PROBLEM SOLVING: SOME THOUGHTS ON METHODS AND PEOPLE*

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It is reasonable to assume that our supply of problems is not going to run out in the foreseeable future and perhaps the market for problem-solvers will also remain firm. The rapid and efficient solving of problems is, of course, the primary aim of the research worker in industry. Whether these problems are deliberately posed, such as in the quest for new products, or dumped in the Technical Director's lap by Sales or Production is of little importance. The point is they must be solved quickly and efficiently.

To some extent, we take the process of problem solving pretty much for granted. We become familiar with it during our college training and we subsequently practice our own particular aspect of it so that it becomes almost habitual. Recently, however, some of our staff have thought it worthwhile to review various approaches to the solving of industrial problems. Not surprisingly, a great deal of lively discussion resulted, and I have drawn freely from the ideas expressed at these informal gatherings.

It is always difficult and dangerous to generalize about science, and particularly scientists, but for purposes of discussion it is possible to divide approaches to problem solving into three main classes: the theoretical approach, the experimental approach (although this is not the only approach to use experiments) and a more recent approach called Operational Creativity. We shall refer to the idealized people who populate these classes as model-builders, experimentalists and "blue-skyers" (for reasons which will be apparent later).

These classifications should not be confused with other arbitrary divisions often drawn between something called "pure" science and something else called "applied" science. The divisions I have just made are concerned only with method and not with motives.

To compare the value of the approach of the model-builder, the experimentalist and the "blue skyer" would be quite pointless. It would be like comparing the relative utility of a hammer, a screwdriver and a drill. To

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argue that the screwdriver is better than the hammer would clearly be ridiculous; each is uniquely suited to perform certain tasks. Such is also the case with the tools available for problem-solving.

First, let us consider the theoretical approach. Here, facts are fashioned into a theory, which must be proved by a carefully designed experiment. The experiment in turn produces more facts which may make it necessary to modify the theory and thus necessitate further experimental proof, and so on. Workers in this field often use mathematical language to represent their models. The recent tenth anniversary of the tragic Texas City disaster is a reminder that the cause of the ammonium nitrate explosion was determined by the model-building technique. The theoretical approach is essential here—after all, one can hardly go around exploding enormous quantities of ammonium nitrate in order to get additional data. A more recent and highly successful application of this method by scientists skilled in Operations Research is the calculation of physical constants and other engineering data for complicated piping networks. Complex "plumbing" problems may now be solved on a computer, instead of by an army of engineers carrying out dreary mathematical calculations. In this case, a problem which has already been solved has been resolved by a far more elegant method. This is a familiar pattern—many current problems being handled by the experimentalists may ultimately be resolved more rapidly and accurately by the model-builder, using the evidence accumulated by the experimentalists.

The model-builder is sometimes compelled to resort to the empirical approach. A good example of this is in the identification of materials by IR absorption spectra. Although the theory is highly developed and the instruments very refined, the model-builder cannot positively identify a material from theoretical considerations. He must be able to compare his spectra with that of a known and closely related, or better still, identical, compound.

Scientists engaged in this sort of work are usually highly trained, articulate people who like to talk about what they do. It is probably fair to say, however, that they are much more interested in discussing their methods than their results. The extreme case of the model-builder is the scientist who is so preoccupied with his method that he selects his problems in order to provide opportunities to use his technique. At the other end of the scale, there is a gradual, almost imperceptible, change into the predominantly experimental area. For instance, who can judge where the field of the polymer scientist ends and that of the advanced resin chemist begins?

The experimentalists' method for solving problems is perhaps the most familiar to industry. It can accurately be described as an empirical method, although this is a dangerous word because it has become identified in many people's minds with an approach which is dependent *only* on experience and observation, and is not scientific. This is most unfortunate. It is true that the empirical approach *is* based on experiment or experience, *but* in gathering his evidence, the experimentalist must be guided by scientific principles as rigid as those recognized by the model-makers. In a sense, every individual experiment which the experimentalist carries out is, or should be, an exercise in theoretical scientific problem-solving on the micro scale. By the nature of his work, however, the experimentalist is often unable to see far ahead. He must feel his way carefully from experiment to experiment, gingerly probing for profitable areas to explore. This kind of work is costly and time consuming; it is, therefore, vitally important that the experimentalist should not only know *how* to do his work, but, still more important, he should know *what* to do. A worker who has the reputation for solving problems of this type quickly does not arrive at the solution by some magical process or just plain luck. His success is attributable to his intuitive ability to decide which is the most profitable experiment to do first.

The experimentalist should be aware of the danger involved in the misuse of this technique. Just as the model-builder may lose his power of contribution by transferring his affection from the problem to the method, so the experimentalist can become a liability if he neglects scientific principles. Many a research director has become prematurely aged on being presented with a long series of experimental results without a "control," and the failure to recognize an important variable such as humidity, is another favorite pitfall.

The experimental technique is, of course, uniquely suited to many industrial problems. Workers in this field often deal with natural materials, with complicated resins, with blends and mixtures in which the components interreact in surprising and unpredictable ways. Although we have, for instance, a tremendous background of literature on the behavior of such materials as rubber, proteins and cellulose, I believe that workers in these fields would agree that we are not yet ready to tackle many of the problems of modern industry by the extensive use of theory.

What of the experimentalists themselves? Perhaps, on an average they have not received the high degree of specialized training associated with the model-builders, but their experience is broad. They are generally practical minded, and their first impulse on tackling a problem is to get out into the laboratory and get started with some experiments. As a group, they communicate less readily than the model-builders, both with their fellows and with people outside of their sphere. They are inclined to talk in terms of the feel, the smell, the look of a material rather than to show how nicely it fits into a theoretical framework. The experimentalist talks more about his results, and less about his technique. While every experimentalist fondly hopes that theoretical conclusions will ultimately be drawn from his experimental evidence, he will not feel too guilty if he has merely solved the problem, without providing material for a theoretical treatment. Nevertheless, many experimentalists feel frustrated because they are rarely able to follow through on the theoretical side once a problem has been solved from the practical standpoint.

In discussing the results of his work with non-technical people, the experimentalist is sometimes at a disadvantage. His language, his approach and his end result may appear to be deceptively clear, so that the layman feels that he really understands what has been going on. The oversimplification of the process in his own mind leads him to question the expenditure of time and—let's face it—money on the problem.

The model-builder is less likely to find himself challenged in this way. The layman stands in awe of him. His work is much less readily understood (or perhaps we should say misunderstood) by the layman and he is less vulnerable to criticism. On the other hand, the layman is more skeptical of the theoretical approach to "real" problems, and is often reluctant to utilize this technique when the problem justifies it.

The third method of solving problems now available to industry is called Operational Creativity. It is obvious, of course, that the creative process is a necessary part of the techniques which we have previously discussed. Without it, the most rigorous scientific approach or the most carefully constructed experimental system cannot advance into the unknown. However, the exponents of Operational Creativity have deliberately set out to control and exploit the creative process, rather than let it happen in a haphazard fashion. They do this repeatedly, and at will. The technique now being used has evolved over the past ten years or so, primarily by the efforts of a group led by Dr. W. J. J. Gordon of Arthur D. Little, Inc. During the conception stage of an idea, their tools are talk and a tape recorder. They work in groups of four to five people, in sessions lasting two to three hours, trying by means of free association to determine, first of all, the fundamental basis of the problem, and then ways to solve it. Ideas presented by the individual in the group need not always be practical, for they may stimulate other ideas leading to a practical solution. Necessarily, therefore, their problems must be couched in broad terms, and in the conception stage they avoid becoming side-tracked by detail.

Since innovation is their prime purpose, their choice of problem needs careful consideration. They measure their efficiency at innovation by their success in obtaining basic patents, as opposed to design patents.

As might be expected, groups embarking on such a new approach to problem solving will initially be somewhat inhibited in their reaction to free discussion. It is sometimes necessary in the early stage for the leader to withhold the specific problem until a session has got well under way by discussing some basic concept such as "opening things." When the time is ripe, the leader will then throw in the real problem: "We want a new can-opener."

Later, as the group becomes closer-knit, this device can be dispensed with. The members learn to identify themselves very intimately with the problem—become part of it, in a sense.

There are probably less than six groups of people practicing Operational Creativity at the moment. One successful project which can be mentioned was the development of a new product for the Kimberley-Clark Corporation. Because Operational Creativity produced an answer to a problem which had eluded other approaches, Kimberley-Clark is setting up their own group. Another recent success of Dr. Gordon's group was the development for the Horto Corporation of a new technique for anchoring the foundations of light buildings, such as greenhouses and garages, to the ground.

One technique for collecting ideas, which bears a superficial resemblance to Operational Creativity (O. C.) has become known as "Brainstorming." There are, however, a number of fundamental differences between these two approaches:

1. O. C. groups take the concept through reduction to practice; brainstormers "think up" ideas only.
2. Brainstorming sessions may be short, and repeat sessions usually decrease in productivity. O. C. takes considerable time—as many as 30 sessions have been held on one problem. The group becomes closely knit, and tends to increase in productivity with time.
3. O. C. is most useful in attacking and defining basic, broad problems, whereas brainstorming usually concentrates on narrow, well-defined problems.
4. O. C. relies on the group working as a whole toward a unique solution; brainstorming encourages a competitive attitude within the group, and thus every suggestion is an individual solution.
5. Brainstorming emphasizes the quantity of ideas generated; O. C. the quality.

At the moment, the "blue skyers" are too small a group to permit us to generalize about personality. It is obvious, however, that they must be articulate—if not eloquent—since their concepts arise only from discussion. They must learn to work together without inhibitions which would tend to dampen the flow of ideas and concepts. O. C. adherents believe that individual creativity can be enhanced by participation in group sessions. Their educational background is varied, but the group must contain or have access to a person skilled in the technical area under examination.

How do these groups fit together to form our problem-solving society? We should be unrealistic if we expected them to get along with one another all the time. The model-builder says of the experimentalist, "There he goes again—re-inventing the second law!," and the experimentalist complains that the model-builder tries to baffle everyone with science, by maintaining a blackboard full of impressive equations. In turn, each of these groups is skeptical of the blue skyers, because they are newcomers and they talk so much!

Perhaps the model-builder envies the relative ease with which the experimentalist sells his approach to management. The experimentalist has a slight inferiority complex because he is viewed in certain quarters as a "second-class scientist." The "blue skyer" is in a militant minority at this stage, and suffers accordingly.

It is, therefore, vitally important that research management on any scale of operations should realize the importance of maintaining close contact between groups following the various disciplines. These groups must not be allowed to withdraw from one another, to feel that their method is "right." There is so much to be gained from the free exchange of ideas and the willingness to seek advice from others that every opportunity for liaison should be encouraged. Scientists who have the ability to recognize the strength and limitations of the various approaches to problem-solving (and of the problem solvers too!) can do much to benefit the problem-solving world.