Review Article

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On Deciding a Research Problem

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Abstract

This article describes in simple terms the meaning of research, difficulties faced while selecting a research problem, sources of research problems and factors to be kept in mind while finalizing a research topic. Though the world is full of problems, selecting a problem for academic research is a challenging task. It is the selection of the area and the topic that decide the success of the entire research work. Experience has shown that hurried finalization has cost time, energy and money on the part of research scholars. The author has shared her own personal experiences she had while deciding to work on a research topic. The crucial step in any academic research is deciding on the problem. It is expected that this article will be helpful to those interested in carrying out any academic research done for the sake of getting degrees.

Keywords: Research; Theories; Replication; Problem for research

Introduction

Human brain is endowed with unlimited probing capacity. Sky is the end to our imagination and ability for enquiry. From the barbaric life of the nomadic pre-historic man, human beings have evolved into technologically equipped and hence superior species. There is no gainsaying that man has conquered the earlier barriers of space, time and distance. All this is possible because of constant enquiring nature of complicated and advanced brain functions unique to humans.

When this enquiry is done systematically, diligently, intelligently, scientifically, consciously and continuously it becomes research. 'Research' as a word has French origin and not a combination of the prefix 're' and 'search' as many believe; it is like the words 'rehearsal' and 'resemble'.

Research implies asking of a series of questions such as 'what', 'why', 'how', 'when', 'where', attempting to find answers in the form of descriptions and explanations resulting in the generation of new knowledge and verifying the knowledge generated. Research begins and ends with observation. The initial observation leads to assumptions, presumptions and hypotheses whereas the final observation is verification for confirmation.

We are familiar with the specific characteristics of research which are objectivity, precision, verification, offering parsimonious explanations, generalizabilty, scientific nature and pursuit of truth.

The constant pursuit of truth in the form of repeated research leads to negation or revision of earlier findings, generalizations and theories in the absence of objective evidence. Repeated negations through repetitive or verification researches ultimately take the researcher to a point where the theory cannot be disputed or refuted any further which then becomes a confirmed fact. This in fact is the essence of research which is beautifully brought out by John Best in his definition of research as "The systematic and objective analysis and controlled observations that may lead to the development of generalizations, principles, or theories, resulting in prediction and possibly ultimate control of events".

From a general understanding of research, we have to proceed to academic research which is a formal, time-bound work. This article brings forth issues and aspects related to deciding a research

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problem. In order to describe, explain, predict and control events, the quest for seeking answers to unsolved problems is the focus of learned scientists. Their findings result in changing policies, principles, programs and practices in the discipline in which research is conducted.

Deciding a Research Problem

In academic research choosing a problem for investigation is itself a problem. The author's personal experience stands witness to this. About two years ago the author delivered a lecture in Adi Parasakthi Group of Institutions at Melmaruvatthur near Kanchipuram, Tamil Nadu, for the purpose of motivating a mixed group of faculty and scholars from the disciplines of engineering, nursing, nutrition, education, arts and other science groups to undertake research. The participants were asked to write down in a piece of paper a problem from their academic experience where they felt it was an unanswered one or one that needs systematic enquiry. Much to the surprise of the author only five out of 500 participants suggested a problem!

Another experience of the author was that while selecting a problem for her Ph.D. work she had to consider factors such as quality supervisor, innovative area, feasibility in terms of field work, time and cooperation by research participants. Though she very much wanted to work in the area of philosophy of education, being a post-graduate in both Philosophy and Education, it was difficult to fix up a supervisor who satisfied the criteria just stated. Probable guides were either experts in Education only with no specialization in Philosophy or had specialization in Philosophy alone with limited exposure to education. One of the senior colleagues of the author who worked on Sri Aurobindo's Philosophy of Education faced difficulty in getting the adjudicators appointed to assess his report. The lip services rendered by experts in apex bodies of higher education stand exposed when it comes to appointing persons with Ph.D. in interdisciplinary areas. Hence, innovations are bound to be limited and job opportunities are practically nil for those who venture research in interdisciplinary areas. This is very true of social sciences. Even scholars from applied sciences face such problems.

Non-academic Reasons that Decide the Choice of the Problem

Books on research methodology provide a lot of criteria for selection of a problem; but I am yet see one publication on the non-academic factors that determine the choice of the guide who will be supervising the research work.

Some factors are related to the area, subject, the context of the researcher such as gender, age, economic status, location, etc. In fact it is the availability of a suitable guide that determines the selection of a problem. The guide should be an expert not only in the area of research, but methodology of research, statistical techniques, etc. In addition, the guide should be uncorrupt, a person of clean character and an honest person. One can understand that the hurdles in selecting a research problem partly lie with the difficulties in choosing a supervisor.

Personal Experience of the Author

It will be interesting to share the author's personal experience in fixing up a research supervisor for her Ph.D. work in the early part of eighties. Being a double gold medalist at P.G. level (M.A. and M.Ed.), all of those whom the author approached initially to be her guide, were very willing to take her as their ward. Suddenly it occurred that to guide a double gold medalist, the supervisor should be a quality academician. Then the search started with the only idea of choosing the best academician leaving other considerations for research. But deciding on efficiency of experts, while the author was a new entrant to the profession with very limited contacts was a tough task. Fortunately, in 1979 there was a State level conference for the Heads of Departments and Colleges of Education in Tamil Nadu. The purpose of the conference was to revise the curriculum for B.Ed. and semesterise the B.Ed. program. The participants were about 20 in number. The author obtained special permission from the Directorate of Collegiate Education which organized the event, giving in writing that no TA/DA would be claimed for participating as an observer. Participation was very rewarding and helped the author in identifying the outstanding participant of the conference. The author found that one Prof. S.Srinivasan, the then Principal of Lakshmi College of Education, Gandhigram, affiliated to Madurai Kamaraj University, Tamil Nadu, who was her own teacher at M.Ed. level. The author felt that the entire process was a successful one and approached Prof. Srinivasan, an intelligent, knowledgeable and honest academician who was happy to accept her for supervising her Ph.D. work. Later on the author came to know that he was a student of Arthur Jersild, a great psychologist from the U.S. The idea behind sharing this personal experience is that a good 'home work' is required before finalizing the research guide. At every stage he promoted creativity and quality by providing utmost freedom to the author. The enthusiasm and the spirit of both the scholar and the guide lasted from the beginning till the end of the research work. This was because of the care taken while selecting the guide.

Sources of Research Problems

Theories, personal experiences, reviews of earlier research findings, sparks from seminars on current issues in the subject or area of research are a few sources that help in identifying a research problem. Theories are rich sources of problems; they provide the rationale for existing events, practices and policies which are constantly investigated for further refinement. Theories provide directions for research, explaining the rational basis and assumptions. Tested and verified theories are ideal for practical implementation. Theories give explanations for complicated problems which may be empirically tested further.

When Newton observed an apple falling down while sitting in his garden, he thought, "Why does this happen? Why is it that the fruit is not going upwards or side wards? Is it that earth has some pulling power that draws things towards it?'' This observation leading to an assumption led to the formulation of the Theory of Gravity. Thus, a new knowledge is generated because his attempts to find explanation from available literature could not answer his questions satisfactorily. The knowledge generated by him was scientifically tested repeatedly in various contexts and the theory of gravity was established into a fact. Being skeptical and probing continuously are essential prerequisites to problem identification. Not accepting all things because they are already accepted is a desirable quality of a researcher. Skepticism makes the research process objective and empirical. There is a famous saying that "If you start with probabilities, possibly you may end with certainties; but if you begin with certainties, you may end with probabilities". Therefore, the researcher has to probe continuously on improbable possibilities and possible improbabilities.

Replication or repeating a research work is another source of research problems. Verification type researches are helpful to check the validity or veracity of research findings across different populations for wider generalization. If the findings are incomplete, insufficient, disputable, conflicting, then there is strong need for replication through fresh investigation. It is not necessary to check the findings of every study, but milestone studies need verification, applying different methodologies, over a period of time and across various cultural contexts, as they will have better impact on a larger population. Refuting an earlier finding, subjecting it to several studies, will confirm or negate the earlier theory. This will remain a fact until further negation. Atoms were thought to be indivisible and the smallest units of matter until Dalton's period. It was Rutherford who showed that using cyclotron atoms were divisible into proton, electron and neutron. Now particle physics has advanced to a stage of research on neutrinos or god particles. This is the beauty of scientific research. It should be remembered that 'hypothesis generation researches' are more qualitative and evolving, whereas 'hypothesis testing researches' are more quantitative and confirmatory in nature.

Besides the sources discussed so far, there are a few clues that may be helpful for researchers.

1. Observing carefully existing practices in the areas of scholar's interest.
2. Critical reviews of good research articles.
3. Some unanswered questions discussed in textbooks in the course of presentation of ideas.
4. Trend reports presented in reviews of earlier researches given by eminent scholars.
5. Keen observation and sensitivity to daily life experiences suggest sparks for research.
6. Consultation with experts working extensively in areas of one's research interest during training programs, workshops, conferences, etc.

Factors to be considered while deciding to Work on a Problem

1. Consider the future employment or promotion prospects in the professional ladder.
2. Identify a broad area initially and then finalize the topic. Jumping straight away into the topic is risky.
3. Select a topic that has a scope which is neither too broad nor too narrow. The author interestingly enough came across a M.Ed. dissertation titled "Problems of Commerce Education", while accrediting a university (as a NAAC Peer Team Member) which runs M.Ed. program. This may be an essay topic suited for U.G. level work! There was one example which the author found in a good book on research methodology. This was an example of a too narrow a scope for a study, if at all it can be termed so; it was "Effectiveness of pre-class reminders to children in reducing instances of pencil sharpening during class time"!

Three major questions that need to be answered by an academic researcher, before finalizing a research topic are:

1. "Why do I want to select this topic?"
2. "What policy recommendations can I make through this study?" or "What contribution can I make to existing practices in my area after spending a lot of time, effort and money?"
3. "What is new in this study?" A convincing answer to these questions will ensure quality research work and reduce the gap between Research and Development in social sciences.
4. Avoid topics on philosophical, ethical and debatable issues that are fluid in nature and not suitable for research.
5. Avoid opinion based researches that are non-contributory. One topic which is over researched is "Job Satisfaction of Teachers" at various levels teaching various subjects. When there are too many factors interacting with job satisfaction, how can be a concrete finding arrived at? The measuring scales for such studies are highly subjective and incomplete.
6. Do not try to select topics relating gender, community, religion, location, order of birth, age, experience, type of family, type of management of school, socio economic status, with academic achievement and similar variables, unsupported by theory but very often found in many flimsy studies that fail to make any impact on the society or on the academic circle. Findings of such studies ever remain inconclusive and contradictory.
7. Better be conscious and cautious about the "M"s while deciding a topic:

   (a) Manpower – your skills as a researcher (qualitative studies are more demanding than the quantitative studies). Also, think of possible noncooperation by participants of your research work.

   (b) Measurement possibilities (some variables are observable but difficult to measure, especially in social sciences, where we have only approximate values, with scales having no equal intervals).

   (c) Materials for research (now it is easier to access resource materials, thanks to the IT revolution). Also, money or financial resource needed to complete the study. Plan well ahead.

   (d) Manageable nature of the study. Just because there are ready made tools for assessment of variables, there is a tendency among scholars to take as many variables as they can, to impress upon the examiners; this will result in reports, the size of which will be that of full grown babies! Quality is somehow linked with the number of pages, hypotheses, objectives, findings, and the like by some guides and adjudicators. The author wants to state that her doctoral research had only one objective, one hypothesis and one finding.

This article is an attempt to help the novice enthusiastic research scholars who are in search of both a problem and a suitable guide. The personal experience has been shared to make the paper more interesting and educative to scholars.

Research, if properly understood, is a fascinating activity and a continuous process and should not be stopped with obtaining a degree.

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