AN EXAMINATION OF THE USE OF QUANTITATIVE METHODS IN THE ECONOMIC DIRECTION OF INDUSTRIAL RESEARCH. II.

By

J. KLÁR

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2. *The investigation of necessary and of possible expenditure.* The fundamentals of Socialist research are that it is *planned* and *organised*. Planning requires that the aims of scientific research be determined in harmony with the aims of the plan for the people's economy and that research should itself be of a planned character. The requirement that research should be organised, may be satisfied if the plans are born of the cooperation of all those concerned in them. It follows from the planned and organised nature of Socialist research that the compilation of the lists of problems should everywhere take place in agreement with the interests of the people's economy, moreover that the various problems should be compared with a view to the *necessary* and the *possible* expenditure involved in their research. This should take place either simultaneously or previously to the evaluation of the foreseeable fruits of each problem according to its significance and to a calculation or an estimate of the probability of its success. The comparison will then enable the volume of the research program to be determined realistically.

The *necessary expenditure* consists on the one hand of the intellectual manpower and financial requirements (both of fixed and circulating assets), for completing the research project, and on the other, of the intellectual manpower and investment requirements for the introduction of the research results.

The *possible expenditure* may similarly be divided into two components — the amount of the expenditure available for the research work and that available for the introduction of the results of research. Thus while the necessary expenditure is concerned with the total *demands* made by research work, the possible expenditure *shows the sources available, in brief, the capacities* for the satisfaction of the above demands.

It is good practice to estimate the *necessary* expenditure — if this is possible — as a function of time, *i. e.* to state the necessary requirement of intellectual manpower and of assets as a function of the predicted duration of the project. (A particular piece of research might, for instance, yield results with two research workers in two years, while in order to obtain the same results in one year, two more research workers might be needed.)

The necessity of the expenditure on each problem can, of course, only arise once a particular problem on the list of research themes is actually incorporated in the research program.

Contemporaneously with the qualitative analysis of the *necessary expenditure* upon the achievement of research results and upon their introduction, every effort should be made also to establish quantitative conclusions on the expenditures involved. In respect to this quantitative evaluation, there are expenditures that are *easier and others that are harder to present in numerical form.*

It is *easier* for instance, generally to express *part of the financial expenditure* on research on the various problems on research equipment (apparatus, instruments, auxiliary equipment, *etc.*), in numerical form. The value of the investments necessary to introduce the research results may also generally be more easily expressed in figures. The fact that numerical information is here available, does not, however, mean that the various numerical values are always precise estimates.

A *harder* task is to *estimate the research work* necessary for the various problems. Both foreign sources and domestic experience have furnished information on various experiments at evaluating research work by some numerical system, *e. g.* different values for the labour-time of research workers with different qualifications and classification, etc. These methods are, however, for the greater part based on the formal, or mechanical application of certain empirical facts and relations.

The numerical evaluation by such methods of the research work of research personnel with *lower qualifications* may sometimes be undertaken successfully, though in other cases it might be precisely these methods that distort — through their formal approach — the correct statements of an institute head who has a good knowledge of his research personnel.

In the case of a highly qualified research worker the only type of statements that can generally be realistically made by a head of research is *what sort of work on which* of the problems on the list the research worker is suited for.

In the course *of all* expression of the intellectual work necessary for research, however, great care must be taken *only to make quantitative statements that can be realistically estimated.* The difficulties involved are obvious. In the case of a designing or constructional job of average importance, it is usually quite feasible in advance to state the requirement of intellectual manpower, *i. e.* the number of persons whom it is necessary to engage and the duration of the work to be done. This is a very much more complex task where research work is concerned — firstly because the significance of the differences between the abilities of individual research workers is much greater for the pursuit of research activity, secondly because, partly due to the differing skill and ingenuity of research workers, partly to the peculiarities of research work that distinguish it from other types of work, it is not alwys possible precisely to calculate *how many* research workers and *how much time* are needed to accomplish a particular piece of industrial research.

A formal numerical expression of the requirements may appear to diminish the uncertainty involved in the actual business of drawing up a program, and thus also the number of possible alternative programs. It may, on the other hand, deprive those concerned of the chance of obtaining information that *corresponds to the truth*, before adopting final decisions on the research program.

Simultaneously with a quantitative analysis according to the above principles of the intellectual labour and asset expenditure necessary to achieve research results, it is imperative to examine the various *possible* resources and capacities available for these purposes.

These possible expenditures, which we have called *capacities*, must of course be examined according to the same categories as are applied in the case of the necessary expenditure for each problem.

A part of the asset expenditures available for research and for the introduction of research results, which are *thus possible* asset expenditures, may also generally be well expressed in numbers. The numerical values may in most cases be determined or estimated from the plans for the people's economy, or for the sector of industry concerned.

A realistic determination of the actual *intellectual* manpower capacity available for research work, is a good deal more difficult.

If the requirement of the *necessary* intellectual manpower with lower qualifications may be easily expressed numerically, the *available* manpower capacity can also be well stated in numbers. Thus, in respect to certain problems it is sufficient, as far as laboratory assistants are concerned, to state the numerically easily expressed necessary labour time, in which case the total capacity available can also be well presented numerically, in hours of work.

The situation is, however, quite different with the *higher qualified* and *highly qualified research workers*. Here again, quantitative evaluation can mostly lead at best to the statement of certain relations, *e. g.* that a particular research worker who is suited to work on problems *A*, *B*, *C* and *D*, may in the period of the program engage in successful work either on problems *A*, *B* and *C together*, or on problem *D alone*. There is, therefore, uncertainty in the determination of intellectual manpower capacities, due mainly to erroneous estimates.

3. *The evaluation of the results to be expected from each problem.* When the possible problems to be included in the program of a research establishment have been listed and arranged, moreover the necessary and the available expenditures for the research projects and the introduction of their results have been compared, it is necessary, either as the following step or simulta-

neously, to carry out the next important task, which is *to estimate the expected results of research* on the various problems.

The estimation of the research results that may be expected for the various problems may take place in three stages. In many cases, however, the three stages occur *simultaneously* in the thinking of the person doing the estimating, instead of being separate.

First an estimate must be made of the magnitude of the optimally attainable research results, *i. e.* the significance of the various problems, considered from some particular point of view.

Secondly the *probability* of the *attainment* of the various results must be estimated.

The *third* step is to determine the *expected result*, from a synthesis of the two previous estimates.

The research results which can be achieved in the various problems can only be well expressed numerically, if the introduction of the research result in production results in a properly mensurable economic effect (*e. g.* an increase in productivity, a decrease in costs, a decrease in foreign exchange expenditure, *etc.*). This, however, is not always the case. Moreover the *significance of a research problem* may — depending on the aim of the research project — be *highly variable* and dependent on very different criteria, so that a different set of aspects have to be considered in each particular case. (In those cases where the utilisation of a research result leads to structural changes in the realm of production the effect of the research result can generally only be very roughly estimated.)

The sense in which *probability* has here been used, is more that of the hopes or doubts of experienced research experts, with regard to the success of research on a particular problem. This interpretation of probability is therefore, in the majority of cases, only a subjective estimate of probability in the mathematical sense. An interpretation of this kind is necessary, because the precise determination of probability is rarely possible (as mass phenomena are here rare), moreover because this is the procedure actually adopted in practice in forming a previous estimate of the success of a particular research projects.

In consequence of these interpretations of the attainable results and of probability, the *expected result* is also not used in the mathematically precise sense of a "predicted value", but in that of a comparative evaluation of the attainable result and the probability of its attainment.

When estimating the *expected results* of research problems, it is frequently the case in practice that only statements of the following types may be made:

The research result attainable in problem "A" is of *great significance* and according to the evidence of the research carried on so far, and of the literature available, *there is a great probability of a result being achieved.*

The result which may be achieved in problem "B" is also of *great significance* but — since the research workers available have insufficient experience in this field — *the probability of a result being achieved is small.*

Statements of this sort may — if they are sufficiently well-founded — serve as the basis for various decisions.

In most actual cases the only statement that can be made is that the research result that is attainable in one problem is of *greater significance* than that attainable in another.

The arrangement of the list according to the order of the results attainable through the various problems, is another place where the method used in the examinaiion of the economic models of production may be put to good use.

These items of information do not, however, in most cases permit the research problems to be arranged in an unequivocal order of *significance.* At best they can be panged into a few groups, according to their significance (*e. g.* problems of medium, or of the greatest significance).

Having accomplished this arrangement or grouping of the problems according to their significance, they must be arranged according to the *probability* of attaining the planned research result. Experience, however, shows that in many cases, even after obtaining the opinions of well-versed experts, the problems may at best be *classified in a few groups* (*e. g.* problems with small, medium, or great probabilities).

Once an estimate has been made for each problem both of the magnitude of the result that can be achieved (its significance) and of its probability, *i. e.* the problems have been grouped according to these two approaches, then the third step may be taken, when they are arranged in the order of magnitude *of the foreseeable results.* This arrangement may take place according to various considerations in *numerous ways.* One possible method of arrangement, for instance, is the following.

If the problems may, both in respect to the significance of the attainable result and the probability of success be divided into three groups each, then they may be arranged according to the following pattern:

The prospect of success	The significance of the subject: small	medium	large
large	1.1	1.2	1.3
medium	2.1	2.2	2.3
small	3.1	3.2	3.3

Fig. 1

The various problems are inscribed in one of the intervals of the above pattern. Let the problems in the interval determined by the i-th row and the j-th column be distinguished by the double index (i, j). Thus a research problem of medium significance with a great probability of success is allocated the suffix (3, 2). Obviously, several problems may be allocated the same index.

Once arrangement according to the pattern has been completed, the problems may be ranged in order according to the results to be expected.

First *the unequivocally decidable main aspects* will have to be examined, according to which the problems possessing the same indeces and thus belonging to the same interval may be ranged.

If the problems are to be arranged in order of *decreasing foreseeable, results*, then it is obvious that of the problems present *in a particular row*, those with *higher column indeces will precede those with lower column indeces.* Thus among the problems where results can be expected with medium probability, those of greater significance will precede those of smaller significance in the order established.

Another unequivocal feature in drawing up an order is that *of the problems present in a particular column those where the result may be expected with greater probability* (*i.e.* those with higher line indeces) *will precede those where the result may be expected with smaller probability.*

If, moreover, in the above pattern *the problems along the main diagonal* joining the interval (1,1) and the interval (3,3) are examined, *a further obvious approach to drawing up an order may be observed*, in that the problems marked (3,3) take precedence over the problems marked (1,1).

Similar statements may be made with respect to the problems along the subsidiary diagonals, parallel to the main diagonal. The problems marked (2,3) precede those marked (1,2), moreover the problems of the type (3,2), precede those of type (2,1).

The above three main criteria of arrangement are not always sufficient unequivocally to decide the order of problems. The criteria do not enable a decision to be made on the precedence over each other of, for example, problems with suffixes (1,3) and (3,1).

A closer examination of the aspects that have been described for arranging the problems in order, will show that in practice it is mostly, though frequently not consciously, according to these considerations that decisions are taken over the supposed precedence of a particular problem over the others. So far, therefore, we have done no more than describe the correct procedure for evaluation that is actually adopted by the *suitable* leaders of research, setting out the main features.

If the results that can be achieved in the realms of the various problems could always be well-expressed numerically and the probability could also be estimated more accurately, then the exact mathematical expression of the "predicted value" could be used to achieve an unequivocal order of the problems. In this case, having reference to the above pattern, the expected values belonging to each particular interval would be proportionate to the product of the line and the column indeces.

1	2	3
2	4	6
3	6	9

This mode of evaluation satisfies the main criteria of arrangement which have been set out, in that *the values increase monotonously from left to right by rows, from top to bottom by columns, and from top left to bottom right by the main and subsidiary diagonals from left to right.*

In most cases, however, as has been pointed out, it is not feasible accurately to determine the predicted value, so that the order of precedence *along the diagonals from top left to bottom right is not always unequivocal.* Difficulties of the same type also arise if the evaluation of the problems does not take place by a separate analysis, according to their potential results and probabilities, but by direct estimation.

In order for an evaluation to be useful for programing it is necessary for the evaluating figures ordained to each of the problems to possess the following additive property: If two groups of problems are chosen at random from the list of problems, the sum of the evaluating figures belonging to that group must be larger, in which the foreseeable results of research are greater.

It is not, of course, an easy task to find a method of evaluation that satisfies this condition, for it becomes necessary to decide such questions as: Does the *joint* expected result from a problem of great significance and medium probability and one of great significance and small probability equal the expected result of one research problem of great significance and great probability?

In order to supply the correct answer the technical, scientific and economic experts on research and the mathematicians doing the programing, will have to engage in thorough investigations to consider the correct evaluation. It may be, however, that the evaluation cannot be carried out *unequivocally* and that realistically *only some considerations towards evaluation can be given*, in the shape of certain relations, *e. g.* that "In the order of problems according to the expected result, research on the problems in places 5 and 6 jointly promises greater results than on problem 4." In this case it is expedient to find *several methods of evaluation* such as the above evaluating functions depending on the line and column indeces of the pattern discussed. These must comply with the realistically definable main requirements, but may in other respects differ from one another.*

* One such function for instance is

$$a \cdot i + b \cdot j = V,$$

In the above example the evaluation of research problems according to expected results took place through a pattern consisting of three lines and three columns. It is possible, however, also to prepare patterns of more or of less lines and columns, depending on the accuracy with which the significance of the attainable results and the probability of the attainment of the results can be estimated.

Arrangement and evaluation according to patterns of this type is particularly useful if a large number of problems may be considered, when drawing up the research program.

The pattern discussed, for instance evaluated the research problems according to some kind of significance and the probability of success in that piece of research. This method of consideration *must not*, however, *be accepted as the only possible evaluation*, for there may be cases where, for example, the significance of the problems and the expected duration of the research or the probability of success and the duration of research, etc. ought rather to be considered. Nor are the seemingly unequivocally decidable main approaches to arrangement, which were discussed in connection with the use of the pattern (see p. 191) always fully unequivocal. (There may, for instance, be overlapping between the problems included in the various intervals.) Problems may arise in the pattern in the arrangement not only of the problems along the diagonals from top right to bottom left.

In describing the above method of evaluation and the pattern used for it, it has seemed expedient to discuss a method that is *generally applicable* and which, for this very reason, omits the solution of matters of detail. No reference or proposal has purposely been made with respect to either the numerical expression of the various "*probabilities*" (which may obviously be done in several ways of which any may be preferred, precisely because an opportunity for objective measurement is relatively rarely available), not to the definition of the "*significance*" of the various problems (or groups of problems), for if this can be precisely stated numerically it needs no explaining, while if it may wholly or partly only be estimated, many possible methods of evaluation may again be available.

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where i and j are the line and column index respectively of the interval being considered, a and b are positive numbers, where

$$a + b = 1 .$$

V is the evaluating figure for the problems in the interval of the suffix (i, j), *i.e.* one proportionate estimate of the expected result.

The above function formally complies with the main considerations for arrangement that have been mentioned, and the main conditions posed. The values of a and b must not, for the moment be precisely defined. To *each* pair of values given to a and b, there belongs one particular order of evaluation of the problems. The variants in order belonging to the different pairs of values a and b, may be well used in the actual job of drawing up a program.

The purpose of statements of this type is precisely to afford the various institutions an opportunity to consider the various matters of detail necessary for application according to *their own particular characteristics.*

If the task is to determine the "*significance*" of particular problems, a series of possibilities may arise, *e.g.* the ratio of expenditure to returns, the time required for realisation, the better utilization of raw materials, savings in imported materials, the avoidance of grave damage to the plant, the production of better quality with more healthy working conditions, the cheaper and better satisfaction of domestic demand, the better exploitation of the possibilities of the international market and thus the obtaining of foreign exchange, *etc.* as well as the combination of the possibilities that have been enumerated.

There are similarly many ways of evaluating the "*probability of success*". It may, for instance, be possible to determine certain multiplication factors based on empirical data or on other substantiated considerations, which can be used to express the various probabilities numerically, and so forth.

All these matters of detail may best be decided using a particular pattern that best corresponds to the requirements of the given case, so that their discussion in too much detail at this juncture is superfluous.*

4. *The selection of the research program.* The preparation of the research program, *i. e.* the selection of the problems that can be considered and with which the research institute is to be concerned in the period of the program, may and does take place in a number of ways.

The group of selected problems, which is now a research program, must comply with the following *three main conditions:*

1. The total of expenditures belonging to the problems in the program may not exceed the capacities available.

2. The program must include those problems, research on which, as a result of the evaluation, is absolutely necessary, or possibly independently

* The international literature has so far carried very little material on the numerical methods of investigating the economy of research. It has only been recently that a few authors — mainly in the Western press — have attempted to discuss the matter. These authors can, according to the content of their work, be divided into two classes. Members of the one category seems to have a quite fair acquaintance of the problem but write so nebulously as to permit very little to be understood (obviously because they wish, before publishing their results, to "sell them" to those interested, and the purpose of their paper in this case is merely to advertise their knowledge).

In the works of the other category — though the number of such works is very small — interesting information may be found. The trouble here is that the authors attain their results through very considerable simplifications and the application of fairly mechanical methods. For instance such main criteria for the evaluation of particular problems as the probability of success, the duration of research, the order of magnitude of the necessary expenditure, the various market possibilities and the expected profits are formally allocated "points" and this seemingly obviates the various uncertainties in evaluation. If the results of marking by points are set up in tables for all possible research projects, and arranged in order of magnitude, there we have the desirable *order* of the research program.

In actual fact, of course, the matter is far from being so simple.

of the programing, the so-called *compulsory* problems, provided that the expenditure on these problems does not exceed the available capacities. (In this latter case the research establishment must in the first place consider its own capacity.) Complusory problems are generally included in the program on the instructions of the State direction of research or because of research contracts that have been concluded.

This latter, "inevitable" part of the research program may also contain problems that have "survived" from the previous program period, though of course only if the capacities of the research institute and those needed for realisation have not changed in such a way that the inclusion of these problems in the program would itself contravene condition 1.

Apart from the above limiting conditions determined by the selected, and the so-called compulsory problems, there may occasionally be *special conditions* (*i.e.* if a particular problem also contains other problems or certain problems exclude one another from the program). The appearance of such special conditions generally depends on how the main groups of problems were broken up into separate problems when the list was compiled.

3. A program that complies with the above conditions must also fullfil another stipulation, that the expected result associated with the program should be the maximum.

Several methods may be found to ensure the preparation of a program that complies with the above conditions 1., 2. and 3. We shall here only treat some of the main problems that arise from *the peculiarities of the subject.*

F. Professor of Economics J. KLÁR, Budapest XI. Műegyetem rakpart 3., Hungary.

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