

THE CHOICE OF THE PROBLEM¹

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THE matter of how to judge the goodness or badness of a result, particularly when this result is a theoretical formulation, has received considerable attention in recent years. Today in psychology, such decisions are increasingly necessary. Our subject matter has become quite boundless: muscle twitches and wars, the sound of porpoises and problems of space, the aesthetic qualities of tones and sick minds, psychophysics and labor turnover. The range of organisms involved in the studies of these problems extends from pigeons to people, from amoeba to social groups. Further the techniques of measurement have been honed and sharpened by electronic tubes, computing machines, mathematical niceties, and imaginative testing procedures. Too often, in this lush environment, we as researchers may find after some months of toil and research that our findings, although in accord with nature and beautifully simple, are utterly petty and we ourselves are no longer interested in them much less anyone else being interested.

This problem of carefully selecting and evaluating a problem does not involve the researcher alone. In our complex beehive of today, this is a question for the teacher of research, the thesis and dissertation director, the research director of laboratories or programs, and the dispensers of research funds—in a small way, department chairmen and deans, and in a large way, the guardians of the coffers of foundations and government agencies.

We are not without criteria, of course. Either implicitly or explicitly we seek justification for what we do. Certainly when grants are involved we seek for some reasons to justify the getting or the giving of money. For our consideration, I have rummaged around and turned up six widely used bases for doing an experiment: curiosity, confirmability, compassion, cost, cupidity, and conformability—or, more simply, “Am I interested,”

“Can I get the answer,” “Will it help,” “How much will it cost,” “What’s the payola,” “Is everyone else doing it?”

I believe you will find that these are the things that enter our minds when we evaluate a student’s problem, dispense a sum of research money, or decide to put ourselves to work. To anticipate myself, however, I will try to establish the fact that these bases, used alone or in combination although perhaps correlated positively with a “successful” piece of research, will probably have a zero or even negative correlation with a “valuable” piece of research.

Before proceeding to examine these criteria, however, let me introduce a clarifying footnote. Although I am concerned about selecting a problem beyond the routine and “successful” experiment—and here I shall use completely indiscriminately (with apologies to the philosophers) such terms as “good,” “valued,” “enduring,” “worthwhile”—I do not wish to disparage the necessary place of routine experiments, i.e., well conducted experiments which fill in and extend those more creative ones. I think that even the most cursory consideration of the history of science reveals that the “original” or the “important” experiment almost inevitably pushes out from routine work that has preceded it and is further dependent upon the supportive routine experiments for their fruition into the field. Most definitely, I would contend that it is far more important to do a routine experiment than no experiment at all.

But back to the problem before us: Are our reasons for experimenting sufficient guidelines to decide about experiments? The first of these, *curiosity*, is the grand old man of reasons for experimentation and hence, justification for our experimentation. In the days when knighthood was in flower, this was the most familiar emblem on the scientist’s shield. It was enough to seek an answer to “I wonder what would happen if . . .” This was sometimes formalized with

¹ An abridged version of the Presidential Address to the Southern Society for Philosophy and Psychology given at the fifty-second Annual Meeting in Biloxi, Mississippi, April 15, 1960.

the dignified phrase, "knowledge for knowledge's sake."

Today this is not a strong base of operation. Perhaps costs have outmoded whimsey; perhaps the glare of the public stage has made us too self-conscious for such a charming urge. Or, perhaps more forebodingly, we are less curious—or, perhaps a combination of these has made curiosity less defensible. More critically, when we look more closely at this justification for doing, or proposing to do an experiment, it does not turn out to be one. Clearly a person can be curious about valuable things, trivial things, absurd things, or evil things. I think we would all find it a little difficult to judge the relative merits of two pieces of completed research by trying to decide which of the two experimenters was the most curious. Perhaps wisely, then, it seems more and more difficult to convince deans or directors of research, or dispensers of funds that a problem is worth investigating because we, personally, happened to be puzzled by it.

The criterion of *confirmability* as a criterion of worthiness of the pursuit of a topic has two sides: a philosophic one and a pragmatic one. Philosophically, this criterion reached its glory in the '30s and '40s when the voice of a logical positivist was heard throughout the land. In no uncertain terms, they told us that the criterion for a problem was "that the question asked could be answered." On the pragmatic side, this criterion is interpreted to mean: "Pick variables which are likely to be statistically significant." Undoubtedly, the philosophical point of view has done much to clear up our experimental work by hacking through a jungle of undefined and ambiguous terms. From the pragmatic point of view, this has been a much valued criterion for the graduate student with several kids and who must finish his thesis or dissertation to get out and start earning a living. However, it is just this criterion that may be voted the most likely to result in a pedestrian problem. It demands problems which have easily measurable variables and clearly stated influences. It discourages the exploration of new, complex, or mysterious areas. To exercise this criterion alone would force one to choose an experiment of measuring age as related to strength of grip on the handle of the dynamometer against exploring variables associated with happiness. Both may be quite worthwhile, but the latter has less of a

chance of being approached so long as we exercise the criterion of confirmability alone.

The problem of *costs* must enter into considerations of undertaking an experiment. In the real, live world, determining the value of a thing is very simple. Find out how much it costs. Clearly, anything that costs a lot is very valuable. A car or house which costs more than another car or another house is naturally worth more. One pays for what one gets and one gets what one pays for. This thinking carries over into the world of scientific affairs. Space probes are obviously important because they cost a lot; a project which can get a large grant must be a good one or else it would not cost so much.

Certainly this is very faulty reasoning. That methodologies differ in their costs is quite obvious; the more expensive the methodology, the more valuable and important the activity is not a direct derivative. Einstein undoubtedly used less equipment than his dentist but we may suspect that Einstein attacked the more valuable problem. Departments of Philosophy are far less expensive than Departments of Veterinary Medicine but I do not believe them to be necessarily less valuable.

It is quite true that when a large sum of money is expended on a particular project that some decisions have been made that the desirability or the value of that project justifies the expenditure of this large sum. Money may serve as a crude index of where to look for decision bases that justify large expenditures but cannot serve as judgment bases themselves. If a person says, "To do this piece of research will cost x amount of money and will occupy x amount of my life," he has merely brought his problem into focus and has raised the critical question more clearly, i.e., is such an expenditure worth it? He has not solved the problem of how valuable his experiment is, but raised the question—some other criterion must be sought for an answer to that question.

A somewhat new criterion has entered into thinking today: *compassion*. As we have moved into the applied world, this rather new criterion has come into increasing use—at least this seems true of psychology. A person asks himself as he begins an experiment, "Will the results make things better?" and implicitly assumes that an affirmative answer will make his experiment or his project more valuable. The problem then is assessed in terms of its solutions or answers resulting in a

patient's improvement, a reduction in prejudice, a happier or a healthier world, etc. A variation on this question, as it is asked in the market place and in some of the other sciences directly, is in a slightly cruder form: "Will this be useful; what service will this finding perform?"

H. G. Wells has a comment on this guideline for performance in his book *Meanwhile*:

The disease of cancer will be banished from life by calm, unhurrying, persistent men and women working with every shiver of feeling controlled and suppressed in hospitals and in laboratories. . . . Pity never made a good doctor, love never made a good poet, desire for service never made a discovery.

In one sense of the word, this criterion is a form of the old, applied vs. basic issue that plagues all sciences that live with one foot in technology and the other foot in theory. I cannot begin to resolve this issue here. I can, however, I believe, say this: it is quite possible for a piece of research undertaken in compassion or for utility to be quite valuable, enduring, well thought of, etc. It is also quite possible that it be trivial, superficial, limited, useless, etc. The same may be said for any given piece of basic research in which utility or compassion was never an issue. I am not one who believes that because a piece of research has no relevant use, it then by definition is valuable; or that a finding because it is useful is worthless. If these statements may be granted, it would appear then that some more fundamental criteria must be applied.

Cupidity is a variation on the criterion of compassion. Here, however, the "pay-off" is not for others but for oneself. Very simply the research is evaluated in terms of whether it will get one a promotion, favorable publicity, peer applause. Well, sometimes, undoubtedly what is good for you is good for others, and hence of general value.

However, the contrary is just as likely to be true: namely, what is good for you is not necessarily good for others at all. For example, making the natural assumptions that deans cannot read and department chairmen do not, the greater the number of papers, then the greater the probability of promotion. This results in a whirling mass of fragmented, little, anything printables to be constantly turned out instead of mature, integrated, programmatic articles. In being good to yourself you have done little or no good for others. The most casual recall would suggest that the impelling

motives behind most significant advances in thought have not been cupidity, rather to the contrary such advances seemed to be more "selfless" than "selfish."

The last of my "useful" criteria is that of *conformity*. In these days of togetherness, it is not at all surprising to find conformity and its cousin, comfortableness, serving as guidelines for determining the should or should not of experimentation. We mean, by conformity, the choice of the currently popular problem, i.e., one within an ongoing and popular system, for example, operant conditioning, statistical learning theory; or a currently popular area of investigation, for example, sensory deprivation, or the Taylor Manifest Anxiety Scale.

As with all of our preceding criteria for deciding about a research project, conformity clearly has its merits. It would be foolish to turn one's back on new methods or a recent breakthrough of ideas which have been developed and certainly the interactive stimulation of mutual efforts in the same area are helpful factors in research. These, however, seem to be more means than ends to be sought for. One must be cautioned against becoming overenamored by the availability of a method at the expense of thoughtfulness or being charmed by the social benefits of working in an active area at the expense of the scientific implications of such work. More simply, some things that a lot of people are doing are quite worthwhile, and some are quite ridiculous.

Unfortunately, however, the assiduous cultivation of all these virtuous goals may still, it would seem, even in combination result in the most pedestrian of problems. We must then search further for means of assuring ourselves that the problem is a good one. The usual criteria do not seem to answer.

I am going to say that there are three fundamentals which form the basis for good experiments or good problems. None of these are new. By disclaiming originality, however, I can claim that they are profound and that their presence or absence makes a significant effect in the value of the problem to an individual. Two of these are characteristics of the person, and one of the problem. The most common tags for my trio are: knowledge, dissatisfaction, and generalizability. The first two of these, of course, refer to the

individual himself and the third to the problem itself.

I think that there is very general agreement that one can only work effectively in an area when he has a thorough understanding of this general area of concern. It is quite often that the significant finding comes from a fusion of quite a number of simple studies or a perception of gaps in the detailed findings or the methodologies and procedures of others. I am quite sure that the vaunted, creative insight of the scientist occurs more frequently within a thorough knowledge of one's area than as a bolt from the blue.

In a quite mechanical sense, a failure to obtain all knowledge possible about one's problem area is to fail to profit from the errors of thought of the past, if they be errors, or the knowledges obtained, if such knowledges are correct. Either represents an arrogance of a witless kind. Moreover, in this very practical sense, unless these backgrounds are well assessed by the worker, one may find oneself both discovering a most well-known discovery and be in the embarrassing position of arriving at the party dressed in full regalia a day late. Or perhaps, more tragic for the world of ideas, such a person without knowledge may remain unheard, being incapable of gaining the attention of competent workers through an ineptitude of expression or lack of relation to the field.

Secondly, however, to avoid the sins of conformity, suggested previously—for specialized knowledge groups can become more ingroupish than a band of teenagers—a healthy opposition must be present. I have designated this as dissatisfaction. Other terms to be used are skepticism, negativism, or perhaps more charmingly, iconoclasm.

It is, of course, quite possible that I am very wrong in emphasizing the necessity of an opposing set to the existent knowledges and methodologies in one's time. Clearly the most convenient position would be to invoke the concept of "genius" or "insight," leading to important problems as a result of broad surveys of the literature, or efficiency in employing procedures. This, however, would hardly be useful as a guideline. One can hardly suggest to a person that they strain and have an insight, or try hard and be a genius.

There is, on the other hand, considerable empirical evidence, or at least examples, to substantiate the fact that original discoveries contain an element of active revolution. Skipping lightly

through psychological history, we can point to Hemholtz' classical rate of nerve conduction experiments which flew in the face of established knowledge about immediate conductivity, Freud defied the reign of conscious thought, Watson negated mentalism, Köhler set against the tide of trial and error learning, in recent times Harlow has spoken not so much for positive adient motivation as against avoidant motives as the prime mover of man.

Logically, and psychologically (and happily they conjoin occasionally) this seems to make good sense. A significant research problem is a creative act. One can hardly be creative if one is avidly listening to the voice of others. We have good evidence from such experimentations as the Luchin's jar experiments that developed sets can clearly block solutions to problems. More simply, if one agrees with everything that everyone else says, one's role is automatically limited to feeding the fires, applauding the words or, at best, carrying the word. None of these are actions that lead to truly important research activities.

We may have, however, great knowledge and object quite violently to the items of this knowledge and proceed to conduct small experiments to substantiate our objections and still be doing little more than picking at a pimple on the face of one's science, to use a vulgar analogy. A further critical requirement must be recognized—a critical requirement that is most difficult to capture in words: to be an important result, one's findings require "extensivity." Another word used here is that one's results must be generalizable. Poincaré, in his *Methods of Science*, states most clearly the reasoning underlying this requirement:

What, then, is a good experiment? It is that which informs us of something besides an isolated fact. It is that which enables us to foresee, i.e., that which enables us to generalize. . . . Circumstances under which one has worked will never reproduce themselves all at once. The observed action then will never recur. The only thing that can be affirmed is that under analogous circumstances, analogous action is produced.

A further quotation from the same book amplifies this point of view:

. . . it is needful that each of our thoughts be as useful as possible and this is why a law will be the more precious the more general it is. This shows us how we should choose. The most interesting facts are those which may serve many times.

Very simply, this boils down to being able to evaluate the probable consequence of your findings with the question which goes something like this: "In how many and what kind of specific circumstances will the relationships or rules that hold in this experiment hold in such other instances?" If the answer to this is only in instances almost exactly replicable of this particular circumstance, the rules that we obtain are likely to be of little consequence. If, however, the rule applies to what apparently is a vast heterogeneity of events in time and space, in varieties of species and surrounds, this rule is likely to have great value. Stated otherwise, the extent to which our variables and situations are unique and rare in contrast to universal and common largely determines the extent to which the findings are likely to be considered trivial or tremendous in their implications.

My summary can be quite simple:

Research today is both complex and costly. Guidelines are needed to sort among these complexities to enhance our chances of a sound investment, be this personal, financial, or temporal.

Six criteria may be, and often are, applied to judge a project's "success" potential: curiosity, confirmability, cost, compassion, cupidity, and conformability. There is probably a good probability that studies meeting the guidelines will "pay off" in some form of coinage—perhaps small change.

However, for a study to be an enduring and critical one for the history of ideas or to enter into that stream, three further items seem involved:

1. You must know thoroughly the body of research and the techniques of experimentation which are related to a given problem area. Naivete may be a source of joy in an artistic field but is not the case in valued research efforts.

2. You should be able to disbelieve, be dissatisfied with, or deny the knowledge that you have. (This is no paradox in relation to our first statement. Recognize that the first requirement is propaedeutic to this one. This is an active, not a passive state; this is to know and then know differently, rather than a know-nothing state.) Valued research seems to grow from dissatisfactions with the way things are, rather than agreeable perpetuation of present ways of proceeding.

3. You should, very simply, look for the forest beyond the tree, test the generality of your proposed finding. If your finding is referent to a rat in a particular maze, a patient on a particular couch, or a refined statistical difference, then that rat, that patient, or a captive statistician may listen to you. This would be a skimpy and disappointing audience to my way of thinking.

It is quite likely that we cannot all become geniuses. We can at least try to be less trivial. Learn as much as we can, believe in new ways, seek as great extensity in our variables as we can.