International Handbook on Innovation
Edited by Larisa V. Shavinina

Problem Generation and Innovation

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Abstract: Most of the literature on innovation concerns fostering better solutions to existing problems. Many innovators in the sciences and engineering argue, however, that problem generation is far more critical to innovation than problem solution, involving not just a thorough grasp of what is known (epistemology), but of what is not known (nepistemology). The proper definition of a problem gets an innovator more than half way to its solution; poorly posed questions divert energy, resources, and ideas. This chapter explores how problem definition and evaluation act as catalysts for insight and examines strategies used by successful innovators to generate productive problems.

Keywords: Nepistemology; Ignorance; Problem generation; Problem evaluation; Types of innovation.

Introduction: The Nature of Ignorance

Most people believe that creativity and innovation, especially in the sciences and technology, are forms of effective problem-solving. I, however, believe, that creativity and innovation consist of effective problem-raising. People are creative only when they need to do something that cannot yet be done. Identifying, structuring, and evaluating problems in ways that allow their solution are therefore as important—arguably *more* important—than finding solutions. We must know what we do not know before we can effectively solve any problem.

One could, of course, argue that all of the important questions have already been asked, that ignorance is finite. One might believe that the number of questions that can be asked is limited so that ignorance decreases in direct correspondence to the increase in the volume of knowledge. One might, however, assert the opposite: that ignorance is infinite. The greater the volume of knowledge we accumulate, the greater the sphere of ignorance we can recognize around us. Every question breeds more questions without end. I favor the latter view. Every time someone in history has proclaimed that a field such as physics, medicine, philosophy or art has finally reached the end of its possible progress, a revolution has already been under way in that field, opening up unexpected vistas for exploration (Root-Bernstein, 1989, p. 45). For example, even as Lord Kelvin was preaching that physics was a closed book with no new surprises to yield, Einstein was inventing relativity theory and Planck was creating the quantum revolution. Indeed, Einstein wrote that the very idea that physics could ever become a closed field was repugnant to a physicist in the twentieth century:

> It would frighten him to think that the great adventure of research could be so soon finished, and an unexciting if infallible picture of the universe established for all time (Einstein et al., 1938, p. 58).

On the contrary, Einstein argued, in the struggle for every new solution,

> new and deeper problems have been created. Our knowledge is now wider and more profound than that of the physicist of the nineteenth century, but so are our doubts and difficulties (Einstein et al., 1938, p. 126).

This is the situation in every fecund field. New explanations and new techniques always create unforeseen sets of new problems. In consequence, what drives progress is not the search for ultimate knowledge, but the search for ever more wondrous questions.

Creative people in every discipline recognize the importance of generating or discovering new problems. That which we cannot yet do impels us to invention. “Recognizing the need is the primary consideration for design”, said Charles Eames (Anon, 2000, p. 4). He and his wife Ray were the first designers to utilize molded plywood, fiberglass, wire-mesh, and cast aluminum to make practical everyday objects such as furniture. Novelist Dick Francis says that whether you are trying to solve a mystery or write book, “You have to ask the right questions” (Francis, 1992, p. 288). Filmmaker Godfrey Reggio agrees. Commenting on his film ‘Koyaanisquatsi’ he cautioned viewers that it is not a solution to the world’s problems, but an unfolding question:

> You know, the question is really more important than the answer. I can frame the question, but I don’t know the answer (Kostelanetz et al., 1997, p. 251).

In fact, the more important the question, the more important the ideas to which it will give rise. Sir Peter Medawar, whose research made possible medical transplant technologies, has written that scientists interested in important discoveries require important problems. “Dull or piffling problems”, he says, “yield dull or piffling answers” (Medawar, 1979, p. 13). And Werner Heisenberg, of uncertainty principle fame, stated another oft-repeated principle known to all scientists: “Asking the right question is frequently more than halfway to the solution of the problem” (Heisenberg, 1958, p. 35). One of the keys to creativity according to all of these people lies in the study of problem recognition and generation. It is not what we know, but what we do not know that drives inquiry in every discipline. Learning how to question deeply and well is therefore, as Socrates made clear 2,500 years ago, one of the most important keys for unlocking hidden knowledge and one that opens doors in every field of endeavor.

Nepistemology: The Types and Origins of Ignorance

The philosophical study of what we do not know falls under the heading of *nepistemology.* Epistemology, as many people will be aware, is the study of how knowledge comes into being. Its complement, nepistemology, is the study of how ignorance becomes manifest. Despite the extraordinary importance of nepistemology, the field has little literature and even fewer practitioners. For some unfathomable reason, the existence of problems is taken for granted and their nature and origins generally ignored. In a world in which education, business, and government are focused on problem-*solving*, it is worth remembering that problems must be invented just as surely as their solutions. Because good solutions can be generated only to well-defined problems, problem recognition and problem generation is arguably a critical step. Failure to ask questions results in the stagnation of knowledge. Asking the wrong question yields irrelevant information. Asking a poorly framed question yields confusion. Asking trivial questions yields trivial results. However, learning to recognize what we do not know, developing the skills to transform ignorance into well-defined questions, and being able to identify the important problems among them, sparks innovation.

Few programs in history have been designed specifically to teach problem generation. Socrates, of course, asked questions but it is not clear that he taught his students how to ask their own. The Bauhaus, a design movement based in Germany during the 1920s and early 1930s, certainly incorporated problem generation as a fundamental part of its Foundation Course. When assigned to design a table or paint a painting students were challenged to begin not by seeking answers, but by seeking questions: “what is a table? . . . what is a painting?” (Lionni, 1997, p. 166) Only by questioning the assumptions they brought to their work, they were taught, was it possible to produce innovative answers. Similarly, English sculptors such as Henry Moore, Jacob Epstein, and Barbara Hepworth reinvented sculpture by “asking themselves the question: ‘What do we mean by sculpture?’ ” (Harrison, 1981, p. 217). By returning to so-called primitive techniques of direct carving and structural rather than realistic forms, they discovered new questions about the nature of art and thereby new answers. Learning to ask the right question always opens up new possibilities (Browne et al., 1986).

One of the very few contemporary questioning programs is ‘The Curriculum on Medical Ignorance’ founded by surgeons Marlys and Charles Witte and philosopher Anne Kerwin at the University of Arizona Medical School in Tucson, Arizona. The purpose of the program is to teach medical students how to recognize their own ignorance and that of other doctors in order to better define what still remains mysterious and take a first step towards enlightenment. The Wittes and Kerwin have identified six basic types of ignorance. (Witte et al., 1988, 1989). First, there are things we know we do not know (known unknowns, or explicit ignorance). A second type of ignorance consists of things we do not know we do not know (unknown unknowns, or hidden ignorance). The next kind of ignorance is that consisting of things we think we know but do not (misknowns, or ignorance masquerading as knowledge). Fourth, ignorance may be found among the things we think we do not know, but we really do (unknown knowns, or knowledge masquerading as ignorance). Next, some ignorance persists due to social conventions against asking certain types of questions in the first place (taboos, or off-limits ignorance). Ignorance may also persist due to refusal to look at some types of answers to perfectly legitimate questions (blinkers, or persistent ignorance).

Two essential points must be understood about this typology of ignorance. First, each distinct type of ignorance requires a different set of techniques in order to reveal itself clearly. Second, problem recognition and invention are active processes. Proficient questioners are those who can draw upon a wide range of problem-finding tools and apply them appropriately to diverse questioning contexts. If nothing else, the Curriculum on Medical Ignorance reveals the poverty of simply asking 'why' or 'how' or 'what' for every observation one makes.

Explicit ignorance, the things we know we do not know, consists of questions and problems that experts in a field formally recognize because current explorations repeatedly fail to provide useful answers. Explicit problems from diverse fields include questions such as the following: How does the immune system differentiate 'self' from 'nonself?' Should animals have the same sorts of rights accorded to human beings? Does art progress or merely change to reflect the values of each new generation?

Explicit ignorance can be discovered using a wide range of proven approaches. One is to question the assumptions underlying the question. Some questions turn out to be unanswerable because they contain implicit concepts that are not valid. For example, what do we mean by 'progress' and how can we apply that term to art? What is a 'right?' Do 'rights' necessarily entail responsibilities? Can an animal that cannot understand or implement its obligations under the law be accorded 'rights' or must such rights reside in a legal guardian, as is the case for mentally incompetent human beings? Any problem, like these, that exists for any period of time demonstrates that the experts not only do not have the answer, but do not have the question. Persistence of questions implies a lack of proper question formulation. Questioning the question itself can often yield insight.

A second way to identify explicit ignorance is to focus on paradoxes. Paradoxes exist only where two or more well-formulated sets of knowledge come into conflict and thus represent the locus of the unknown. Such loci should be embraced, for where there are paradoxes, there is, as the physicist Neils Bohr repeatedly pointed out, hope of progress (Moore, 1966, p. 196).

A third method for recognizing explicit ignorance is to learn how to perceive the sublimity of the mundane. 'Sublimity of the mundane' refers to the inordinate beauty manifested in everyday things. Unfortunately, as practitioners of every discipline are all too well aware, that which is commonplace is easily overlooked or, in the terms of artist Jasper Johns, is recognized without being seen (Root-Bernstein et al., 1999, p. 32). Whether a person sets out to perceive what makes a flag flag-like, as Johns did in his art, or to ask why a banana turns brown but an orange does not, as biochemist Albert Szent-Gyorgyi did, such everyday problems, precisely because they are so commonplace, will also turn out to be fundamental. Johns's art has provided new insights into how we see, while Szent-Gyorgyi's researches on banana browning resulted in the discovery of vitamin C, for which he earned a Nobel Prize.

A final method for *revealing* explicit ignorance is to err. As Francis Bacon once wrote, truth comes out of error more rapidly than out of confusion. A clear mistake does far more good to any investigation by identifying the specific nature of the problem being addressed than any other method. Hence the trite (but true) aphorism that we learn best from our mistakes. People who never err not only never succeed, but have no mistakes (and hence generate no problems) from which to learn.

Hidden ignorance, the things we do not know we do not know, is perhaps the most difficult form of ignorance to discover precisely because it is not explicit. One of the most important characteristics of hidden ignorance stems from the fact that human beings tend to account for any phenomenon in any field in terms of what they already know. Thus, many forms of ignorance remain hidden simply because people are too proud to say the simple sentence, "I do not know". Instead, we tend to make up stories to hide our ignorance. For example, prior to the discovery of electricity, every culture attributed lightning to the wrath of the 'gods' rather than simply admitting ignorance. Similarly, prior to the discovery of germs, infectious diseases were attributed to bad air, bad water, and bad habits. No medical expert in any culture seems to have had the courage to simply admit that they did not really know how diseases spread.

To discover hidden ignorance requires very different methods than to address explicit ignorance. Hidden ignorance must be surprised. One way to surprise ignorance according to Sir Peter Medawar is to design research to challenge expectations. (Medawar, 1979, passim) His advice is predicated on the philosophy of science espoused by Sir Karl Popper, *who* maintains that theories can never be proven (because the observation you have not yet made may invalidate your position), but theories can always be *disproven*. The object of any research project in any discipline should therefore be to find the evidence that will disprove one's expectations. Physicians, for example, often call for tests to 'rule out' a possible but unlikely diagnosis. The specific way in which expectation is disproven will then reveal the nature of one's unsuspected ignorance.

Another method related to challenging expectation is to look for information where no reasonable person would look. There are many ways to implement this strategy but each entails to a greater or lesser degree the risk of appearing to be foolish or even crazy. The most acceptable method is to run many highly varied and even outrageous controls with any experiment. Winston Brill, a member of the National Academy of Sciences (USA), for example, recounts that his most important breakthrough occurred when he was designing a pesticide based on a particular theory of how its structure should look. He synthesized several versions of his compound in what he predicted to be active and inactive forms and also took a few chemicals off his shelves at random to use as controls. None of the chemicals he had designed as a pesticide worked well, but one of his random choices was a stunning success, representing a new class of pesticides that no one could have predicted in advance (personal communication). Only by doing the unexpected can the unforeseen reveal surprises.

Another approach to revealing unknown unknowns is purposefully to turn things on their heads to yield a new perspective. The artist Wassily Kandinsky, for example, unthinkingly turned one of his early representational paintings upside-down and left it for several weeks. Re-entering his studio one evening, having forgotten about the painting, he suddenly became aware of it, illuminated by the setting sun, but did not recognize it or its content. All he saw was form and color. Suddenly he realized that it might be possible to create totally non-representational art, something that even the abstract painters of that time had not yet considered. By viewing his own work literally upside down he discovered that there was a whole realm of art that no one had realized was there, let alone begun to explore (Herbert, 1964, 32ff)

Playing contradictions (sometimes known as playing the devil's advocate) is another fecund source of unknown unknowns. Nothing reveals how little we know than to take a proposition such as 'the world is spherical' and assert instead that 'the world is flat'. As the novelist H. G. Wells pointed out a century ago, very few are the supposedly educated members of society who are able to produce evidence or experiments (especially ones that do not resort to the data gathered from space craft) that will clearly distinguish one of these possibilities from the other. (Wells, 1975, pp. **32–25**) That which is obvious often becomes **inobvious** when its opposite is asserted, thereby raising questions and possibilities that would never otherwise come to light.

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Finally, one can reveal hidden ignorance by drawing out the most absurd implications of any idea. Many children excel at this strategy, taking anything an adult says and applying it literally—too literally!—to any situation they meet. Much satirical literature ('Candide', Voltaire's send-up of rosy philosophical optimism as espoused by Leibnitz and Pope, is a good example) uses this strategy to reveal the limitations inherent in popular philosophies. What extrapolation of this sort reveals are the boundaries beyond which an otherwise quite reasonable proposition or method fails. The point at which it fails is often the point at which our ignorance begins.

Ignorance masquerading as knowledge, or misknowns, presents yet another class of problems. A classic example from medicine was the belief, rampant among physicians for most of the twentieth century, that stomach ulcers are due to mental stress or eating spicy foods or alcohol and can be treated only by a bland diet and relaxation techniques. Stomach ulcers are, in fact, caused by the bacterium *Helicobacter pylori* and can be cured by a proper regimen of antibiotics. Stress, alcohol and spicy foods exacerbate the consequences of the bacterial infection. Thus, secondary factors were confused with primary causes.

Several methods exist for discovering that the king is wearing no clothes. One is to doubt all correlations. Everyone is taught that correlation is not causation, but in practice this warning is all too often ignored. In the case of ulcers, because spicy foods and alcohol *exacerbated* the symptoms, they appeared to be so highly correlated with the onset of ulcers that they were mistakenly identified as causative agents. Evidence that many people eat spicy foods and drink lots of alcohol without getting ulcers was ignored.

A second method for identifying misknowns is to question habit. For centuries, painters habitually propped their canvases up on easels in order to work on a surface perpendicular to their view. It took the courage of Jackson Pollock to take the canvas off of the easel and place it on the ground to experiment with the artistic possibilities of working in a new way.

Two additional methods also exist for identifying misknowns. Perhaps the most important is to doubt most those results or findings that accord best with your preconceptions. Those results or observations that best fit our preconceptions and expectations are the least interesting and yield the fewest questions and insights. Ignore them. They do not yield problems. The things that are the most interesting in terms of revealing ignorance are those that are the most disturbing or which conflict most clearly with strongly held beliefs or practices. Therefore, pay attention to the heretics, revolutionaries, and people stepping to their own drummer. Many of their ideas will be wrong, but many of the problems they reveal will be valid.

A related method for identifying misknowns is to collect anomalies. Anomalies are phenomena and observations that do not fit within the theoretical or explanatory structure of a field. They are the things that experts ignore as inconvenient nuisances. As philosopher Thomas Kuhn pointed out in his book *The Structure of Scientific Revolution* (1959) every great discovery has resulted from an individual paying attention to the anomalous results that most practitioners of a field have refused to countenance or have actively ignored because anomalies generate problems.

Finally, try doing the impossible. There are two kinds of impossible things: those that nature will not allow us to do (e.g. perpetual motion machines) and those that our predecessors have found beyond their means. The history of medicine is full of procedures that were declared to be 'impossible', such as blood transfusions and transplants, that are now part and parcel of everyday clinical practice. Indeed, the inventor of the instant camera and its Polaroid film, Edwin Land, advised innovators not to "do anything that someone else can do. Don't undertake a project unless it is manifestly important and nearly impossible" (Root-Bernstein, 1989, p. 415).

Next we must deal with things we do not know that we know, or hidden knowledge. There is a story that one of the great eighteenth-century mathematicians sat down one day to try to calculate the perfect dimensions of a container for storing and shipping alcoholic beverages such as beer or wine. After taking into account the available materials, limitations imposed by the fact that the containers had to be carried by men from one place to another, and so forth, he generated his solution—only to find that he had described the wooden barrels already in use. Similarly, a recent study of medical treatments has found that many of the most 'innovative' are based on folk remedies that have been around for millennia. Just for example, honey pastes have been used since at least Babylonian times to treat severe wounds and burns and have recently been introduced in many hospitals to treat infections and ulcerations that are beyond the scope of current antiseptic, antibiotic, and surgical treatments. As both these cases show, the basic problem underlying hidden knowledge is that it exists either among people who are not considered to be valid sources of knowledge by disciplinary 'experts' or resides in historical documents bypassed by subsequent developments in the name of 'progress' (Root-Bernstein et al., 1997, Chapters 3 and 12).

The causes of hidden knowledge suggest solutions. The most obvious is to go outside the boundaries of acceptable disciplinary sources for information. Charles Darwin broke all of the standards of science in his day (not to mention social conventions) by writing to farmers, pigeon fanciers, horse breeders, and other non-scientists in order to discover the accumulated wisdom about artificial selection that existed outside of the academic community. The physicians who rediscovered the modern therapeutic uses of honey paid attention to non-traditional practitioners of their art such as family folk medical traditions or local traditional healers. Similarly, many innovators, such as the cubist artists Brach and Picasso and the modern composers Richard Reich and David Glass, having mastered formal Western techniques during their early training, then turned to non-academic sources of inspiration such as traditional African and Hindu arts.

Albert Szent-Gyorgyi, the Nobel laureate who discovered vitamin C, has suggested that another general way to reveal problems of unknown knowns is to go back and re-do classic experiments from 50 or 100 years ago using modern techniques (Root-Bernstein, 1989, p. 412). These re-creations, he says, almost always yield novel insights because the investigator brings to them different problems and theories that change their interpretation. Artists such as Henry Moore have similarly used ancient techniques such as direct carving, invented by Egyptian and Greek sculptors, to revolutionize modern art by applying it to new materials.

Finally we reach persistent unknowns (taboos) and unacceptable or blinkered knowns. Both classes of ignorance are caused by the refusal to ask certain types of questions or to entertain certain types of answers. These classes of ignorance often result from social taboos or customs and sometimes from economic or political considerations as well. For example, many women living in underdeveloped nations (especially those dominated by Muslim sects) may not ask "How can I control my fertility?" Artists in the United States have met similar constraints on their ability to ask questions such as whether man has been formed in God's image or vice versa. And biomedical researchers studying AIDS have found it so unacceptable to ask whether the human immunodeficiency virus explains everything about AIDS that many have lost their funding and positions. The characteristic of taboo questions is that one risks one's career or even one's life by asking them.

Certain types of answers may also be avoided just as adamantly as certain kinds of questions. For example, alternative medical therapies are used by more than one third of European and American adults, but they are rarely subjected to the kind of double-blind controlled clinical trials that are required of pharmaceutical drugs. Both taboos and blinkered knowns help explain the situation. In the first place, it is not in the interest of purveyors of alternative medicines to have to meet the standards of either efficacy or purity set for pharmaceutical agents. In no case would meeting such standards increase their market share or profit margin and in many cases the standards might eliminate their product from the market. Thus, it is in the interest of these purveyors to foster the persistence of ignorance about their products. However, it is too expensive for Food and Drug Administrations or pharmaceutical companies to seek answers to issues of efficacy and safety. The only motive that these institutions have to test alternative medicines is to accredit them as prescription drugs. It costs, however, approximately one quarter to one half a billion U.S. dollars to obtain Food and Drug Administration approval for a new drug. In order to make such an investment worthwhile, a pharmaceutical company must be able to obtain patents on the drug and its manufacture so that other companies cannot take advantage of the work they have put into proving safety and efficacy. Unfortunately, alternative medicines are, by their nature, non-novel agents already in the public domain, and hence unpatentable. Thus, there is no business incentive to find answers to the problems posed by alternative medicine usage (Root-Bernstein, 1995, passim). We therefore persist in our ignorance about such therapies and will do so until other incentives make it worthwhile for us to seek answers.

Additional examples of blinkered answers can be found in various approaches to AIDS adopted around the world. In Africa, for example, it is against the social mores of many men to use condoms, although they know that condoms are the most effective method of protecting themselves against AIDS. In some Muslim countries, AIDS-prevention counselors are not allowed to mention the fact that AIDS is spread most commonly by homosexual men and female prostitutes because neither group may be mentioned in conversation or print. Thus, programs that have proven to be very effective in controlling AIDS in some countries cannot be used in others because of such taboos.

Because the roots of taboo questions and blinkered answers are social, the methods for addressing it are also social—or, more accurately, anti-social. One of the most noteworthy aspects of many creative acts, whether in the sciences, humanities, social sciences or the arts, is that the people who carry them out are labeled heretics, provocateurs, misfits and worse. The greatest questioner of all time, Socrates, was put to death for the anti-social implications of his questions. Galileo was charged with heresy by the Catholic Church for daring to question the Ptolemaic view of the universe that underpinned Church doctrine. Darwin became a pariah among fundamentalists of many religions for questioning whether God had indeed created man in His image. Margaret Sanger and Marie Stopes, the revolutionaries who gave Western women contraceptive knowledge, each went to jail more than once. So did the many suffragettes who worked to give women the vote, simply for asking why women should not be able to do the things men do. One must have the courage of such people in order to break the taboos that prevent most of us from asking certain types of questions or facing the consequences of certain types of answers. The more dangerous the questions are to people in power, the greater the courage needed to ask them.

Problem Evaluation and Classification

Now, once problems have been recognized or generated, they must be evaluated. Just as there are different kinds of ignorance, they yield a great diversity of kinds of problems. Problems can be classified according to *type*, *class*, and *order*. Type refers to the technique necessary to *solve* a problem. Class refers to the *degree* to which the problem is solvable. And *order* refers to the placement of a problem within the context of the universe of other related problems. While each of these concepts may initially sound very abstract and academic, each is, in fact, extremely useful in practice. Classifying problems allows each to be linked to an appropriate method for achieving a solution and allows links between problems to be explored explicitly.

Just as it is true that not all forms of ignorance can be recognized or discovered in the same way, not all problems can be solved using the same methods. There are, in fact, at least ten types of problems, each distinguished by the manner in which it must be addressed in order to achieve a solution. The ten basic problem types are: (1) problems of definition; (2) problems of theory; (3) problems of data; (4) problems of technique; (5) problems of evaluation; (6) problems of integration; (7) problems of extension; (8) problems of comparison; (9) problems of application; and (10) artifactual problems (Root-Bernstein, 1982, *passim*; Root-Bernstein, 1989, p. 61). Readers will immediately recognize a significant overlap in the terminology and concepts embedded in this typology of problems with the typology of innovation types proposed by Sternberg et al. in this volume. The type of problem an innovator chooses to address therefore determines the type of innovation she or he produces.

Problems of definition concern the purity of the language and its meaning as used by any given discipline. Representative issues include ‘what is velocity’ or ‘what is a legal right’ or ‘what is beauty’. Such questions cannot be addressed by experiment or observation or any form of research. Such definitions may be determined axiomatically in the same way that we accept definitions of basic concepts such as the numbers one, two, three, etc. or when we state that velocity is distance traveled divided by the time of travel. Definitions may also be addressed by means of a process, such as the legal system that determines whether something is a right.

Problems of theory or explanation involve any attempt to find or make sense of the relationships in existing data or observations that are currently incomprehensible or anomalous. Theory problems require the recognition or invention of a pattern that makes coherent sense of available information.

Problems of data involve the collection of experiences or information relevant to addressing some particular kind of ignorance. Relevant techniques include observation, experiment, and any kind of playing with materials that yields novel data.

Technique problems are those that concern the manner in which novel data, observations or effects are to be achieved. Such problems generally require the invention of instruments, methods of analysis or display, or techniques that allow new phenomena to be observed or created.

Evaluation problems arise when it is necessary to determine how adequate a definition, theory, observation or technique may be for any given application or situation. For example, is any particular observation an anomaly or an artifact of the way in which the observation was made? Evaluation problems require the invention of criteria and methods for evaluation. One might, for example, consider the entire field of statistics to be a set of solutions to evaluation problems.

Problems of integration often involve contradictory or paradoxical situations in which it is not obvious whether two theories, data sets, methods, or styles of research are compatible. For example, is there any benefit to teaching art to science students and science to art students? If one were to do so, how would one go about it? Integration problems generally require the rethinking of existing definitions and theories to determine what hidden assumptions may provide bridges between apparently disparate concepts.

Extension problems concern the range of possible uses to which any method, technique, theory, or definition can be put. Ray and Charles Eames, for example, experimented widely with plywood to find out what its possibilities and limitations were as a design material. Such extensions require extrapolations from existing practices and ideas to unknown ones in the form of predictions, play, and testing. The object in addressing extension problems is to discover the boundaries limiting the valid use of any particular solution methodology.

Problems of comparison arise when more than one possible solution exists to a problem and one needs to determine which is the best. Explicit criteria must be generated to make such comparisons. Such criteria are embodied in logic and in the use of analogies. While it may be relatively obvious how to compare the relative merits of two types of glue, for example, it may be very difficult to determine whether glue, staples, screws, nails, or hooks are the best means for attaching any two objects together. Unless one has a good understanding of the nature of the problem to be solved, criteria for generating such comparisons cannot be made.

Problems of application attend any attempt to extend a solution from one problem instance to another. Chemical engineers, for example, are highly sensitive to the fact that reactions that occur quickly and nearly completely in a test tube may be disasters when scaled up to hundreds of gallons. Similarly, while the principles of flight are the same for a model airplane and a jumbo jet, the application of those principles differs in obvious ways. Thus, problems of application often involve modeling or scaling-up issues, or the transfer of a solution from one discipline to another.

The final type of problem is the artifactual problem. Artifactual problems arise from misunderstandings. Such misunderstandings can arise from inaccurate or misleading assumptions, as in the classic lawyer's question: "When did you stop beating your wife?" Or such misunderstandings can arise from ignorance. For example, I was once asked by a reviewer of a paper I had written to provide a correlation coefficient for a trait that had no variance: all of the members of one group had the trait; none of the members of the other group had it. By definition, no correlation coefficient can be calculated in such a case and I had to point out that the problem was not valid.

Just as there are many types of problems, there are many classes of problems defined by the degree to which a complete answer can be achieved. Problems can be classified by whether they are: (1) unsolvable; (2) solvable only by approximation; (3) exactly solvable; (4) solvable as a class; or (5) solvable only for particular cases. Knowing the degree to which a problem can be solved is essential not only for evaluating the degree of precision that one can expect in an answer, but also for determining how important a problem may turn out to be.

Although practitioners of every field will recognize that some problems are amenable to more precise or general solutions than others, the only professionals who explicitly classify their problems according to such criteria are mathematicians (Wilf, 1986, pp. 178–221). In mathematics, problems are described as 'P', 'NP complete' and 'NP incomplete'. 'P' problems are those for which it can be demonstrated that the entire class of such problems can be solved using a common algorithm, or general problem-solving technique. An example of such a problem is whether any two numbers, x and y, are divisible by a common factor. It is possible to prove that any such problem can be solved. Thus, mathematicians can prove that 'P' problems are solvable even before addressing any particular manifestation of such a problem and before generating any particular solution. 'NP' problems, in contrast, are those for which it is impossible to solve the class of such problems, but for which individual solutions may be possible. One can always prove that any solution to an 'NP' problem is valid, but not whether such a solution exists in advance of actually finding it. An example is the classic traveling salesman problem in which a salesman must visit a large number of cities in a country and wants to do so in less than a certain number of days. There may or may not be a solution that satisfies the salesman's needs. Worse, there is no algorithm that will allow him to find out. He must generate possible solutions to the problem and hope that one satisfies his criteria. Worse yet, for most such traveling salesman problems, the number of possible solutions is too large to explore in any reasonable way, so that one can never prove that one has achieved the optimal solution to the problem, even if one finds a viable solution. The best one can do is to generate many solutions and compare them, looking for the one that is best of the batch of solutions on hand. Most real-life problems are of this NP type and require the generation of many possible solutions that are compared for their effectiveness. (See Root-Bernstein, 'The Art of Innovation', in this volume.)

Once an 'NP' problem has been solved, it may or may not yield general solutions to other such problems. In the case of the traveling salesman problem, each case must be solved individually within the entire class of such problems. Mathematicians call such 'NP' problems 'NP incomplete'. The class of problems that can be solved by the same algorithm as an already solved 'NP' problem is called the class of 'NP complete' problems. Such NP complete problems are not P problems because it is cannot be shown in advance that they have a solution—they must be found

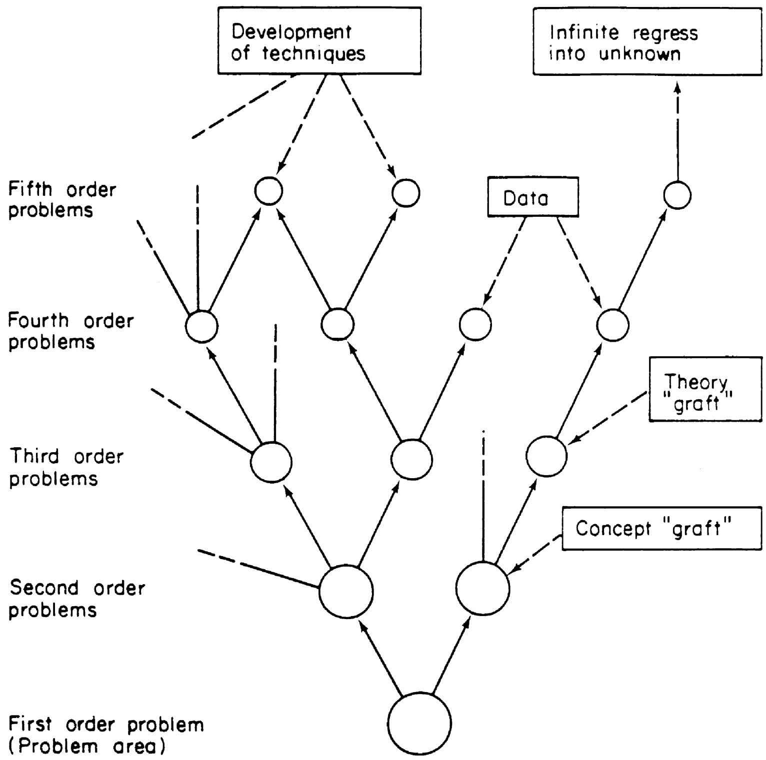

Figure 1. **Caption copy not supplied**.

one case at a time by trial and error. Moreover, as with NP problems in general, no one can ever prove that the optimal solution has been achieved for any member of an NP complete class. A better mouse trap might still be possible.

Mathematicians also recognize that there are problems that are neither 'P' nor 'NP', but are demonstrably unsolvable. This is the basis of Goedel's proof, which demonstrates that it is impossible to devise a mathematical system that has no unprovable assumptions. It has also been proven that it is impossible to trisect an angle using a compass and ruler. One might call such problems 'I', for impossible.

The existence of 'I' problems such as trisecting an angle raises another important point concerning the nature of problems. I have been stressing the principle that poorly defined problems yield confusing answers, whereas well-defined problems yield clear answers. One of the clearest possible answers to a well-defined problem is that it is not solvable at all. As astrophysicist Gregory Benford has cautioned, "The existence of a well-defined problem does not imply the existence of a solution" (Benford, 1989, p. 155). For example, many people have desired to create perpetual motion machines. The criteria defining the problem are extremely well defined: such a machine must be capable of creating more energy than it uses. Stated as a question, the problem becomes: how does one create energy *de novo*? Anyone who has physics knowledge knows that, while this is structurally and logically a well-formulated question, it is not a reasonable question. To create energy *de novo* would violate the laws of thermodynamics. Thus, despite the fact that the problem can be stated exactly, it can also be shown that the problem has no solution. The question of whether God performs miracles is of the same class because miracles are, by definition, metaphysical or supernatural events beyond human comprehension, thereby placing any evidence beyond our ability to validate or replicate it. Such questions are therefore beyond rational discourse and belong, properly so, to questions of faith.

Finally, problems must be evaluated in terms of their order, which is to say with regard to their relationship to other problems. The most graphic and useful way of exploring the relationship between problems is to generate a hierarchical tree displaying their logical and methodological connections (Fig. 1). The logical problem tree was invented by biologist James Danielli to illustrate the fact that only very rarely is any problem addressed in isolation (Danielli, 1966, *passim*; Root-

Bernstein, 1989, p. 63). A problem of theory may require the invention of a new method for gathering relevant data and need to be solved in order to determine the feasibility of some practical application. An ordered problem tree allows investigators and inventors to determine which problems need to be solved in what order, using what methods. More importantly, a problem tree illustrates the degree to which any specific problem is more or less general and more or less connected to other problems of various types and classes.

Ordered problem trees are particularly useful for evaluating claims of significance. It is common for investors in new technologies and for funding agencies offering grants to require inventors to provide a justification of the utility of the work they propose to carry out. Thus, a cell biologist may justify the utility of his or her studies of cell division by the fact that cancer cells divide abnormally and therefore research on cell division may yield a cancer cure. Such claims may or may not be accurate, since the number of problems that need to be solved to link basic bench research with a clinical application may be so large as to be beyond reason. The degree of ignorance that must be addressed can only be evaluated when an ordered problem tree has been constructed to demonstrate the types, classes, and number of related problems that define the general problem area.

Problem Evaluation

An understanding of how problems are generated and the various types, classes, and orders into which they can be categorized provides useful information about how important or trivial any given problem is likely to be. Once again, there is virtually no formal research on this subject, but it is probably fair to say that what are called 'important' problems are those that are completely solvable for a large class of cases, and that this property of solvability is directly related to the ability to construct a very strongly connected problem tree that links many types of problems robustly. But the fact that problem evaluation requires a detailed understanding of where any given problem exists within a tree of other linked problems should warn us that there is nothing intrinsic to any given problem that makes it important or not. A seemingly trivial problem may provide the key to solving an entire problem tree of great significance; or a seemingly important problem may provide only a trivial answer because it can yield only a specific answer that connects with and informs no other problem. Many problems that may appear to be intrinsically interesting or worthy of attention (e.g. world peace) may be dependent on so many sub-problems of such an intractable nature (local economic conditions and resources, cultural habits, education, etc.) that there is no practical way to address them. Working directly on sub-problems may be a more practical goal.

However, the irresolvable nature of general or even specific problems should never be a barrier to problem-generating activities. The most important problems are always those that need to be solved and cannot be. Simply perceiving the detailed nature of such nested problems can be of practical value in and of itself. One of the strategies employed by creative people in every field is to construct a problem tree in order to identify the critical problems that cannot yet be addressed and, having identified and characterized these gaps in our knowledge, wait until answers are supplied by someone else. When these sub-problems are finally addressed, the problem tree becomes complete, and the importance of what may have appeared to be very trivial, individual problems suddenly takes on vast significance. One of the characteristics of the most innovative people is to discover the world in a grain of sand.

Another characteristic of great questions is that they are bold or even dangerous and it takes at least a modicum of bravery or bravado to pose them. Solving daring problems is of course important, but daring to pose them in the first place is still the key to making breakthroughs in any field. Many people are capable of solving well-defined problems, since solutions, as Einstein and his collaborator Leopold Infeld wrote,

> may be merely a matter of mathematical or experimental skill. To raise new questions, new possibilities, to regard old problems from a new angle, requires creative imagination and works real advance in science (Einstein et al., 1938, p. 5).

Thus, learning how to pose the most insightful, provocative, and challenging questions is surely an art as much in need of training and practice as those arts devoted to problem-solving.

But in the end, the creative urge to generate problems comes down to motivation. What makes an explorer in any discipline is their attraction to the mysterious, the incomprehensible, the paradoxical, the unknown. As Albert Szent-Gyorgyi, a Nobel laureate in Medicine and Biology wrote,

> A scientific researcher has to be attracted to these (blank) spots on the map of human knowledge, and if need be, be willing to give up his life for filling them in (Root-Bernstein, 1989, p. 407).

Fellow Nobel laureate and physicist I. I. Rabi agreed: "The only interesting fields of science are the ones where you still don't know what you're talking about" (Root-Bernstein, 1989, p. 407). Thus, the most creative people are always explorers quite literally in the way that Magellan, Columbus, or Lewis and Clark were explorers, aiming for the regions of our most manifest ignorance.

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