Understanding the Issues Involved In The Selection Of A Research Problem In Social Sciences: An Explorative Study

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Abstract
The purpose of this paper is to understand the issues involved in the selection of a research problem in social sciences. This paper answers three basic questions, how can a researcher choose his research topic? How can he identify research problems in the chosen topic? How can he select an appropriate research problem among identified research problems? This paper addresses the strategies of choosing a research topic. The paper further describes the sources for identification of the research problems in the chosen topic. The paper ends with the evaluation criteria of a research problem so that an appropriate research problem can be selected. This paper will help the beginning doctoral students in selecting an appropriate and significant research problem.

Keywords: Research Topic, Research Problem, Evaluation Criteria, Research Ethics

Introduction
Research starts with the selection of a suitable research problem. Research problem is a question that researcher wants to answer or a problem that a researcher wants to solve. In a classic work, John Dewey (1993) spoke of the first step in the scientific method as the recognition of a felt difficulty, an obstacle, or problem that puzzles the researcher. Therefore, problem selection is the first step in the research process. Most researchers find the problem selection a difficult task. This difficulty arises not because there is limited range of researchable problems in social sciences but due to the inability to locate relevant research problem without duplicating earlier studies. This inability may be due to his limited knowledge of research process and his unpreparedness for identifying the problem. He may also be unfamiliar with the areas in which research is needed and procedures he is to follow for selecting a suitable problem from the topic in which he is interested. In this connection, a researcher often asked a question-How to select a research problem? This paper is an attempt to find the answer of this question.

Choosing a Research Topic
The first task of a researcher must be to choose a topic within which he would conduct the research. Here topic means a subject area that contains numerous potential research problems. This is the first step in the process of the selection of the research problem. Topics arise in various ways. Natural curiosity or burning interest in a particular area may lead to the selection of a research topic.

The whole research process is normally time consuming and a lot of hard work is needed. If a researcher chooses a topic which does not greatly interest him, it would become difficult to keep up the motivation to do research. All problems or issues motivating a study begin with a perception that something requires attention. Problems are not given by nature, but by how, whom and why they are perceived…that being the case, any formulation of a problem is a partial representation reflecting the perspectives and interest of the observer (Van,2007). The research topic should be one that holds both the interest and curiosity of the researcher. Interest and curiosity in a research topic depends upon the deep knowledge, experience and sensitivity of the researcher.

Being selfish is something few adults would, openly at least, admit to. Yet it is central to the sanity of the hard-pressed researcher. At the start of your project you are about to take on a considerable commitment which is probably in addition to many continuing demands on your time….So be selfish, focus on what interests you ,think about your curriculum vitae and your future professional development as well as the impact your study might have on the workplace, and then step forward with confidence (Edwards and Talbot, 1999).

A researcher can choose a topic in the subject in which he has deep knowledge. Suppose he is a student of financial management in a university. His subject areas of interest are capital budgeting, financial leverage, working capital management and profit management. He has deep knowledge and keen interest in working capital management. He also has a curiosity to know more about unknown aspects of the working capital management. Here working capital management can be the research topic for him to conduct research.

A researcher learn new things and undergo new experiences every day. His personal experience may serve as a good source of ideas to find a research topic. For instance, a researcher observed domestic violence suffered by wives of alcoholic husbands. This experience may provide ideas to identify several research problems relat-
ed to domestic violence against women. Here domestic violence against the women may be the research topic for the researcher. There may be so many such personal life experiences of researcher to find a research topic.

Sensitivity to the environment is a good source for research topic. If we are sensitive to life situations, we may hit upon questions worth of investigation. It is a mark of scientific genius to be sensitive to difficulties where less gifted people pass untroubled by doubts (Sellitiz, Wrights man & Cook, 1969). The most evident source of research topics is contact with the external world and direct observation of it. Many of the world’s great scientific advances are due to pure observation (Bless, Smith & Kagee, 2007). The story about Newton justifies to this. Though apples might have fallen on the heads of the people before Newton’s time, only the sensitive Newton applied his mind on this event which leads to the discovery of “Law of Gravitation”. A teacher can sense problems related to students’ behaviour, teaching methods, parents, textbooks, curriculum, the way of presenting the subject matter, use of educational technology and evaluation.

It is good idea to think about the field in which a researcher would like to work after completing his studies. This will help him in finding an interesting topic for research and which may be of use for him in future. If he is a student of social work, wanted to work in the area of youth welfare or domestic violence, he might take research in one of these areas. If he is studying marketing, he might be interested in researching consumer behavior. If he is studying to become a teacher, he might be interested in researching teacher behavior. The choice of the topic must be very personal otherwise researcher may find it difficult to have motivation in doing research. Further, he must do armchair thinking for identifying research problems in chosen topic.

Identifying Research Problems
Research was born out of man’s problem and man’s major problem demands research (Good, Barr, Scates & Douglas, 1972). There are several sources of research problem that a researcher can investigate. Therefore, after having chosen the research topic, you must start finding research problems. A topic contains several research problems which may belong to the pure, applied or action research. A researcher must do deep study of the research topic and try to find out the unsolved problems. The more he thinks or reads about his topic, the more research problems he will identify. A deep understanding of the known facts in the chosen topic is the most important step in the identification of the research problems. If a researcher know what studies have already been conducted in the chosen topic, he can also know about the problems which have remained unsolved. Although, there are no set rules for identifying a research problem, certain suggestions can help him. However, Greer (1977) suggested three broad categories of problematic situations each of which remains important in contemporary social science research. He classified them under three labels namely: policy problems, problems of social philosophy and the problems which are original to the development of social sciences disciplines. In other way, three important sources for problematic situations are experience, deduction from theory and relevant literature.

Experience of a researcher is one of the good source of research problems. Research is a combination of both experience and reasoning and must be regarded as the most successful approach to the discovery of truth (Cohen and Man ion, 1994). If a researcher as an educational practitioner is sensitive towards the problems related his day to day professional work, he would come across many research problems. In a classroom there is a dynamic interaction between teacher and pupil, between pupil and pupil and between pupil and materials. This interaction provides a rich source of research problems. Teachers have intuitions about new relationships or why certain things in school happen the way they do. Teacher often question the effectiveness of certain classroom practices that have become routine but that may be based more on tradition or authority than on scientific research. They wonder if alternative procedures would be more effective. A teacher might have a question about strategies to improve the academic achievement of the students. An elementary teacher may have questions about a new method to teach reading. In this age of accountability in education, teachers want to know if programs and practices they use are the most effective. Different professional personnel in the field of education view the educational scene from different angles and they may encounter many problems of research in their own prospective. Research can provide the answers to such questions.

One premise that any researcher must challenge is the acceptance of existing scientific dogma or paradigms (Kuhn, 1962). Therefore, theories are a good source of research problems. A beginning researcher can take an interesting theory and then make a hypothesis about what could be expected to happen in a particular classroom situation. It will help to determine whether a particular theory can be translated into actual practice. For instance, social comparison theory suggests that students form academic self concepts by comparing their self perceived academic accomplishments to some standard or frame of reference. The frame of reference for the most students is the perceived academic abilities of their classmates. A hypothesis that could be deducted from
this theory is “Gifted students placed in selective homogeneous classes will have lower academic self concepts over the time than equally gifted students in heterogeneous classes (mixed ability classes). It is only through the application of scientific enquiry, that we can profitably test the validity, scope and practicability of various theories in educational situations.

I was desperate for an idea, any idea, so I began by asking around. Surely someone out there in practice would have an exciting question that they felt must be asked but not the knowledge or resources to pursue?.....When it became obvious that no one was going to present a research question to me on a plate I began my search in earnest. I read a lot and went through back copies of journals. I particularly choose the Journal of Advanced Nursing and The International Journal of Nursing Studies to look through because these were very general in their content, were academic in nature and very often researchers would mention ‘implication for further research’ at the end of their paper. After leafing through several journals I came across an article about creativity and nursing (Miles & Huberman 1994).

The final step in approaching a problem is to apply one or more relevant theories by conducting a complete review of the literature (Bourgeois, 1979). Therefore, another valuable source of problems is the published literature in your area of interest. The review of literature is a necessary and important part of the research process (Locke, Silverman & Spirduso, 2010). Published literature includes Research Reports, Research Abstracts, Research Journals, Encyclopedias, Surveys of Educational Research, Research needs in the Study of Education and Research Periodicals. The review of literature will not only provide research problems in your area of interest but also suggests the way in which research is conducted.

All published research reports generally conclude by making suggestions about further research. The researcher can pick up a suitable problem for their own study. Replication of research studies is also a useful and worthwhile activity for a beginner in research. There is a common feeling that once a study has been conducted, it should not be repeated. Repeating a study at different times on different groups in different contexts and places increases the extent to which research findings can be generalized and provides additional evidence of the validity of findings.

Alvesson and Sandberg (2011) identify ‘gap spotting’ and ‘problematization’ as two possible strategies for generating research topics from the literature. Gap spotting means identifying gaps in the literature or opportunities to extent current theories in some way or other. Problematization means a more fundamental investigation of the Assumptions that underlie existing theories.

Extension of completed research studies is also a useful and worthwhile activity for a beginner in research. He may find a question that represents the next logical step in the research on a problem. In such type of cases a critical analysis of the solved problems and a bit creativity is required to find new unsolved problem. He may find several potentially researchable problems. Sometimes research studies are criticized for weakness in design, tools, treatment and analysis of data, contradictions and inconsistencies in the results and so on. A valuable and worthwhile contribution can be made by repeating such studies after making necessary modifications in the design and procedures so as to correct or modify the findings for their inconsistencies.

Review of research integrate and summarize studies on specific topics can be use full for identifying a research problem. Review of research provides research done, research in progress and research needed in a specific broad topic. This can help the researcher in finding the research problems. Light and Pillmer (1984) have written that disagreements among findings are valuable.....conflicts can teach us a lot.

In addition to the above mentioned sources, movements originating outside our own discipline or country often expose us to new path of research (Ary, Jacobs & Razavieh, 1979). The world-wide women’s movement has drawn awareness to the gender gap in knowledge to be filled on the role of women in the development process. This movement therefore leads one to expect that researchers in the country will be motivated to select valuable topics from such programmes as ‘better life for rural women’ or ‘the unremunerated work of women.’

Most postgraduate students who have not had any experience can get ideas from discussions and their reading in education courses. Intensified discussion within a group of interested persons may often be a means of identifying pertinent questions, and of developing new ideas about a problem (Clover, Vernon & Howard, 1979). Discussion works as a brain storming exercise. It helps in finding a research problem. While discussing with research oriented people and experts of the field, the shrinkages of mind are opened and then there is a germination of a seed of new problem. These discussions give chances to the researcher to get into close contact with crucial problems. Sometimes researchers have problem in their mind but they are not able to verbalise and give proper shape to the problem. It is a discussion with research oriented people and experts of the field that help researcher to evolve a manageable research problem. It is a very use full resource of research prob-
lem identification. Discussion also make it clear whether a problem belongs to fundamental, applied or action research.

The technological and social changes are constantly bringing new problems and new opportunities for educational research. The use of hardware and software in classroom instructions, the training of teachers in the methodology of teaching through team teaching, micro teaching, simulation and computer are being advocated by the educationists all over the country. All such innovations in education need to be carefully evaluated through research process (Lokesh Kaul, 2009).

If a researcher wants to conduct a study in the area of alcoholism. The list of identified research problems may be-The causes of alcoholism, the process of becoming an alcoholic, the effects of alcoholism on family, community attitude towards alcoholism and the effectiveness of a treatment model. Once researcher identified a long list of the problems from various sources, he can proceed to the next stage where he select the most appropriate and significant problem from the list of the problems to conduct his research.

Evaluating and Selecting a Research Problem
It is neither advisable nor feasible to study all problems selected in the chosen topic. Every problem has equal chance of being accepted or rejected depending upon its success or failure on the criteria usually adopted for the evaluation of a research problem. Whereas no standard procedures exist for evaluating the suitability of a research problem (Ezejelue & Ogwo, 1990), certain criteria have been found helpful for the selection of a problem. Indeed the researcher’s personal knowledge, experience and interest as well as external circumstances generally determine the choice of a problem (Webb, 1961 and Good, 1969). The researcher and research process together form the internal and external criteria respectively for the selection of a research problem. While selecting the research problem, it is advisable to clarify the internal and external criteria neutrally to conduct a free flow research.

Internal Criteria
The internal criteria deals with the researcher i.e. his interest, his competence and his resources (time and money). This is also called Self Criteria for the selection of a research problem. Interest of the researcher should be the most important determinant for the selection of the research problem. In the absence of interest, it would become difficult to keep up the motivation to do research. Even a small difficulty may become an excuse for discontinuing the study. In this case, the relevance of the research to the broader community should be considered (Bless, Smith and Kagee, 2007).

A mere interest will not work in the selection of a research problem. The researcher must be competent to plan and carry out a study of the problem. Before selecting a research problem, he need to ensure that he met certain level of expertise in the area he is proposing. He must possess adequate knowledge of the subject matter, relevant methodology and statistical procedures.

The practical aspects of a project’s feasibility centre primarily on two related concerns: time and money (Kelly & McGrath). In selecting a research problem, one of the major considerations is whether there will be sufficient time and money to adequately complete what researcher hope to do in his research work. Many significant problems are usually not investigated not because researchers are incompetent to do so but perhaps because they are beyond the limit of their financial resources (Hay, 1984). Time and money constraint involves the selection of population, the proper development of measuring device, actual data collection and analysis of the data.

One way to decide what interests the researcher most is to start with the process of elimination. Go through list and delete all those problems in which he is not very interested. He will find that towards the end of this process, it will become very difficult for him to delete anything further. He need to continue until he is left with the something that is manageable, considering the time available to him, his level of expertise and other resources needed to undertake the study.

External Criteria
The external criteria deals with the aspects of research process i.e. significance of research, method of research, sampling, tools, statistical techniques, data collection, generalization, administrative co-operation and novelty. This is also called Scientific and Social Criteria for the selection of a research problem.

Although every problem in science involves a question or a series of questions, not every question qualifies as a scientific problem (Merton, 1959). A researchable problem is one that can be investigated through the collection of data. Many interesting questions in education cannot be answered by collection of data. Philosophical questions like “Should schools give more attention to character education?” cannot be answered by scientifically gathering and analyzing data. Ethical and value-loaded problems which are viable but cannot be empirically verified or explained (Akindele & Adebo, 1993). Such problems should be avoided. They can hardly be answered directly through scientific research (Nachmias and Nachmias, 1976). But it is possible to restate philosophical questions to make them researchable. The previous question could be restated as follows: “What is the
The effect of a character education program on the incidence of cheating in high school?” It would be possible to gather data on this question, which could then be used by educators to help make decisions about a character education program.

Problems requiring investigation are unlimited, but available research efforts are very much limited. Therefore in selecting problems for research, their relative need and urgency should be considered. An urgent need problem should be given priority over an unimportant one. For example, in industrial management today, problems of productivity, capacity utilization, motivation and industrial unrest are more important than problems of financial leverage, profit planning and marketing. Research must be focused on needful and urgent problems.

The problem should be one whose solution will make a contribution to the concerned body of knowledge or to the solution of some significant practical problem. The problem may fill in gaps in current knowledge or help resolve some of the inconsistencies in previous research. Researcher should be able to answer the question “So what?” with respect to his proposed study. Would the solution make any difference to educational practice? Would other educators be interested in the findings? Would the findings be useful in an educational decision-making situation? It should be socially relevant. This consideration is particularly important in case of higher-level academic research and sponsored research.

A research problem is a step towards new knowledge. The problem must have novelty or originality. There is no use of wasting time and energy on a problem that had already been studied by other researchers. To avoid such duplication, it is essential to examine very carefully the record of previous studies. But this does not mean that replication is always needless. In social sciences in some cases, it is appropriate to replicate a study in order to verify the validity of its findings to a different situation.

The problem should be one that will lead to new problems and so to further research. A good study, while arriving at an answer to one question, usually generates a number of other questions that need investigation. Avoid problems that have little or no relationship to theory or previous research. In this reference, a beginning researcher should selecting a problem that could possibly be expanded or followed up later in a master’s thesis or even a doctoral thesis.

Sekaran and Bougie (2010) suggest that research problem should be relevant, feasible and interesting. Relevance is achieved if the problem relates to a definite need in the domain of practice or theory that is recognized by appropriate stakeholders. Feasibility addresses two related aspects. Firstly it must be possible to address the problem through research and secondly it must be possible for you to carry out that research within the contrants of ethical conduct, access, time and other resources. Feasibility is therefore closely related to the scope of the research.

Unlike researchers in the physical sciences, educational researchers are dealing with human subjects with feelings, sensitivities, and rights who must be treated ethically. Do not plan a research that may cause physical harm or psychological harm such as stress, discomfort, or embarrassment that could have lasting adverse effects. Fortunately, most educational research does not involve great risk of harm to subjects. However, the potential for harm always exists, and a researcher should be prepared if a participant requests counseling or other help after participating in the study.

Each identified problem must be evaluated in terms of the above internal and external criteria and most appropriate one must be selected by the researcher. Once he is confident that he has selected something that he is passionate about and can manage, he is ready to go to the next step i.e formulation of the research problem.

Conclusion
This paper explores a range of strategies regarding the selection of a research problem in social sciences. The selection of a research problem involves choosing the research topic, identifying research problems and evaluating the research problems with reference to internal and external criteria. The researcher first must choose a research topic in which he wishes to conduct a research. Further, he must identify possible research problems in his research topic through personal experience, deduction from theories, review of literature and other sources. Lastly, he must evaluate the research problems in terms of internal and external criteria so that an adequate and significant research problem can be selected to conduct a research. The number of social researcher is increasing everyday in the developing country like India. They all have difficulty in the selection of a workable research problem. This paper will help them in selecting an appropriate and significant problem for their research work. In this way, their significant research work will contribute a lot for the development of the nation.
References


