IDENTIFICATION AND FORMULATION OF RESEARCH PROBLEM

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Introduction

IDENTIFICATION AND formulation of problem constitute the starting phase of research enterprise. Its importance cannot be emphasised enough, for success of a research enterprise depends on the selection of an appropriate problem and its proper formulation. While a faulty selection may fail to sustain researcher's interest in the study, a deficient formulation may land the researcher in unanticipated difficulties at later stages.

For the same reason, it is a challenging and time consuming task. Merton, a renowned sociologist, observes : "[I]t is often more difficult to find and to formulate a problem than to solve it." This indeed is a task that merits serious attention and tremendous patience. Darwin, for instance, took years to find and formulate his problem. Impatience in this respect does not pay. Though patience need not be mistaken here for complacence which is what may happen in many cases.

This task has been formalised in some ways in the academic world. Synopsis, for instance, is one formalised version of it. Every student working towards a research degree is required to submit a synopsis. A synopsis is nothing but statement of a well formulated research problem. Project proposal is another formalised version of it. Any researcher seeking grant from a research funding organisation is required to submit a project proposal. Its academic part is nothing but statement of a carefully formulated research problem.

From the above it is clear that the importance of finding and formulating a research problem has been formally recognised in the academic circles. What is, however, surprising is that the existing textual literature on research methods does not seem to attach sufficient importance to it. In most books on research methods there is not so much a mention of this topic. Even where it is included in the contents, its treatment is somewhat formalistic as well as sketchy.

This article seeks to present some of the ideas of the author, based on his experience as well as reading, on this relatively neglected theme. In the interest of systematic exposition, it is proposed to discuss the subject in two parts : Identification of problem and formulation of it. The focus will be on reviewing the existing state of affairs and on presenting some guiding considerations. Attempt will be made to draw illustrations as far

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as possible from the Indian context on the one hand and from legal research on the other.

Identification of problem

It will be in order to begin by defining a research problem. A research problem is not the same thing as a social problem. A research problem is defined by intellectual curiosity while a social problem is defined by the values of a group. A situation may be problematic for one group but it may not be so for another, depending on their differing value systems. Increase in incidence of crime may be a problem for social workers, but it may not be so for the underworld of criminals. For a researcher, on the other hand, it is not only the crime but also the law abiding behaviour which constitutes a problem. It is as important, and from an intellectual vantage point even more important, to ask why people adhere to law abiding behaviour. In no sense is law abiding behaviour a social problem. But it may be an important research problem. Thus a research problem is a cognitiv phenomenon while a social problem an evaluative one. This does not imply that a social problem cannot turn into a research problem. A social problem may turn into a research problem once it is so formulated by a researcher.

What are some of the sources of finding a research problem? Taking a cue from the existing practices, one can immediately mention the following :

(1) Research supervisor.
(2) Research literature.
(3) Research funding agencies.

It is well known that students desirous of pursuing research for a degree generally leave it to the supervisor to find and suggest a problem for them. This is so partly because they do not want to strain their mind and partly because they begin with a sense of total dependence on the supervisor. Whatever the reason, the fact remains that most research students look up to the supervisor to get a research problem assigned to them. They seem to have hardly any interest of their own in the problem, let alone a sense of involvement in it. The students who come up with a research problem on their own are more of an exception than a rule.

The research supervisors in turn draw upon the existing research literature for searching a problem. Research books, research journals and trend reports are some of the more important components of research literature which in one way or another throw up research problems. From such literature one may get a clue to an unexplored area, a hypothesis to test, or a new direction of inquiry. One may as well adopt a problem *in toto* for investigation in a different society. In fact, quite a sizeable part of social science research in India is a result of the study of research problems borrowed from American and European journals. That is the reason why it remains secondhand.

A third source of problem finding is the lists of research priority areas drawn up by the research funding agencies. The Indian Council of Social Science Research, for instance, has identified subjects of research priority which will get funds on preferential basis. At the time of elections, similarly, special funds are earmarked by some research sponsoring organisations for election studies. There is no dearth of professional researchers who will quickly change their research interests in order to take advantage of such research funds.

The above sources and practices of problem finding are fairly widespread. All of them are, however, external sources. What they miss in common is the importance of subjective factor in the choice of a problem. In each case the researcher tends to work on a problem given by others—a supervisor, or an author of a trend report, or an editor of a list of priority areas. The problem does not seem to come to him from within. His role is only that of a chooser out of a given number of research problems.

This lack of subjective factor in identifying a problem is, in our considered opinion, at the root of much of the bogus research that we have in social sciences in India. It renders the research activity a ritualistic activity. How can one produce quality research unless the research problem has sprung from within. There is a substantial difference between choosing a problem out of a given list and identifying one out of one's own suffering.

Without undermining the value of the existing sources and practices of problem finding, we should like to underline the centrality of the subjective factor in identifying a research problem. Our submission is that it will help improve the quality of research if the researcher works on a problem identified out of a suffering experience. Suffering, it will be agreed, is the source of creative ideas, and more so is intellectual suffering. Intellectual suffering means a sense of deep excitement about the problem arising either out of one's experience of having lived through that problem in actual life or out of one's empathetic experience of it. Such an experience turns into intellectual suffering the moment one suffers it at intellectual plane over and above the experiential plane. Take, for instance, apprenticeship in legal profession. A junior working with a senior lawyer has to undergo certain experiences which are likely to give him a perceptive insight into the nature of junior-senior relationship. He knows it better than anybody else where the shoe pinches. If he is able to relive this experience at intellectual level and work on it, the output is likely to be masterpiece. An outsider who has not had such an experience may of course get an insight into it through intellectual reach, but will have to struggle much harder to reach anywhere close to it. The point is not that without subjective experience of the problem creative research is impossible; the idea rather is that the research is likely to gain in quality if the research problem is born out of an inner experience, or at least out

of one's genuine intellectual involvement in it.

Having examined the sources, it will be pertinent to note some guiding considerations in the choice of a research problem. The first such consideration which follows from the above is that a research problem should be identified preferably out of one's inner experience which one is able to suffer at intellectual plane.

Another important consideration is that the research problem selected should be empirical. In fact, scientific research by its very definition presupposes choice of an empirical problem, and there being much scope for empirical research on social phenomena there is also a great need for it. Such a need is even greater in the realm of legal studies because legal research has been dominated by interpretative studies. There are hardly any empirical studies of legal phenomena in India, and whatever few studies there are, these have been made mostly by foreign scholars. The Indian law scholars, with a few notable exceptions, have largely been not only indifferent but even hostile to the idea of empirical research. In view of this the importance of selecting an empirical problem for research cannot be exphasised enough.

Yet another consideration to be kept in mind while selecting a research problem is that of its theoretical potential and practical relevance. The problem selected should be such that it has a potential to advance our existing theoretical knowledge, or else it has at least some practical relevance. It may be added that this remains a common shortcoming of much of the existing research, more so of studies of doctoral level. On the theoretical front, they hardly seek to link the data with any theoretical structure by way of revising or refining it, let alone developing an altogether fresh theory. On the practical side, not much research is being undertaken on the problems confornting the nation. It is, therefore, of utmost importance to identify such a research problem as may enable one to make a contribution either on theoretical or practical front, preferably on both.

Formulation of problem

It is possible to distinguish two broad approaches to problem formulation—deductive and inductive. The exponents of positivist epistemology patronise deductive approach while the advocates of ethnomethodology prefer inductive approach. The deductive approach takes a set of propositions derived in an *a priori* manner through deductive reasoning from a theoretical premise as starting point of problem formulation. These propositions define the lines of inquiry and are tested for their validity in terms of the systematically collected evidence. In simple words, it views problem formulation in terms of developing a set of hypotheses which will be tested in the inquiry. For this purpose, it insists on having a research design, a representative sample, an idea of independent and dependent variables, and a standard instrument.

The inductive approach, on the other hand, recommends "direct naturalistic examination of the empirical social world" for purposes of problem formulation. It implies that the investigator will first acquire a "close and reasonably full familiarity" with the area of life under study and will then progressively sharpen his focus as the inquiry proceeds. The inductive approach expects a researcher to go to the field with an open mind, having no *a priori* assumptions, and to develop, focus and sharpen his investigation in the light of his understanding of the reality in the field so that the research problem is grounded in the empirical world. In Blumer's words, it is a flexible procedure "in which the scholar shifts from one to another line of inquiry, adopts new points of observation as his study progresses, moves in new directions previously unthought of, and changes his recognition of what are relevant data as he acquires more information and better understanding." Clearly, the inductive procedure of problem formulation stands in sharp contrast to the fixed and circumscribed procedure of positivist deductive procedure. This does not mean that there is no direction to the inquiry; it means that the focus is originally broad but becomes progressively specified as the inquiry goes forward.

These methodological positions notwithstanding, problem formulation signifies an attempt to specify the direction of inquiry. This involves three tasks : Framing the title, formulating research issues, and operationalising the concepts.

A title is a formal shorthand statement of the problem. It is supposed to signify the focus of inquiry. Framing a title is no easy task. Generally one comes across two types of titles—descriptive and analytical. A descriptive title is framed in terms of the content of study while an analytical title points out the perspective of study. Examples of descriptive title are : "Law Colleges and Law Students in Bihar", "Indian Supreme Court Judges : A Portrait", "Lawyers at a District Court". What these titles indicate is the scope and content of each study. On the other hand, analytical titles bring in sharp focus either an issue or the perspective of the study. Examples of an analytical title are : "Kinship in Professional Relations : A Study of North Indian District Lawyers", "Barristers and Brahmins in India : Legal Cultures and Social Change", "Lawyers in Government : The Most Serviceable Instruments of Authority", "Self Service or Community Service : A Study of Lawyers as Professionals". Clearly, all these titles either throw up an issue or signify the perspective used in the study. Few will dispute that an analytical title is better than a descriptive title, provided the study is designed to deal with an analytical issue or is informed of a perspective. An analytical title is certainly more complete in that it indicates both the content and the perspective of the study. In terms of format such a title usually has two parts—main and subtitle—as is evident from the analytical titles cited above. It is a different matter that in some of them the analytical point is reflected in the main title while in others in the subtitle. It is generally preferable to formulate the main title in terms

of an analytical issue or perspective.

The central task involved in problem formulation is that of framing the terms of inquiry. The specific terms of inquiry may be formulated either in terms of research questions or guiding hypotheses. Merton, for instance, has underlined the importance of research questions for framing the terms of inquiry. In particular, he mentions three types of questions : Originating question, the question of rationale and the specifying questions. Originating question is a statement of what one wants to know and it can range from ascertaining facts to explain empirical uniformities or variations. The question of rationale states why the originating question is worth asking and what will happen to other parts of knowledge or practice as a result of answering the question. The specifying questions are concerned with specifying the conditions that point toward possible answers to the originating question in terms that satisfy the rationale.

These questions can now be illustrated with some examples. Suppose we are interested in assessing the role of education in promoting attitudinal modernity in developing societies. Thus our originating question is : Whether or not education promotes individual modernity particularly in a developing society, say in India? The question of rationale will have to deal with the theoretical significance and policy relevance of the originating question. The specifying questions may be as follows : What is it about education that tends to stimulate modernity? Is it level, or spread, or structure, or curriculum, or extra-curricular programme that augments modernity? Under what conditions education tends to enhance modernity? These are obviously specifying questions as they seek to specify the conditions which affect the role of education in modernity.

The terms of inquiry may as well be formulated in the form of hypotheses. A hypothesis is a hunch, a testable proposition the validity of which remains to be determined. Framing the terms of inquiry in terms of hypotheses presupposes some prior knowledge about the phenomenon. Like research questions, it is possible to think of two categories of hypotheses—master hypothesis and subsidiary hypothesis. This can again be illustrated with examples relating to the same theme. The example of master hypothesis on education and modernity is : The level of education is positively related to modernity, *viz.*, the higher the level of education the greater the modernity. This hypothesis may also be formulated in the form af null hypothesis as follows : There is no relationship between level of education and modernity. Examples of subsidiary hypothesis are : Science students tend to be more modern than arts students; hostelers are likely to be more modern than day scholars; those who participate in extra-curricular activities tend to be more modern than those who do not.

Operationalisation is another important task involved in problem formulation. Operationalisation means devising empirical referents of an abstract concept. To continue with the problem of education and modernity, we have the concept of modernity here. Unless defined in precise

operational terms, modernity may mean different things to different people. It therefore needs to be defined in operational terms. Notionally, it may be defined as a set of such attitudes as secular, rational, universalistic, *etc.* These constituents of modernity need to be given an empirical rendering. Take, for instance, the term "secular". It has several connotations. Equal regard for all religions is one meaning of secularity. Withdrawal of the authority of religion from other than religious spheres is another. First we have to specify its meaning. Let us define it in terms of equal regard for all religions. Having done this, certain items are to be framed to tap equal regard for all religions. Instances of such items are : "No religion is as good as my own"; "world will be a better place to live in if all people adopt my religion." A positive response to these items is indicative of religious intolerance while a negative response is indicative of religious tolerance. This is how a concept is operationalised.

Now some general considerations. Specificity and clarity are the two major qualities of a sound formulation. In fact, formulation of a research problem is a process of progressive elimination of the irrelevant and specification of the relevant. A good formulation is specific in terms of its scope and coverage, perspective and focus, and universe and sample. Generally, there is a tendency on the part of a novice to embark on too ambitious a formulation to be manageable. Nothing can be more inimical to research than this. Hence, this tendency needs to be kept in check. Clarity is another desirable quality of an appropriate formulation. Words having more than one meaning or dubious implications have got to be carefully avoided. Vague and imprecise expressions must be shunned too. An example in point is the following formulation : "Structural Effects of Marriage Legislation on Society". The word "structural" here does not have a definite meaning. Similarly, the word "society" is too broad to mean much. This formulation can more appropriately be reformulated as follows : "Effects of Marriage Legislation on Social Structure of a Community". Indeed the particular community should be specified.

These, then, are some of the guiding considerations which may be helpful in identifying and formulating a research problem.