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FIRST STEPS IN THE CONDUCT OF INQUIRY:

HOW TO GET RESEARCH STARTED*

Paul J. Gordon

Indiana University

FIRST STEPS IN THE CONDUCT OF INQUIRY

First steps in the conduct of inquiry are the focus of this statement. Help through the first stages of getting research started is the concern.

The Original Impetus and Primary Focus

The original impetus was recognition many years ago that undergraduate and graduate students, young faculty members, staff people in business and government, even consulting and advisory people frequently seemed confounded in their efforts to get started on problem finding and problem solving.

Closer observation suggested more difficulty with problem finding than with problem solving. For a time, my own hunch was that some people more than others probably have a yen and a capacity for coping with poorly-structured and ill-defined beginnings. This may be true. It may even be that such people have less interest, even less talent for other facets.

Bit by bit, however, it also dawned that there was and we continue to have available far more written on broad questions of science and specific rigors of hypothesis testing than on the middle and difficult stage of developing theoretical foundations, questions, problems and hypotheses in the first place. It was then and after further experience in guiding research efforts that I wondered whether it might be possible more effectively than up to that point to extend the capacities and successes of larger numbers of people.

My concern was and continues to be not only the tragedy of "all-buts" who never finish doctoral dissertations and then feel burdened for years afterward to explain to those who care not. There is the additional tragedy of those who end up with a distasteful required experience with no research or publication thereafter.

Why should there be such a heavy investment supposedly in learning to produce a result never thereafter produced again? Whether judged in educational or investment terms, the human as well as the material waste seems difficult to justify. Though many factors may be involved, the case for the concentration of this treatise seems impressive.

The relative lack of attention to the early stages and the frequent lack of capacity on the part of mentors more fully to assist in the early stages together seem to produce more floundering, more anxiety, more self-doubt, more sense of inadequacy, more misunderstandings, more delays, more incompletes, more manufacture of needless busy work to keep distracted, more cost and trauma and resolve never to tangle with inquiry again than anyone needs.

Surely, this cannot be an effective planned way to weed out those less suited to a life of investigation or scholarship of one sort or another. And surely, while sadism may be among the human endowments, it cannot be so rampant as to explain the observations described in any sizeable number of cases.

At this point, my interest is not so much in the numerical count or in salvaging people after troubles arise. It is more in not letting things get to that pass in the first place. A more positive aim is to increase the zest as well as the capacity of those who do survive so that they will want to put their newly developed abilities to work again and again.

The primary focus, therefore, is on the first steps, the matter of getting research started, an area relatively neglected, an area of important need. Exploration of why the first steps frequently entail unexpected setbacks for newcomers should provide therapy and encouragement for people who have experienced difficulty and doubt. It should also invite concern for understanding the decision processes involved so that they may be repeated on later ventures with increased ease.

Major Purposes, Premises and Applications

Two purposes guide the total presentation. One is to explore why first steps frequently seem so ill-defined and so taxing. From nothing to a proposal that has sufficient possibility to merit discussion is the relatively neglected area. The other is to set forth and to discuss the use of a series of questions precisely intended to help in filling that gap. Cycling and recycling the questions should result in generating decision criteria for limiting and specifying the area and the ingredients of any inquiry. The considerations and the discretion needed to put the questions to use in deciding on a particular piece of research are later discussed item by item.

The major premise is that first steps in the conduct of inquiry of any sort are probably the most difficult, the most important, the most consequential, the least understood, the least treated and those on which the least help is likely to be available.

They are difficult because the limits are not clear until one decides what they are to be and the decisions involved are difficult. They are important because early decisions set the terms of reference for any subsequent judgments. They are consequential because standards and cut-off points for any project are built into the early choices. They are not well understood because they involve elements commonly thought to be scientific as well as other elements of creativity and choice also involved in scientific work but not commonly and explicitly elaborated.

They are not too well treated because existing treatments dwell on scientific values, scientific methods of a recognized objective character and include as exemplary controlled testing of hypotheses and the like. More often than not, they bypass the nebulous and elusive matter of how questions and hypotheses are originated and how one gets from a zero point to a take-off point. Not too much help is readily available because those who might be helpful recognize that the necessary learning must take place through getting involved in the gut experience. The important education involved is ultimately independent self-education, not spoon feeding or solely following someone else's direction in a compliant way.

Yet, there is no ready reference that tells just what the experience for individuals will be, subjective as well as objective. Nor is it likely to be clear beforehand what array of talents will be needed to move from ill-structured wondering and self-doubts to well-structured problems and the confidence to nail things down in researchable terms. Reference to works already published may only compound any sense of inadequacy for the less initiated. The antiseptic residue that appears in published form frequently omits anything informative about the subjective experience of the first stages.

The foregoing observations, let us concede, probably have more validity in fields of inquiry where traditions of theory building and research design and ways of socializing newcomers into these traditions are less well established. Examples of this class might include a great array of interdisciplinary, incipient disciplinary, policy oriented, action oriented, professional, quasi-professional, applied and ultimately utilitarian in contrast to basic and less immediately utilitarian studies.

Those closest to my own concern and experience are social, behavioral, political and economic studies related to a great range of organizations with implications for policy and administration under a diversity of conditions. Ultimate applications might be in business administration, public administration, health administration, educational administration, international administration and so on.

That is in contrast to fields of inquiry generally perceived to be better established in their traditions of theory building, research design, additive contributions to discipline and the rest. Examples of this class might include at least the heartland areas of so-called laboratory sciences and those with appropriately developed laboratory and experimental traditions. Physics, biology and chemistry with room for inclusions and exceptions for such as astronomy, geology and psychology would qualify.

Beyond the heartland, however, which is to say at the front edges even of the so-called hard sciences, top investigators reckon with some of the same problems of elusiveness and softness so often thought to be a more exclusive attribute of the category first cited. As others have observed, endurance does not add truth to the canard that the intellectual demands of one set of sciences to be called hard are much greater than for another set to be called soft. One might argue that developing traditions of rigor where they have not previously existed might be more rather than less demanding.

Nevertheless, something approaching effectiveness in understanding and coping with first steps is essential if inquiry is to proceed, to reach some point of completion, hopefully to result in some learning about matter and method and conceivably to yield findings of some significance. Investment in research that does not result in world-shaking findings can be more readily defended than effort expended that does not even result in improved capacity and confidence for the next venture. Even for negative or negligible findings, the result may be important and one may be able to recognize and report learning with regard to the processes used. Also, an important point to recognize is that, if early steps can be settled sufficiently to get started, help will be found more readily available in later stages.

Title and Sub-Title, Definitions and Decisions

First steps in the conduct of inquiry include but are not limited to decisions concerning terms of reference, scope, purpose(s), building theoretical foundations and connections, finding and defining researchable problems(s), generating questions to be answered, formulating hypotheses as appropriate and so on.

These, of course, cannot be resolved even in a preliminary way without some concern for research design, selecting a population or sample, assuring the validity of the instruments to be used, data collection, analysis, interpretation and all the rest. Subsequent steps must be anticipated sufficiently to suggest that problems as initially defined may be researchable or, at least, that initial definitions and subsequent modifications may lead to researchable alternatives and eventual outcomes.

The conduct of inquiry entails these and many other decisions in no way given or automatic and subject to variation from case to case. Instead, the many decisions involved are likely to be tentative, interdependent and risky right up to the final stages in some projects. One might add, the more pioneering, the more risky. If there were certainty from the start, inquiry would not be justified.

That decisions are involved all the way is often glossed over, yet critically important explicitly to recognize and record. Some of these might fit general expectations with regard to scientific procedure and some might reflect (with no apology but so stated) quite personal choice. Investigators necessarily have differing interests and capacities and operate within personal as well as topical limitations.

The point here is that the multiple decisions throughout might be partly objective and partly subjective, in part taken consciously and in part taken without full awareness. They might be partly recalled and partly forgotten. The latter might be especially the case in the earliest stages if someone else's non-articulated assumptions are unwittingly incorporated into the choices made. With experience, of course, one learns to keep systematic note of these things and therefore to reduce the possibility of later feeling traumatized by any sense of confusion.

The constant need for decisions, the criteria governing decisions, the processes involved and the likelihood that all three are subject to learning and correction as projects move along may not always be self-evident. Though it may seem self-evident once said, it is not always pointed out that the ground and the ground rules may change as learning progresses.

Further, it may not be self-evident that the decisions, the choice of governing criteria and the processes involved are not always as sequential as simple descriptions of scientific method or later published research reports might suggest. Counsel to state the purpose or problem at the start and to follow four or five steps does not prepare one for an experience that may seem more nebulous, ill-structured, mercurial and non-linear than expected. These considerations alone should make it clear why staking out the initial point of departure is so important whatever the impatience, the frustration and the difficulty may seem to be at the time. Salvage later is considerably more devastating.

Experienced investigators may concede that if any original discovery and learning take place, much of the effort may seem more crystal clear after rather than before the fact. It is always easier in retrospect and for the edification of subsequent readers to trace a discernible scientific logic than it is in transit to work one's way out of a maze. Recognition that effective decisions on first steps must anticipate later ones at least in part and that later decisions must fit earlier ones may only add to the sense of unease in getting launched. That is especially for newcomers who may not realize that making and reporting mid-stream adjustments, even corrections, may be par for the course. In the established language of mathematics, now embraced in many other fields, the thinking processes involved in every stage of research are likely to be more heuristic than algorithmic.

Inquiry is incorporated in the title as a term more embracing than research because the latter might connote a limitation only to specific academic or scientific pursuits and methods. No such limitation is intended. The dilemnas addressed in this presentation also arise for many policy and position papers, for term papers, for staff studies, consulting projects and a great variety of problem finding and problem solving activities especially in the less traditional and pluralistic areas of inquiry that make up more and more of the world's agenda today.

The generic problem of starting any kind of inquiry is interpreted as broader than the term research might commonly imply. Also, the term inquiry might, as intended, suggest the curiosity and conjecturing of the early stages more than the procedural rigor of later ones. That is granting that the stages are not watertight which is part of the whole and essential understanding.

The sub-title is intended to communicate the "how to get started" emphasis for inquiry and research broadly defined. Assistance in moving into a starting position is provided for through the series of questions which partly incorporate and partly lead to deriving criteria for precisely the kinds of decisions that commonly must be taken to get started. In either case, the questions provide guidelines or sufficient direction to formulate guidelines for such decisions. The discriminating reader will want to scan them all and use those that most help.

WHY FIRST STEPS CONFOUND

A relatively unexplored question is why first steps seem to confound so many. This seems highly important to explore before jumping to ideas on what remedial action might be taken in general and for individual investigators. Without meaning to provide crutch or alibi, establishing understanding first on this point might liberate relative newcomers who might too early think that there must be answers that they are missing and that all the shortcomings must be their own. That line of defensive thought plus any defensiveness on the part of mentors with high needs for structure and certainty themselves can inhibit precisely the kind of openness and candor most desired for learning the art and craft of research and completing successful projects.

Defining the Problem Area Is a Neglected Problem Area

The difficulties (problems), of course, for those engaged in first steps as well as for those who would undertake the presumption to write about them are several. That is, just looking at the state of the art in general without getting into highly individualized infirmities or the particular demands of particular projects.

One of the difficulties is that not much has been said that gets at the nebulous, elusive, intuitive, creative, gestating, formative, uncertain, ill-structured, pre-scientific early stages in part because not much is known in any precise or validated way. Whatever may be known, experienced or suspected would be in widely scattered sources under other headings and not generally or fully treated in works concerned with inquiry as such. Especially since people who make it their main business to write about research are likely to be concerned with the acceptability and verification of what they write, one would expect the coverage to be more generous on matters that are better established.

A second difficulty, one that too often goes unnoticed, is that so-called problems are mental constructs that commonly have no empirical referents. Even to add that problems are as they are perceived only increases rather than reduces the difficulty. For Gertrude Stein, famed poetess, it may be that a rose is a rose is a rose. For people concerned with inquiry on many levels, however, the term embraces a variety of meanings and connotations that require sorting out. As one example, the use of the term in common parlance and by an investigator working at the front edge of a given discipline need not be the same. As another, it is common for a newcomer to confuse an intellectual itch about an ill-defined problem area with a well-defined research problem. In both examples, what one takes to be known, the character of the problem, may get in the way of what needs to be known for the purpose of research.

Common to both examples above is that problems are as they are perceived and as one elects to define them for general purposes or for the purpose at hand. Clearly, if the factory lights go out and production cannot proceed, the factory foreman may describe either the light out or the work stoppage as a problem. Whoever is to correct the deficiency may look at the immediate situation or two or three steps beyond for a block or a gap of some kind. The search will likely be for a defect that interferes with the continuity of production and one that can be remedied with available knowledge, material and capacities. While the example might constitute a researchable problem, it would not on the face of it likely be considered of any significance or point as a research problem.

To be significant, the findings would have to require some discovery capable of generalization beyond the one experience. Better still, it would be useful for advancing the state of the particular arts involved if the findings could be related to previous theory and other investigation on such matters. The exception would be if the conditions involved were so out of the ordinary that: (a) the process of correction could not proceed without developing and validating new knowledge or method of some kind; and, (b) the new knowledge or method held potential for validation and use beyond the one case situation.

So, there are gradations among problems, research problems, researchable problems and problems that meet criteria important to people with quite differing commitments. These run the gamut from those with basic disciplinary commitments and predominant emphasis on methodology to those with primary interest in practical affairs and practical applications to a middle range professional interest. The latter, in constrast to more purely scientific and more exclusively practical, might be characterized by an interest in occupying the middle ground between knowledge building and knowledge using. In my valuation, both ends and the nexus between all constitute legitimate terrain for demanding research. The point in this context, however, is that these gradations are among the factors that can create uncertainty as to the initial reference points in selecting and defining problems for research.

Excepting the requirement for suitable rigor and candor in whatever is done, there can be no single standard and no single finishing line in judging research. As in other phases of life, the absence of a single standard might make living complicated for some. To say that there is no single standard, however, is important in order to liberate people from unproductive anxieties and to advocate suitable high standards for each effort. This is thoroughly congruent with maintaining that the idea and definition of a problem, especially a research problem and one that is researchable, is tenuous and not readily arrived at without important points of reference, variations in perception and probably a series of successive though not necessarily sequential and linear approximations.

A difficulty consistent with the foregoing but not always anticipated is that, once a situation is described as a problem, there is no guarantee of instant concensus by all observant, right thinking and well informed people. One discovers that what appeared to be a self-evident problem is somehow a subjective and highly individualized and personalized mental construct not self-evident to others. One almost has to prove that there is a problem, acknowledged to be difficult without empirical referents, before one can start working on the problem. Effective definition of the problem normally will be designed to incorporate the best of available proofs including the acceptability and operationality of what is proposed. The best of proofs will include critique and opinion related to the theoretical and empirical works of others where these are available or the general acceptability of the starting point in the absence of such building blocks.

Acceptability as one of the terms of reference might strike a jarring note for anyone who expects all aspects of the pre-research activity to be just as pure as the research activity is sometimes purported to be. Denial of acceptability as a legitimate point of reference, however, would surely cut off some potentially significant departures from established avenues and methods of research. Not infrequently, it is the interstices between the established pathways that harbor the really creative yield.

Mutable Standards in a Complicated World

Another difficulty that can prove troublesome, not only in the early stages but throughout, is concern with just how original, just how significant, just how conclusive, just how generalizeable and just how much of an addition to knowledge everything has to be.

On these, let my own bias be clear, not because it is correct or better than others, but because it may be constructive and useful and because any researcher may be well advised to consider these with other perspectives before proceeding. Those who intend to incorporate inquiry as part of their ongoing commitment will regularly confront these questions and will need some resolutions with which they can live. My view is that the aforementioned standards are worth reaching for --- with some allowance to stop short of perfection --- especially provided that one knows what has been attained and what has not and how to keep others properly posted.

In the world of practical affairs, originality, significance (in the sense of adding to a body of knowledge and method for building knowledge), conclusiveness and generalizeability might not weigh so heavily as acceptability, utility and whether findings can be implemented.

In the academic world, I do not subscribe to the idea that a dissertation is any document that three people will sign, done only as a practice exercise to meet formalistic requirements. The typical input should not be required and the potential larger output should not be denied on the basis of considerations so lightly taken. Originality and significance, however, are matters of degree. This does not have to mean that the possibility of such a finding was never contemplated by humankind and that the implications call for a reappraisal of human direction on this planet. Lesser degrees of originality and significance consistent with research done up to that point on that question might be quite acceptable. Conclusiveness is also relative in the kinds of research with which we are most concerned. Otherwise, enthusiasm for the whole development of inference and probability applied to research is hard to explain.

In most research, conclusions are findings up to that point for the population examined, given the way the question was put and the methods used. Tightly constructed experiments with small populations are likely to be more conclusive in one sense than studies of larger populations (even with careful attention to sampling) but where the variables are less well identified and less controllable. Difficulties in generalizing, however, might arise in both instances for different reasons.

Additionally, it has been observed that as fields such as those with which we are most concerned approach any maturity, they abandon search and claims related to universality, instead seeking generalization in the form of explaining and predicting patterns and variations. This may be just one more instance, even in the area of research, where events are ahead of the more widely publicized and available materials intended to explain and predict them.

The foregoing leads to two useful observations. One is to lessen concern with, even at times to argue against, premature generalization or premature search for universality, when alternate strategies might be more rewarding and closer to reality as well as we can perceive it. The other is to suggest that additions to knowledge do not have to be universal and do not have to come in large packages. Instead, the prime requirement is that all the connections be established so that the addition posited at the outset and the sum at the end may be evident for others to judge.

Another difficulty for new initiates is that the overwhelming volume of material which treats copiously many aspects of research may obscure the paucity of treatment of first steps. Drowning in information overload, it may be difficult to be selective and to recognize that a critical ingredient is lacking.

There is no lack of volume on philosophy of science, various philosophies of various sciences, logic, reasoning, rules of evidence, search for truth and problem solving. There is also a mounting volume on research design and method generally and for specific disciplines, including data sources, data collection, instruments and their validation and so on. Also included are ever more sophisticated tracts on hypothesis testing, mathematical and statistical techniques and computer programming. Further, in well established disciplines and areas of inquiry, one may absorb accepted conventions on the risks inherent in different ways of proceeding and how to hedge those risks without even being consciously aware of whether and when specific learning took place.

Also, there are many works on writing and writing style including the form and mechanics of preparing research reports, dissertations and articles for scientific, refereed and popular journals. Unfortunately, on the latter as on other items, technique, which is important for the credibility of the whole, may triumph over other considerations --- which it should not.

The important point above, therefore, is that the geometric increase in possible ways of solving problems, getting answers and testing hypotheses, itself may help to obscure the lesser preoccupation with finding problems, framing questions and generating hypotheses in the first place.

Choosing Appropriate Research Strategies

Another baffling item at the outset is that one eventually encounters the categorization of diverse research strategies. This refers to descriptive, analytical, experimental, exploratory, evaluative, survey, comparative, field, laboratory, normative and so on.

The positive aspect is that it might be critically important, productive and saving in time and emotional energy to appreciate a range of possibilities. That is to learn which may be appropriate for given projects as well as the strengths, the limitations and the subsequent problems that accompany each.

For example, experimental and laboratory research may be internally valid for that experiment but not externally valid when transferred to a non-laboratory situation. Field and survey work may have more external credibility in the population from which the data were derived but provide for less control of the variables, less possibility of inferring cause and effect and less certainty in knowing why findings fell as they did. Exploratory studies suggest insufficient knowledge to formulate testable hypotheses at the start and therefore the promise of providing them at the close. Too often chosen without a sense of the rigor entailed, exploratory work ideally should be pursued only by those who can build in the rigor necessary for subsequent effort. For evaluative research of any character, the choice of criteria is likely to loom as a central issue. For analytical work, the continuing concern will likely be whether the elements and relationships abstracted from a larger universe are fruitful in generating statements that have explanatory and predictive power in the universe intended. On comparative studies, while it is true that any study that is analytical is also comparative, the term is more often used for cross-institutional, cross-cultural and cross-national work. For these, one may anticipate dilemmas in settling the unit of analysis and the method of comparison as a minimum plus concern with what instrumentation can be held constant if the work is really across cultures. For normative research, one must anticipate that it may not be modish in all circles, yet recognize that, if explanations then can be verified and predictions established, it should be possible to posit norms and subject them to legitimate research.

So much for the briefest sampling of a limited number of research strategies in order to suggest the positive value of being well aware of choices and what might later be part of executing and defending any decision taken.

The negative aspect is that one may be led to choose an inappropriate research strategy or discouraged in the pursuit of worthwhile research that one is motivated to do because of a kind of upsmanship or prestige social pecking order game that sometimes operates. The game is that which associates some research strategies with a higher order of intellectual life and others with a lesser order. The realities, of course, are that such judgments will operate in all phases of life and that some disciplines, graduate schools, journals and professional societies will accord greater esteem to the strategies that they want to be representative of their achievement. That the effect of the foregoing can be enormously positive is not disputed.

dimensions in good balance. For a doctoral candidate, it is assumed that the initial choice will stretch existing capacity, that additional strengths can be developed as the project proceeds and that the composition of the steering committee plus consultations available to the committee will cover contingencies. In fact, the experienced chairman will see that the candidate stretches the starting endowment but does not take on too much. These are the ideal conditions for the academic side where rubrics have long been available even though they break down. While there are enormous variations for the doctoral candidate, the variations are probably greater in other settings where the investigator is assumed not to be a neophyte and where the majority of those who might be consumers are assumed not to be researchers.

The main point here, however, is that many of the questions about scope and plan for research, which are essential to get research started, also serve the purpose of getting some reasonable match of topic, candidate and committee in one case and a reasonable set of expectations for others.

Finally, for our present purpose, there are the many difficulties involved in fear of exposure. Everything said so far adds to the possibility that people not experienced in the world (or the many worlds) of research may undergo what has been called "culture shock" in the area of international travel. That is, being at a loss effectively to read and orient behavior to various and differing social cues. Entering into the world of research, if it is a new and strange one, can also be disorienting if all the familiar terms of reference (taken for granted or not even recognized) have suddenly been removed or reordered. The disorientation leads to feelings of inadequacy which may also lead to fear of exposing real or imagined shortcomings.

The reality is that neither the researcher nor the sponsors are treading in territories where each has the security of fully knowing the other's capacities and fully knowing what comes next. So, the building of trust and confidence are important. As the project proceeds, the investigator will know more than anyone else and will have to keep updating and reorienting the sponsors. They will have to be able both to check and to trust the investigator. Further, from the start and as the project moves along, the investigator cannot help but recognize so much that he or she does not know about the topic, about research technique, about standards of publication, about what to include and what to leave out and still keep up the fact and the appearance of full disclosure.

From the outset, doubt and skepticism and curiosity are essential ingredients of inquiry and research. There is, however, the balancing of too much and too little self-doubt and how the communication of either might effect the relationship between researcher and sponsors. The know-it-all courts come-uppance. The non-communicator may anticipate a widening gap in expectations. The overly-solicitous to communicate may use up capital. And, the person too anxious about inadequacies or too fearful about candid exposure may seriously reduce interest and confidence in his or her capacity. Ultimately, motivation and confidence on the part of researchers and sponsors entail reciprocity. Each has to take positive measures or at least not put out negative signals with regard to the other.

Summing Up a Dozen Difficulties

In sum and without being exhaustive, we have identified about a dozen difficulties that might partly or fully explain why people can be confounded in the early stages of research. Not much is known in a validated way about the mental processes involved in the first stages. The fact is that problems are frequently perceptions and mental constructs without empirical referents. There are gradations of problems, research problems, researchable problems and problems that interest people with differing kinds of research commitments. For research in general, there is no single standard and no single finishing line. Likewise, there is no guarantee of instant concensus by observant, right thinking and well informed people even after something has been called a problem. Equally, some of the most paralyzing concerns such as originality, significance, conclusiveness, generalizeability and addition or contribution to a body of knowledge are of major importance but reasonable and relative in the context of any particular project. Preoccupation with generalization at a universal level has lately shown signs of yielding to generalizations in the form of explaining and predicting patterns and variations. The canon that research should be additive simply means that all the connections should be established so that the addition posited at the start and the sum at the end may be evident for others to judge. The overwhelming volume of material on philosophy of science, research design and hypothesis testing, mathematical and statistical technique, form and style in reporting research, solving problems and getting answers probably obscures the relative paucity of material on finding problems, framing questions and generating hypotheses in the first place. Categorization of diverse research strategies as to their appropriateness for various questions, their strengths and limitations and the subsequent problems that accompany each is useful. Extending that categorization to incorporate a prestige pecking order associated with each choice may be less functional especially if it leads people to pick inappropriate topic or method. It is eminently desirable and reasonable to assure balance between the demands inherent in a given research proposal and the capacities of the person who is to do the research and those who are to sponsor it. And, finally for this dozen, entering into the world of research for some may result in a kind of disorientation, loss of confidence and fear of exposing real or imagined shortcomings. These need to be anticipated, recognized and overcome in order to assure joint and reciprocal motivation and confidence between researchers and sponsors.

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HOW TO GET RESEARCH STARTED*

Paul J. Gordon

Indiana University

Abstract

The purposes of this treatise are two. One is to provide assurance that difficulties in getting research started are ordinary. The other is to set forth a way of working out of the uncertainties, ambiguities and elusiveness that can exist at the start. Consistent with the first purpose, Part I poses two questions. Why are first steps in the conduct of inquiry so taxing? And why have people been so little forewarned? Consistent with the second purpose, Part II sets forth two propositions. One is that the processes involved in working out of a poorly structured context at the start are similar in a great variety of uncertain, ambiguous, judgmental and creative situations in life. The other is that the questions presented and discussed, cycled and recycled, will help in working out of the apparent maze involved in getting research started. The intended readership is noted at the outset. The special demands in writing were to discuss the nebulous without being nebulous and the non-linear one page at a time.
