulsion, then he will not be able to face and overcome the obstacles which come at every stage in research.

**B.** Identification of a Research Topic and Problems

Some sources of identification of a research topic and problems are the following:

1. Theory of one’s own interest
2. Daily problems
3. Technological changes
4. Recent trends
5. Unexplored areas
6. Discussion with experts and research supervisor

Suppose one is interested in the theory of nonlinear differential equations or quasicrystals or fullerenes. Then he can find a research guide who is working in this field or interested to work in this field and then choose a problem for research.

Our daily experiences and day to affairs have rich openings on various aspects such as the daunting tasks of AIDS, air pollution, afforestation and deforestation, child labor, problems of aged citizens, etc.

Technology in various branches of science, business and marketing changes rapidly. For example, in the early years, computers were built in larger size with vacuum tubes. Then evolution in electronic technology replaced them by integrated circuits. Recently, scientists have developed quantum dots. Now the interest is in developing efficient, super-fast and miniaturized computing machine made up of material whose particle size of the order of nano (10−9) meter or even smaller. Similarly, another fascinating topic namely, *thin film* has multiple fields of applications. Recent research on fullerenes resulted in many practical applications.

Choosing a topic of current interest or recent trends provides bright and promising opportunities for young researchers to get post-doctoral fellowship, position in leading institutions in our nation and abroad.

In each subject there are several topics which are not explored in detail even though the topic was considered by scientists long time ago. For example, string theory, quantum computing, nano-particles, quantum cloning and quantum cryptography and gene immunology are fascinating topics and are in preliminary stages.

The supervisors and experts are working on one or few fields over a long time and they are the specialists in the field considered and well versed with the development and current status of the field. Therefore, a young researcher can make use of their expertise in knowing various possible problems in the topic the solving of which provide better opportunities in all aspects.

Don’t choose a topic simply because it is fascinating. In choosing a topic one should take care of the possibility of data collection, quantity of gain, breadth of the topic and so on. The topic should not be too narrow. For example, the study of social status and sexual life of married couples of same sex (man-man marriage and woman-woman marriage) is interesting and of social relevance. But the intricate problem here is that we do not find enough number of such couples to study. This is a very narrow topic at the same time we will not get enough data to analyze. On the other hand, the changes in the social life of aravanis in recent times is a valuable social problem and one can collect enough data.

Further, one has to study advanced level text books and latest research articles to identify problems. Is it necessary to know all the methods, techniques, concepts in a research topic before identifying a problem for investigation? This is not necessary. After learning some fundamental concepts, recent developments and current trends of a topic, one can identify a problem for research. Then he can learn the tools necessary to solve it.

**C.** Definition and Formulation of a Problem

After identifying a problem, in order to solve it, it has to be defined and formulated properly. For this purpose, one can execute the following.

* State the problem in questionnaire form or in an equivalent form
* Specify the problem in detail and in precise terms
* List the assumptions made
* Remove the ambiguities, if any, in the statement of the problem
* Examine the feasibility of a particular solution

Defining the problem is more important than its solution. It is a crucial part of the research study and should not be defined in hurry.

**D.** How do you Asses Whether the Defined Problem as a Good Problem?

A problem in its first definition may not be appealing. It may require redefinition in order to make it a good problem. That is, by suitably rewording or reformulating the chosen problem, it can be made to meet the criteria of a good problem. This is also important to solve the problem successfully. To this end a researcher can ask a series of questions on the problem. Some are:

1. Is the problem really interesting to him and to the scientific community?
2. Is the problem significant to the present status of the topic?
3. Is there sufficient supervision/guidance?
4. Can the problem be solved in the required time frame?
5. Are the necessary equipments, adequate library and computational facilities, etc. available?

If the answers to these questions are satisfactory, then the researcher can initiate work on the chosen problem. In addition, discuss the problem with the current doctoral students and obtain the scope of the problem and other related aspects.

**E.** How are these Questions Important and Relevant to a Researcher?

The researcher should be interested on the problem for the reasons mentioned earlier at the end of the Sec.(VIA). The problem should also be interesting to the supervisor so that the researcher can get the necessary guidance from him. Otherwise sometimes the researcher may find it very difficult to convince the supervisor on the importance and significance of the results obtained. More importantly, the problem must be of interest to scientific community and society. If not then the researcher will find great difficulty to publish his findings in reputed journals and convince the funding agency.

Next, the status of the problem, particularly the importance of finding its solution should match with the current status of the field. But, if the problem investigated is of not much interest to science and society then publications will become useless to him in his research career. Specifically, they cannot help earn a post-doctoral fellowship, respectability and a permanent job in an institution.

A researcher needs proper guidance and encouragement from the supervisor regularly. This is important for keeping the research in right track, to overcome the difficulties which come at various states of research and also to have moral support. A researcher should avoid working under the guidance of a supervisor having serious health problems or family problems, committed his large time to administrative work and strong involvement in nonacademic matters.

The following story was told by S.L. Glashow (Harvard University) [Julian Schwinger:

The Physicist, the Teacher, and the Man. (Ed.) Y. Jack Ng, World Scientific, Singapore, 1996, pp.155]:

Once upon a time, a fox came upon a rabbit who was typing away in the middle of the forest. *What do you think you are doing?* asked the fox. *I am writing my thesis on how rabbits eat foxes* replied the rabbit. *Nonsense*! said the fox, *rabbits don’t eat foxes; foxes eat rabbits*. *Just take a peek in my cave* challenged the rabbit. The fox entered the rabbit’s cave and was never seen again. Some time, later a wolf came to the rabbit, who was still writing his thesis. *What do you thing you are doing?* said the wolf, and a similar interchange took place. The wolf entered the cave and was never seen again. Finally, a bear came to chat with the rabbit. *I am writing my thesis on how rabbits eat bears* said the rabbit. *Nonsense*! growled the bear, who was sent to the cave never to be seen again. A wise owl watched these strange goings-on and was puzzled. Softly sneaking into the rabbits cave, he came upon a neat pile of fox bones. A bit further on, he discovered a neat pile of wolf bones. Finally, at the back of the cave behind a neat pile of bear bones, sat a very fat and satisfied lion picking his teeth with a bear claw. The moral of the story is that *it really doesn’t matter what your thesis subject is. What counts is your choice of an advisor*.

An important point is that before initiating research work on a problem, a rough estimate on costs and time required to complete the work must be made. A problem suitable for Ph.D. degree should not be taken for M.Phil. degree. A problem suitable for M.Phil. degree is not appropriate for Master’s degree. If the collection of data or resources or related information takes many years, then the topic is obviously inappropriate for Ph.D. degree. Controversial subjects should not be chosen. Problems that are too narrow or too vague should be avoided.

Finally, the researcher must make sure that the necessary experimental setup and materials to perform the actual research work are available in the department where research work is to be carried out. Without these, if the researcher initiated the work and has gone through certain stages of work or spent one or two years in the problem then in order to complete the task he would be forced to buy the materials and instruments from his personal savings.