SELECTING AND DEFINING A RESEARCH PROBLEM

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AT THE TRAIN THE TRAINERS RESEARCH WORKSHOP
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1. INTRODUCTION

We shall begin this session by asking ourselves this pertinent question: **"What is Science?".** Science has been variously defined. The Oxford English Dictionary defines it as **"the intellectual and practical activity encompassing the systematic study of the structure and behaviour of the physical and natural world through observation and experiment"**. According to the Collins English Dictionary and Thesaurus, it is "**the systematic study of the nature and behaviour of the material and physical universe, based on observation, experiment and measurement**", while the Cambridge Dictionary sees science as **"(knowledge from) the careful study of the structure and behaviour of the physical world, especially by watching, measuring, and doing experiments, and the development of theories to describe the results of these activities"**. The online dictionary, Wikipedia, terms science "**a systematic enterprise that builds and organizes knowledge in the form of testable explanations and predictions about the universe**". From all the above definitions, we can identify common threads:

- That Science is the method used by scientists to investigate nature
- That science is a systematic and careful study of the physical and natural world
- That science employs observations and experiments in its bid to understand physical and natural world happenings
- That science develops theories to explain/describe these physical and natural world happenings.

Thus Science can be defined simply, like Carey (2011), as "that activity which aims to further our understanding of why things happen as they do in the natural world".

1.1 How then do scientists study the natural and physical world? From the above, it is also quite logical to state that Scientists, in order to study/understand natural and physical world phenomena, conduct research, or systematic investigation to establish facts and reach conclusions (Collins English Dictionary and Thesaurus, 2006). At the most basic level, the scientist begins research by making careful observation of some natural phenomenon. If some events which are not well understood emerge during the scientist's observation, he/she shall speculate about the occurrence of this event, before proceeding to test his/her speculation. In order, therefore, for the scientist to reach useful and reliable conclusions about any aspect of nature under study, the scientist is required to follow a systematic, logically laid out procedure, involving three key steps: observation, speculation/explaining, and testing. First, the scientist must make sure that he/she has a clear sense of all the facts surrounding the phenomenon which is being investigated, as this is necessary to establish whether or not something interesting, or possibly puzzling, may require further investigation. Having established the existence of an unexplained, or difficult-to-explain, event, the scientist must proffer possible explanations for this event, through introducing a new set of factors to account for why and how the event in question has come to be the case (Carey, 2011). Finally, to determine whether or not a proposed explanation is correct, the scientist must identify a consequence of the explanation, something that ought to occur if circumstances are properly arranged and if the explanation is on the right track. The scientist then conducts experiments designed to establish whether or not the predicted result actually will occur under these circumstances. If the scientist obtains predicted results, then there is good reason to believe that his/her explanation is right. If, on the other hand, predicted results are not obtained, then there may

be reason to suspect that the scientist may be wrong or, at the very least, that the proposed explanation may need to be modified. The above methodological and systematic approach to the process of inquiry (in which empirically grounded explanations of natural events/phenomena are constructed and verified) (Betz, 2011), is known as the **Scientific Method**. The **Scientific Method** rests on the notion that every idea about the workings of nature has consequences and that these consequences provide a basis for testing the idea in question (Carey, 2011). Research is thus a systematic process of collecting, analysing, and interpreting information (data) in order to increase our understanding of a phenomenon about which we are interested or concerned. It is, in simplest term, **the process of or body of activities involved in gathering information to answer a question that solves a problem**. Academics involve the dissemination of knowledge. Often, this knowledge is disseminated by means of models or theories. Every so often, these models and theories become inadequate or obsolete, and their applicability problematic (Dissanayake, 2013). New knowledge is needed in order to find a solution. This is achieved through research.

In our daily lives, we often employ this same systematic approach to collect and interpret information and solve small problems of our daily living. For the purposes of the current lecture, our focus shall be on formal research. This is the type of research which researchers intentionally set out on, as a means of enhancing their understanding of natural phenomena. Formalised research can vary widely in range of manners, including complexity, time, etc. Every form of formal research has the following distinct features:

- It originates with a question or problem.
- It requires clear articulation of a goal.
- It requires a specific plan for proceeding.
- It usually divides the principal problem into more manageable sub-problems.
- It is guided by the specific research problem, question, or hypothesis.
- It accepts certain critical assumptions.
- It requires the collection and interpretation of data in an attempt to resolve the problem that initiated the research.
- It is, by its nature, cyclical or, more exactly, helical.

Good research is defined by the following characteristics:

- It is feasible
- It is novel
- It is ethical
- It is relevant, and
- It is interesting

The feasibility of a research enterprise is determined by a number of factors, including its scope; its cost in both time and money; the amounts of subjects and samples required to be handled within the given time-frame and financial resources; the technical expertise required of the research team for the design and execution of the research (e.g. measuring of variables, management and analysis of data, etc.), and; the scope of the research.

1.2 Identification and Formulation of Research Problems/Research Questions. Scientific problems start with having a problem, willingness to solve a problem, and interest in a topic

(Bahçekapili *et al*., 2013). The identification and formulation of a research problem/research question constitutes the starting phase and most important step of the research enterprise, as research problems are at the heart of scientific enquiry. All across the world, one often comes across unresolved problems and unanswered questions. Oftentimes, these become subjects of extensive speculation and wondering, engendering questions, thus prompting a series of linked activities leading to and characteristic of research. The importance of research problem cannot be emphasized enough in research enterprise, as the success of any research endeavour rests on the selection and proper formulation of an appropriate problem. Faulty selection of a research problem may engender loss of a researcher's interest in a given study, while deficient formulation of research problem may lead, in the later stages of the research, to unanticipated difficulties.

The identification, thus, of a problem, followed by formulation of research questions (or objectives), is critical to the research process.

1.3 Nature of and Identification of a research problem.

Being at the core of the research enterprise can make the identification/selection and formulation of research problem quite a challenging and time-consuming task. Many versions of formalised research problem presentations exist. Synopses are, for instance, statements of well formulated research problems which are submitted by students working towards research degrees. Also the academic part of project proposals represents another form of well formulated research problems submitted by researchers seeking funding for their research.

What then is a research problem? A research problem is one which is defined by intellectual curiosity. It is not defined by group or societal values. For example, while an increase in prostitution may be a problem for social workers, it may not be so for their patrons and clients, suggesting that what might be a social problem for one group may not be so for another group. For a researcher, it is not only the rise in prostitution but also the underlying factors fuelling its rise which constitutes a problem. A researcher may want to understand why people are increasingly turning to prostitution and what factors are driving the acceptance of prostitution as an attractive and more acceptable profession. The researcher may also be interested in knowing why prostitution was less attractive in the past. Such understanding may help to shed light on what changes may have occurred over time to elevate prostitution to current levels of acceptance and practice. Here, the rise in the rate of prostitution represents a practical social problem, which is only better solved by understanding the factors that engendered its existence. This understanding may be achieved only through research. When faced with practical problems, as above, investigators become motivated to ask specific research questions, which in turn define **research problems**. Appropriately crafted research problems enable investigators to find research answers. Research answers, in turn, help to provide effective solutions to practical problems.

Research problems are those topics the researcher "would like to address, investigate, or study, whether descriptively or experimentally" (Shoket, 2014). They are therefore the foci or reasons for which researchers engage in any given research enterprise. Research problems are characteristically motivated by incomplete knowledge or flawed understanding. They are therefore only solved by achieving better understanding. In the academic world, the researcher is always looking for better understanding of natural processes and phenomena. As a result, the researcher eagerly seeks out and, sometimes, even invents research problems (where necessary) in his/her search for better

understanding. Indeed, a researcher without a good research problem has a bad practical problem, because with no research problem to work on, he/she has nothing to do. A researchable problem could be a gap in the knowledge of a given field; that expectation and reality do not correlate; that a phenomenon breaks a system or the like, i.e. something does not seem right; perhaps a phenomenon has not yet been categorised or a disciplinary "truth" you may disagree with. Many good research questions are based on the discovery of these types of anomalies. The researchable problem is usually the observation of an anomaly as well as the point of departure for a research question. The following is a list of what could constitute a real research problem (Rienecker and Jørgense, 2015):

- A gap in the field's knowledge
- Something the field has not or should not have finished researching
- An unexplained observation, an observation that sticks out
- Something that has not yet been categorised, analysed (with these particular methods/ theories; this systematism; this degree of detail or from this particular angle)
- Something that does not seem right
- Contrasts that can still be discussed
- Something that is currently being debated in the field
- Something that can and should be argued for (or against, i.e. all representatives of the field are not already familiar with or agree with the argumentation)
- Something that is in conflict with the general view
- Something that must be (re)evaluated, changed, transformed, constructed or needs new designs.

1.4 Defining/Formulating a research problem.

The first step to formulating a research problem is understanding the makings of good research, which has earlier been enumerated as: (a) the feasibility; (b) the novelty; (c) the relevance and the ability of the research to be both (d) ethical and (e) interesting.

Problems for research are everywhere. A key constraint to the development of a robust food preservation industry in Nigeria is the inadequacy of cold storage, occasioned mostly by inadequate and unreliable electricity. As a result, massive amounts of fresh fruits and vegetables are lost annually to spoilage. But have Nigerian technologists and engineers thought of developing cold storage systems with multimodal sources of energy, storage systems employing gas and electricity interchangeably, depending on the availability or not of electricity? On the other hand, we might be frustrated by the widespread environmental pollution and farmland degradation occasioned by the indiscriminate disposal of plastic bags of all sorts across Nigeria and the relatively short life span of our tarmacked roads to come up with a system which prioritises conversion of waste plastic bags and associated (and similar) wastes to shelf-life-extending components of tarmacs for use on Nigerian roads. The above are but a few examples of problems which need to be researched in our environment. As can be seen above, a researcher may, on a regular basis be inundated by ideas of potential research questions. The difficulty oftentimes is in the process of formulating them in a meaningful way (Powers et al., 1985). Formulation of a research problem requires that one has good working knowledge of both the subject of interest and research methodology. Also, a close scrutiny of a question will give an idea of the complexity of formulating a research problem into something

researchable. It is essential for a research problem to be able to stand up to close scrutiny in terms of the procedures required. Hence, the need to invest considerable time formulating a researchable research problem. Without a research problem, planning how to conduct a research is difficult. Kumar (2005) defines the formulation of research problem as being akin to identifying a destination before embarking on a journey. Identification of a destination enables one to select the shortest or best possible route to travel. A researcher must have a clear idea of what he/she desires to discover but not what he/she thinks he/she must find. A research problem can begin with something simple to something very complex, depending on the nature of the research theme. In modern environmental microbiology, with emerging renewable energy issues, many research problems may be developed from issues bordering on achieving greater efficiency, reliability and economic feasibility of bioelectricity.

The formulation of a research problem is very important, as it defines the quality and validity of the contents of a research report. Thus, formulating research problem is the most crucial step in research. It is therefore necessary to allocate enough time to the formulation of research problems, as clearer and better formulated research problems lead most invariably to clearer research and more reliable output and progress. The key sets of factors have been recognised as being involved in the formulation of research problems. These are the internal and the external factors or criteria (Shoket, 2014). The internal factors or criteria are those criteria that are determined by the researcher, like a researcher's Interest, researcher's own resource, and researcher's competence (or expertise). External factors (criteria) are those that are not researcher-determined and include problem researchability, novelty of research problem, facilities, feasibility, usefulness and social relevance, and research personnel.

A. Internal Criteria.

1. Interest is considered the most important of the factors which guide the formulation of a research problem. As research processes oftentimes require a lot of hard work and are usually time-consuming, it helps if the researcher chooses a topic which interests and challenges him/her. Otherwise it might become difficult for the researcher to develop and sustain necessary levels of perseverance and motivation. Interest in a problem is often driven by the researcher's educational background, experience, outlook and sensitivity (Shoket, 2014).

2. Expertise. This refers to the competence of the researcher to design and undertake research enterprise (including data acquisition, data analysis, etc.). Mere interest in a problem is not enough. A researcher must be have expertise (or be competent) to plan and carry out a study of the problem. He/she must possess adequate knowledge of the subject-matter, relevant methodology and statistical procedures.

3. Researcher's own resource: In the case of researcher-funded research, consideration of researcher's own financial capacity is quite pertinent. If it is beyond researcher's financial capability, researcher may not be able to complete research work, except he/she gets supported financially. Time as a resource is more important than finance. Research is a time-consuming process; hence the ability to allocate sufficient time to a given research should be properly considered when formulating the problem.

B. External Criteria

4. Data availability. If the research title needs collection of information (journal, reports, proceedings) before finalising the title, it is important to ensure that these are materials available and in the relevant format.
5. Relevance. It is important to always choose a topic that suits one's interest and profession. It is necessary to ensure that one's study adds to the existing body of knowledge. Of course, this will help to sustain interest throughout the research period.
6. Ethics. In formulating the research problem, one should consider some ethical issues as well. Sometimes, during the research period, the study population might be adversely affected by some questions. In ICT, some scenarios might occur especially research related information security, which might concern certain authorities. Therefore, it is always good to identify ethics-related issues during the research problem formulation itself.
7. Researchability of the problem: The problem should be researchable, i.e., amenable for finding answers to the questions involved in it through the scientific method.
8. Novelty of the problem: The problem must have novelty. There is no use of wasting one's time and energy on a problem already studied thoroughly by others.
9. Importance and urgency: Problems requiring investigation are unlimited, but available research efforts are very much limited.
10. Facilities: Research requires certain facilities such, as well equipped library facility, suitable and competent guidance, data analysis facility, etc. Hence the availability of the facilities relevant to the problem must be considered. Problems for research, their relative importance and significance should be considered.
11. Feasibility: A problem may be a new one and also important, but if research on it is not feasible, it cannot be selected.
12. Usefulness and social relevance: Above all, the study of the problem should make a significant contribution to the concerned body of knowledge or to the solution of some significant practical problem. It should be socially relevant.
13. Research personnel: Research undertaken by professors and by research organizations require the services of investigators and research officers. But in developing countries, research has not yet become a prospective profession. Hence talented persons are not attracted to research projects.

The difference between a trivial project and a significant project is not the amount of work required to carry it out, but the amount of thought that you apply in the selection and definition of your problem (Beach and Alvager, 1992). Researchable problems can arise from any of many varied sources. Personal experience can be a quite a strong source of researchable problems. A random observation that consumption of certain local vegetables improves oral freshness in the morning could engender a researchable problem, which in turn might lead to the ultimate development of herbal and non-herbal oral care products and procedures. Similarly, the observation that consumption of an indigenous fermented food alleviates certain disease symptoms, like those of irritable bowel syndrome, could stimulate the initiation of a researchable problem towards both understanding of the 'whys' and 'whats' of this, and in the end probably result in the development of new functional food and nutraceutical products, as

well as new medical procedures. Professional practice experience can also be a major source for identifying knowledge gaps that would benefit research.

The literature can also be a veritable source of researchable problems for a researcher. Especially young and novice researchers have benefitted extensively from this as source of researchable problems. A researcher might identify a topic of interest and review appropriate literature to determine the kinds of research which have been conducted in this area as well as prospective gaps in the state of knowledge relating to the topic. In addition, seeing how other researchers have approached a problem could oftentimes engender new ideas, or perhaps, points that would benefit from replication. For example in the brewing and malting industry, it is known that abrading malting barley improves process performance through accelerating malting process, improving grain modification and reducing the length of the malting process. A researcher on sorghum malting who has read about the above may get identify gaps in sorghum malting in relation to the effects of abrasion on malting performance. This may in turn lead to the synthesis of new researchable problem, which besides generating new information and filling the above knowledge gap, could lead to the development of an entirely new and probably improved sorghum malting process. Literature may additionally provide assistance to researchers looking to develop new researchable problems through the identification of currently existing gaps in knowledge and specific suggestions of future research in the area. The latter is usually found in a variety of research literature, including a host of unpublished materials, like dissertations and theses, as well as published research articles and reviews. In some cases exploring the literature could engender the adoption *in toto*, of a researchable problem for investigation in a different society. For example, after reading up information on consumer perception of genetically modified organisms (GMOs) in food in the United States and several countries in Europe and Asia, a Nigerian social researcher, can identify a knowledge gap in the perception of Nigerian consumers to GMOs and initiate a researchable problem, through the complete adoption of the above researchable problem from the US, Europe and Asia. Conferences could also be great sources of new research ideas and problems.

It is an accepted norm that students pursuing research degrees generally rely on their supervisors to find and suggest potential researchable problems for them. These research supervisors in turn draw upon their personal research experiences, as well as other sources like existing research literature (like research journals, review articles, research books and trend reports) for searching out researchable problems. From the supervisor's experience, one may get a clue of what areas in the topic are unexplored and for which significant gaps in knowledge remain. One may also get ideas of what hypotheses require to be tested, or of entirely new directions of enquiry. Mentors may also be good sources for possible researchable problems, with ideas being generated through conversations and collaboration. Mentors may also be able to help with grant-writing, office space, lab facilities, etc.

Oftentimes, funding agencies and professional associations draw up a list of research priorities. A National research funding agency may identify a critical gap in the provision of a solution to a National problem and thus identify subjects of research priority which will be allocated funds on preferential basis. Professional researchers, all over the world, are known to quickly change their research interests in order to take advantage of such research funds.

The selection of a research problem is based on the key criteria of: (1) interest; (2) expertise; (3) data availability; (4) relevance and; (5) ethics. These have been discussed above.

2. Functions of a Hypothesis.

A hypothesis is better defined in the context of the scientific method. Therefore in this section, we shall start by going back to the definition of research fundamentals. We have earlier defined research at the beginning of this article. Several categories of research can be quickly identifiable, depending on methodology (empirical vs theoretical), applicability (fundamental vs applied) and locality (academic vs industrial). Empirical research implies experimental studies and observations made using laboratory instruments and equipment and the measured and observed quantities are used to evaluate existing theories/laws, and/or can provide the facts for developing theories/laws (theoretical studies). Theoretical Research involves and implies the development of theoretical models (on paper), with a major goal being the formulation of a theory/law that synthesizes/ generalizes the data describing the phenomena of the nature (physical or organizational) to be studied. Theories/laws so generated shall be used to predict other results, which in turn shall be verified by empirical measures. A general classification of based on the goal (or applicability) of research recognises fundamental (or basic) and applied research. Fundamental research covers studies which are pursued primarily for uncovering the functioning of the nature, while applied research extends the findings from fundamental research to useful technologies that can be used to develop products which respond to society's needs. Fundamental research is essentially tolerated to a large degree, principally because experiences have demonstrated that discoveries made through these will deliver, sooner or later, certain useful foundations for progress. One may also acknowledge a basic difference in research based on location. As a rule, research at academic institutions, tend to be more fundamental than applied (although these realities are changing increasingly) while industrial research, by its very nature tends to be more applied in nature, as its tendency is more towards the production of industrial products.

Fundamental research (or any other form of research) is not possible in the absence of a clearly organised (or laid out) research framework. This framework is provided by the scientific method, the understanding of which has been described as essential for success and productivity in science (Gauch, 2003). As the "The process of scientific thinking depends both on making careful observations of phenomena and on inventing theories for making sense out of those observations" (AAAS, 1989), the scientific method provides researchers with the framework for scientific research. The invention of theories starts with the invention of hypotheses. Hypotheses are statements that are assumed to be true for the purpose of testing their validity. These statements must be either true or false and must lend themselves to empirical testing and, as a result, empirical confirmation or disconfirmation (rejection). For example, in classical experimental research, a researcher identifies a natural phenomenon that he/she intends to study; the researcher forms/invents at least two hypotheses to "explain" the natural phenomena under study; these are then extrapolated by the researcher until he/she finds an experiment which can distinguish between the two hypotheses. Data obtained from the performance of the experiment will then inform the acceptance or rejection of one or more of the hypotheses, depending on which of the hypotheses is contradicted by experimental results. Results of experiments to test hypotheses may also lead to their revision, or replacement with entirely new hypotheses which, in turn, will need to be tested experimentally. Hypotheses are key elements of the research enterprise. They express the elements of a research problem. As a result, hypotheses define the set of experiments which need to be conducted during the research. In practice, a research topic contains more than one unknown. During the scientific or technologic research process, the researcher drives for the clarification of these unknowns with

irrefutable evidences or proofs. It is important that these hypotheses be well-posed. Several forms of hypotheses can be identified. Declarative hypotheses express the relations between variables that investigators expect to converge, e.g. “there’s significant increase in amylase production in Bacteria grown on impure starchy substrates compared to those grown on pure starches”. Negative (null or falsified) hypotheses express non-existence of relations between experimental variables. They don’t necessarily represent the expectations of the researcher but are often used due to the fact that they naturally fit with statistical techniques, many of which aim at measuring the unlikelihood (that a found difference be higher than zero). An example of a null hypothesis is “The increase in amylase production by bacteria grown on impure starchy substrates is not significant compared to those grown on pure starches”. Interrogative hypotheses are structured in the form of questions and interrogate what is possible between experimental variables, e.g. “is there any significant difference in amylase production between bacteria grown on impure starchy substrates and those grown on pure starches?”.

Results of experiments demonstrate the validity of a hypothesis. Also hypotheses do not necessarily have to be true to be valid, providing information which contributes to the truth is enough. In practice, many hypotheses have been validated because they provide significant information despite their falsified results.

Hypotheses provide the framework for designing experiments for theories, through affirmation or invalidation/disqualification of specific predictions established from this theory.

The following criteria have been identified for designing good hypotheses; (a) their “raison d’être”; (b) their testability or their ability to be validated; (c) their conciseness of description; (d) their clarity; (e) their simplicity; and (f) their briefness. Investigators must have irrefutable reasons, based either on the theory upon which the research is defined, or on observed facts, to define a hypothesis. The facts or theoretical elements serving the definition of a hypothesis can come from:

- Literature review,
- Detailed knowledge dealing with the research’s related works.
- Normally, a hypothesis must not be in conflict with major theories, although in reality, innovation often occurs from conflicts with accepted standards or theories.

Hypotheses must be “testable”, which means that they must be measurable, quantifiable, and observable.

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