ECONOMIC AND SOCIAL QUESTIONS — WIRTSCHAFTS-WISSENSCHAFT UND PHILOSOPHIE — НАУКА И ПРОМЫШЛЕННОСТЬ

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

By

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(Received September 10, 1960)

The literature on economics has so far published little on the measurement of the economy of industrial research, though the subject is of great importance. The inadequate investigation of this problem may probably be attributed to several reasons. One generally effective reason that may perhaps be adduced is that it is a matter of common knowledge that the measurement of the economy of research in the so-called fundamental sciences is a fairly hopeless undertaking. Thus the mist that in most cases necessarily shrouds the economy of fundamental research through its very nature, may surreptitiously and incorrectly also to have spread to the similar problems of industrial research.

The *further reasons* of the lack, or inadequacy of examinations of economy, are that some of those interested (the firms and research establishments) have not delved sufficiently deeply into a study of the matter or have not *sufficiently recognised the significance of the problem* and thus the need for studying it. This is also apparent from the fact that the statistical reports of several institutes, and the way that they are elaborated are not infrequently unsuited to rendering correct and swift answers to some of the important problems of the economy of research. A further reason is that *the theoretical question of the economy of research, has itself not been sufficiently unequivocally defined*. Last, but not least, *the applicability of the methods of measurement is not yet sufficiently clear* and there is little experience available in respect to their use, due to their completely experimental character.

The investigation of economy has become a prominent feature in every branch of the people's economy. It is therefore obvious that the economy of industrial research cannot — despite its numerous peculiar aspects — be an exception.

There are spheres of industrial research — not insignificant ones at that — where *an attempt at the application of exact methods of measurement* *cannot be considered hopeless*, though there are also many where there is, indeed, not much hope for the measurement of economy.

The various firms and research establishments must therefore achieve a *previous evaluation* of their programs for research subjects. Any previous evaluation of the possible research programs and the establishment of an order precedence among several alternative schemes, requires *decisions* for the choice of the most suitable variant. It follows, moreover, that if there is no alternative scheme and the task in hand must necessarily be solved, as in the cases of defence, public health, etc., then the only decision involved is which of the several research establishments that could be considered, will be most appropriate for the task concerned.

Beyond previous evaluation and thus the preparation of a research program, the second task is to *examine the economy of the research results that have been achieved.*

The evaluation necessary for the preparation of a research program may take place partly with the use of quantitative methods.

The application of quantitative methods for the economic direction and the evaluation of the results of industrial research is a very difficult task. There is relatively little experience in this field, the opportunities for experimenting are limited, moreover the instructions contained in the western literature are frequently of very dubious value.

Even if the necessary conditions were available, the achievement of noteworthy results would require the *permanent and close co-operation* of technical and industrial experts, scientific workers experienced in industrial research, mathematicians and economists, to an extent that has not yet evolved — at least not in a widespread way.

The works *technical experts* who are conversant with the realisation of the results of research, frequently have less knowledge of some of those aspects of research work which are significant for our purpose. Those *in charge of research* not infrequently trust in the procedures they have hitherto evolved, in their own experience, judgment, and not least, their routine. They are not particularly eager to bother about overcoming the difficulties in the way of the introduction of quantitative methods, which in their opinion do not have much to offer them. Some of the *experts* in applied *mathematics* frequently lack acquaintance with certain points in economics, which it would be important for them to know. Finally, there are few, even among the well-trained *economists* who are equally at home in the complex interconnections of industrial research and the various branches of technology and are at the same time acquainted with the mathematical methods available.

From all this it follows that those engaged in experiments on this subject may easily adopt an erroneously one-sided approach, in which they willy-nilly overestimate the role of their *own* scientific discipline by training, within the *sum* of knowledge which is required.

It is frequently the case that those in charge of research work overestimate their own methods for the investigation of economy. These tried methods may, indeed, have proved useful, but they are now often out of date and lead to an underestimation of the help that can be obtained from the application of the new procedures. There may also be cases of the contrary event, where the use of quantitative methods may lead to the desire to "compute" the uniquely correct decisions in a mechanical way, completely forgetting that while these methods may indeed provide valuable numerical information that is a good reflection of the true situation over a particular problem, they may hardly serve as substitutes for the appraisal, based on the necessary knowledge of the facts and of the situation, which must precede any correct decision.

A problem which automatically arises in view of the inadequate experimental and factual information available, is *what realistic aim can be set* with some measure of success, for an economist who is well-informed on the main economic problems of industrial research, is intent on understanding the quantitative methods involved and aware of the manifold requirements of the analytical work that is needed?

At the present stage of development and with a view to all the circumstances that have been considered, we must for the time being obivously be content with a *reference to some of the important features* of the problem involved, and try to deduce the main conclusions from these special traits. At the same time, we must try and find a *possible point of* departure to which we can draw the attention of all those whom their various special skills render fit for improvement and later, the development of *good* methods. However trifling, and for the time being insignificant such an effort may now be, it is not possible to make swift and secure progress along mainly untrodden, unexplored paths and even a mere start made along the new path may be more valuable and may lead to faster results, than standing still. The more people there are in future *together* to smooth this path — research workers, technologists, economists and mathematicians, each in his own field — the faster we can get the start turned into completion.

Attention must, however, be drawn at the outset to the fact that *the use of the quantitative methods* which may be tried in the investigation of the economy of industrial research *must not in general be expected to yield more than what can at best be attained by these methods, i. e.* the provisision of one type of help in the rapid and sound survey of numerous types of information.

In respect to the quantitative methods that are at present available, considerable help may be expected *in the preparation of research programs,* *while in the evaluation of the results of research* — apart from the examination of expenditure and yield — their usefulness is more limited.

Two approaches that are basically different for the application of quantitative methods, *are possible* in the preparation of the research programs.

The *one* occurs when the necessary *important information*, such as the significance of each problem according to some criterion, the probability of success, the total expenditure of labour and of resources necessary for the achievement of the planned aim and finally the value of the expected result, *may be precisely determined* beforehand.

In this case the preparation of the research program is similar to the drafting of a production schedule.

Due, however, precisely to the peculiar nature of research work, this case is by no means frequent. The significance of the use of mathematical methods in this case lies in the fact that they may make it possible for the person in charge of research to undertake *a more rapid and thorough survey of more alternative suggestions.*

It is very frequently the case that even the *initial information* necessary for the preparation of a list of possible research problems (*e.g.* the significance of the various problems), is to a large extent only available in approximate form, when it may only be *estimated* with broad margins of error, and moreover when the *evaluation* of the research results *that may be expected*, also *includes numerous uncertainties* (*e. g.* the probability of the success of research on the various problems).

This case in the preparation of programs for research is, due to the peculiarities of research work that distinguish it from many other types of work, a *characteristic* one and must therefore be discussed in greater detail.

An attempt may be made for *an approach* making use *also* of the help of quantitative methods, where the peculiarities of industrial research work are adequately considered. Such an approach, based on the available facts, would be a source of preliminary and general information, compiled with the co-operation of experts in the various field, which could later be further refined and developed. Some of those in charge of research may, indeed, still be averse to supplementing or checking their — possibly sound — logical approach to the selection of research problems, the "inspiration of their sense of judgment", by the use of quantitative methods that may frequently appear to be complicated and generally require the use of a computer. However confident these people are in their routine, *a new development of some sort* is certainly to be expected, as is the case with the programming of production, and this is all the more so as the use of the available methods will, *if correctly interpreted*, by no means be a *substitute* for the work of the experts whose job it is to make the decisions, but will only *reinforce* it, improve it, and put it on a sounder footing. It is therefore probable that in this sphere even so modest an experiment as the present one is justified and necessary, for it may serve to give the first impetus to those who are looking for a new approach and also to those experts in the various fields of scientific activity, who will only be able to mark out the path of progress through *co-operation*.

An acquiescence in the imperfect nature of the methods so far available for investigations of the economy of industrial research will, if it is not exchanged for a search to find new and more efficient methods, not extricate some firms from their present lazy and harmful sloth and compell them to engage in rapid modernisation and a satisfactory measure of technical development (even though the latter were, temporarily, to influence the previous level of profit-sharing). At the same time some of those in charge of research may, if they do not feel the necessity for trying new methods in this field, continue to tolerate a situation where, for instance, in their research establishment they cannot *satisfactorily* show what actual results have been achieved in the general development of the industry concerned and in particular, in the technical development of the firms belonging to the establishment. This, of course, involves a system where the requirement to "show cause" is not a more or less purely formal one and should not provide an excellent opportunity for some people by reference to hazy reasons to make similarly hazy research seem valuable for the development of an industry or a firm, whereas the firms may, in fact, need *less spectacular, different* aid, that would relieve them of their gravest development problems.

In brief, the research and planning institutions must — as the saying goes — laugh together and cry together with the firms in their field, eliminating all competition between them, over the application of a particular achievement. They must be responsible together, for any failure in their plans, as though they were the organs of *one and the same* institution, engaged in differing tasks but striving for the same ends. For success achieved together, they must be rewarded together, for failures, called to account together. *Industrial* research establishments, which are engaged in more or less realistic projects which can be determined at foreseeable dates, cannot confuse either their aims, nor their economic approach with the tasks of fundamental research which operates with very considerable uncertainties and a completely different set of premises.

Returning, after this digression, to an account of the initial experiment to help formulate a research program, it must first be stated that the conditions for the mathematical method that can be used are, for the reasons that have been pointed out, not so favourable as when all the necessary information may be precisely measured and presented in numerical form. What then, are the main features of the work that becomes necessary and what, with a view to the peculiar features involved, is the detailed procedure to be followed?

Some of the main features — only in general terms for the moment — are the following:

a) The *large number of possible problems* and hence the need for the preparation of a precise list of research problems;

b) the considerable uncertainly of *evaluation* (with respect to the significance and probability of the various problems) and hence the *large number of probability factors;*

c) the relatively *large number of possible research programs,* which must, through the retirated examination, correction and improvement of the initial information, be *narroved down.*

After this brief survey of the *general* features, we must next proceed to a detailed consideration of the main steps necessary for the preparation of a research program. These are:

1. The establishment of the possible *sources* of research problems;
2. an *investigation* of the necessary and possible *expenditures;*
3. an evaluation of the results to be expected of each problem;
4. the selection of a research program.

1. *The possible sources of research problems.* One of the most important aspects of the direction of industrial research is *the preparation of the research program.* It is the correctness of this program that determines the efficacy with which an industrial research establishment can accomplish its purpose. The question is: What are the sources and the methods which will permit a research program to be compiled for research establishments, such that its execution will lead to the best possible results.

The determination of the optimal program for research is in many ways different from the optimal programming of production. For one thing, in the latter case numerous phenomena that *cannot be numerically expressed* at all, or only with difficulty, must be taken into account, while in certain cases the uncertain information gleaned from *forecasts* must be used. The preparation of a research program requires *above all* the opinions of experts with considerable practice in research and in production. For the time being there is no quantitative method available, whose results could make up for the experience of these experts.

The preparation of a research program also involves the use of numerous statements that are based on estimates. These statements rarely contain numerical information, but for the greater part they nevertheless also *conceal a quantitative relation of one type or another.* Thus, for example, however well the director of a research establishment may know the personnel of his own institution, it is not probable that he will be able to give a realistic *numerical* appraisal of the abilities of his research workers. When, however, he says that research worker X is more suited for work on a particular theme than is Y, then the relation that is here established is one that is in effect a basically quantitative statement. Similar types of statement are, for instance, that the *probability* of attaining results in research problem "A" is *greater* in problem "B", or that the research results expected to be achieved in problem "P" *are of greater significance* than those in problem "Q", etc. The consideration of a *large mass* of statements of this type when the research program is prepared is a complicated task, and it is in this respect that there is a possibility for applying certain quantitative methods. In the course of the setting up of programs use can be made of quantitative methods which can ensure the *consistent* consideration of the initial statements made in respect to the research program, provided that these statements contain certain quantitative relations.

Any attempt to trace the preparation of the research program of an independent industrial research institution will show that the first task is *to draw up a list of the research problems to be considered.* The problems that can be considered for the program of the research establishment may be taken *from three sources.*

The *first* is an examination of the process of production and of the requirements of production.

The *second* comprises the opinions, ideas and research enthusiasm of the research staff of the institution.

Third are some of the conclusions that may be drawn from an investigation of the general world level.

The most important of these three sources *is the first,* for an industrial research institute must, if it is to be true to its purposes, take its research problems in the first place from an investigation of the requirements of production and the opinions of the experts on production. It is therefore necessary to ask those in charge of production, the technical and economic experts, for their opinion. Without this, industrial research could very easily become divorced from practical life. An investigation must, moreover, be carried out to find out which characteristics of production need changing for the best possible satisfaction of the economic requirements of production.

This work can be facilitated by the *examination* of the so-called *economic "models".**

* The problem of economic "models" is subjected to thorough analysis in a paper by András Bródy. (A. Bródy: On the problem of economic "models". Közgazdasági Szemle, VII. 8–9., 1960. p. 954) Here Bródy points out the very *changing* nature of the sense of the word "model" itself. The school of bourgeois econometrics which has enriched our terminology with this word, does not give a precise definition of the concept. In his opinion the "scientific" approach of bourgeois economists, concealed behind such models, leads to the world of mathematical economics which Karl Marx considered a vulgar trend, that "finally puts an end to even the possibility of a historic approach to the economy and to society".

Quantitative methods of investigation have become widespread in matters of production and of economic activity generally, and the utilization of their results is in most cases successfull. The essence of these methods is, by means of certain abstractions to set up the so-called *mathematical model* of the phenomenon being investigated. This gives an approximate picture of reality, formed by neglecting certain immaterial features, and its analysis can give an answer to problems connected with the phenomenon, it can describe the probable course of certain processes, etc. Models of this type, for instance, make it possible to determine what is, from a particular point of view, the optimal production program of a factory, they permit the main conditions for the increase of production to be found, etc. Once an economic model of the activities of a factory is available, which is adequate to allow the optimal production program of the factory from a particular point of view to be determined with its aid, this model *contains as its parameters* — among others — for instance the specific expenditure necessary for the production of certain products, the capacities available, it describes the relations between quantities produced and the necessary expenditures, etc. The *alternation of the parameters* in the model at the same time involves a change in the optimal program, for to each configuration of the parameters of the model there belongs a specific optimal program, with a corresponding value of the inverse function. In order to obtain information for practical purposes from the model, it is frequently necessary to consider very many factors and its manipulation — even in the case of the simpler, so-called linear models — is often only possible with the help of *high-power computers*, for only these enable an investigation of a multi-variable model to be carried out in a brief space of time.

In several instances very valuable information to facilitate the direction of industrial research may be gleaned from these models of production because they reflect the process of production and some of its economic aspects in a form that permits of mathematical treatment. Their examination frequently provides an answer to the questions of how *a change in certain of the parameters of production affects the economic balance*, how sensitive the economy of production is, to a change in certain of the parameters of production. They also render an answer to the problem of *how much certain of the parameters of production have to be changed*, to attain an economic result that is desired. Since, moreover, any change in the parameters of production may also be due to the introduction of a research result, the established sensitiveness of lucrativeness to the alteration of certain of the production characteristics may call attention to spheres of production where the introduction of research results could lead to economic advantages.

Similarly, if the aim that is set happens to be the attainment, for instance, of a particular economic result by changing certain of the parameters of production, the research and economic experts can tell whether a change in the parameters considered can be achieved by research, and what foreseeable research and economic expenditure would be necessary to achieve the desired result.

The examination of such models of production can thus *draw attention to subjects where research work may become necessary and also provide information on the significance of this work.*

As a first step in this work, the task is to *collect* the research problems that can be considered at all, and to *establish* their approximate *order of* importance, without going too deeply into an analysis of the various questions of detail. In this case then it is, so to speak, the "by-products" of investigation carried out for the development of production, that offer valuable conclusions for some of the tasks of industrial research.*

The *second main source* of research problems is an examination of the possibilities of the research institute and practically of the *research personnel.* Among the research workers in a particular field of industrial research there are always some who are particularly suited for the investigation of certain problems. These problems must also be included, because on the one hand the special skill of certain research workers in some problems can not be neglected and on the other hand experienced experts in research may also raise problems which may not have occurred in the course of the investigation of production, but are nevertheless of great significance.

An important point moreover, in carrying out investigations of this type is that in order to ensure a good general view of the situation only changes in those parameters can be considered, which do not suffice to change the structure of the model. There may, for instance, be research results that can lead to a change in the structure not only of the firm concerned, but of a whole branch of industry. In this case the problem of the utility of the model depends on the extent to which the effect of a planned research result such as this, and the further progress of that result may be anticipated.

A source of problems independent of the previous two, and *the third main source is the study of the world level.* An acquintance with results obtained abroad may also raise problems, especially if the foreign research results concerned are not available, or if they are available, but their adaptation to domestic conditions requires further research work.

* The accuracy and reliability of the results calculated by use of a model *generally* depends — according to a statement in the paper by BRÓDY, which, has been quoted — on whether the three possible types of sources of error arising in connection with the setting up of models, or rather the effects of these errors, were properly taken into account. They are the limitations even of a correctly set up abstraction and errors, caused by accident and by measurement.

In concrete cases, in the examination of the models of production, certain of the parameters of these models can, of course, only be varied between limits (*e. g.* the financial expenditure devoted to research, the scientific manpower available, etc.)

The problems taken from the above three sources may *overlap* and parts of certain problems may even occur separately.

Since any list of research problems may contain several themes that are parts of one another, it is necessary to *arrange the problems*. The *aim* of arrangement — with a view to the later elaboration of the problems — is to set up a list of problems which excludes the possibility both of the various themes being broken up into too small details and also of their being raised in wholly general terms, in an insufficiently concrete form. Both extremes make it difficult to draw up a program. It is, therefore, from this point of view, incorrect to amalgamate several related problems whose significance, the development of their main parameters, etc. are different. It may thus be correct to let important partial assignments, which may be regarded as independent pieces of research, figure as separate problems. In so far as various types of research work may be conducted within a particular problem, all with differing foreseeable results, then instead of one problem, it is best at the outset to include the several mutually exclusive variants in the list of problems. When evolving the proportions of the list of problems, the *principle of arrangement* which, though ideal, may in fact be very difficult to put into practice to even an approximate degree, is one which will help facilitate the application of the method for the evaluation of the problems. This is that the expenditure of research effort necessary for the various problems should be distributed as evenly as possible, or that at least there should not be extremely large differences between the various problems included in the list.

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To be continued in our next issue.

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BOOK REVIEW — BUCHBESPRECHUNG

Tables de Constantes et Données Numériques — Organisme Affilié de l'Union Internationale de Chimie Pure et Appliquée — 11. *Constantes Sélectionnées — Pouvoir Rotatoire Naturel IV. Alcaloïdes* Par J.-P. Mathieu et M.-M. Janot. Préface de Sir R. Robinson.

Éditeur: Pergamon Press Paris — Londres — New York — Los Angeles — Francfort 1959. Publication Subventionnée par les Pays Adhérants à la Commission Internationale des Tables de Constantes et par le Centre National de la Recherche Scientifique. 212 pages.

In Anbetracht der großen therapeutischen Bedeutung der Alkaloide nimmt die Alkaloidforschung ständig an Ausmaß zu. Bei der Isolierung der Alkaloide aus natürlichen Rohprodukten, sowie bei ihrer Reinigung, Identifizierung und der Strukturaufklärung neu entdeckter Alkaloide spielt unter den physikalischen Konstanten die Bestimmung des optischen Drehvermögens eine hervorragend wichtige Rolle.

Die vorliegende Zusammenstellung leistet den Chemikern, die sich auf dem Gebiete der Alkaloidchemie betätigen, große Hilfe, da sie viele, in der Literatur zerstreute und in vielen Fällen nur schwer zugängliche Daten kritisch gesichtet in einem einzigen Bande zusammenfaßt. Das Werk ist nicht bloß eine Tabellensammlung physikalischer Konstanten, sondern überdies gleichzeitig ein Verzeichnis der bekannten Alkaloide; es sind in ihm nicht nur die optisch aktiven Alkaloide aufzufinden, sondern auch diejenigen, welche keine optische Aktivität zeigen und weiterhin solche, deren Drehvermögen zur Zeit noch nicht bestimmt wurde, vorausgesetzt, daß ihre empirische Formel bekannt ist; so wird es möglich, bei Bestimmung neuer Daten dieselben in den Band einzureihen.

Die Tabelle »Optisches Drehvermögen« enthält die bekannten Alkaloide in alphabetischer Reihenfolge ihrer Trivialnamen; an erster Stelle wird der Name angegeben, welchen der Entdecker des betreffenden Alkaloids ihm gab, darauf folgen die öfters gebrauchten synonimen Bezeichnungen, die aber auch an der entsprechenden Stelle des Alphabets wieder angeführt werden. In den auf den Namen folgenden Spalten sind die empirische Formel, das Molekulargewicht, das Lösungmittel, die Konzentration, die Temperatur, die Wellenlänge und schließlich das unter den angegebenen Versuchsbedingungen bestimmte optische Drehvermögen zu finden. Die letzte Spalte enthält die Literaturhinweise, welche unter Beachtung der ab 1873 bis zum 1. Juli. 1959 erschienenen Mitteilungen zusammengestellt wurden.

Im Kapitel »Bibliographie« ist die zitierte Literatur nach Jahreszahl, sodann in alphabetischer Ordnung der Titel der Zeitschriften und weiter der Autoren zusammengestellt. Ein Autoren- und ein Formelregister erleichtern die schnelle Orientierung im Band.

In dem zum vorliegenden Band geschriebenen Vorwort hält es Sir R. Robinson für notwendig (»möglicherweise als in der Wüste verhallenden Ruf«), auseinanderzusetzen, daß er die Cahn-Ingold-Prelogsche Bezeichnungsweise für einen überflüssigen Beitrag zur Nomenklatur halte; seiner Meinung nach könne durch die symbole D und L — unter Anwendung der sich auf die Rangordnung der Gruppen beziehenden Regeln — ebensoviel ausgedrückt werden, wie durch die Symbole R und S.

Das Titelblatt der Sammlung, das Inhaltsverzeichnis, das Vorwort, die Einführung, die Anleitung zur Benutzung des Bandes, die Liste der Zeichen und Abkürzungen sind neben dem französischen Text auch in englischer Sprache gegeben. Die tabellarische Zusammenstellung, die Bibliographie und das Sachverzeichnis sind in französischer Sprache verfaßt.

Papier, Druck und Einband sind musterhaft; die Benutzung des Bandes bietet neben der wissenschaftlichen Hilfe auch einen ästhetischen Genuß.

D. Beke

Conference on Hyperconjugation

Indiana University, Bloomington, 2—4 June 1958. Pergamon Press, London—New York—Paris—Los Angeles, 1959.

The book under review contains sixteen major papers heard at Bloomington where a conference on the subject of hyperconjugation was arranged by the Indiana University in 1958. The lectures were delivered in six sessions. The three papers read in the first session treated the following subjects: thermochemical evidences for hyperconjugation (R. B. Turner); effect of hyperconjugation on solvolysis rates of benzhydryl chlorides (W. R. Moore et al); role of solvent in alkyl substituent effects (W. M. Schubert et al.). The lectures held in the second session dealt with the following themata: hyperconjugation effects of para-alkyl groups (R. W. Taft, Jr. and I. C. Lewis); experimental and theoretical evaluation of the Baker-Nathan effect (M. M. Kreevoy); a simple molecular orbital theory of hyperconjugation (A. Streitweser, Jr. and P. M. Nair). The most lively discussion in the third session was released by M. J. S. Dewar's lecture: "A re-evaluation of conjugation and hyperconjugation: The effects of changes in hybridisation on carbon bonds". Dewar affirms that neither conjugation nor hyperconjugation plays a significant part in molecules termed by him "classic", *e. g.* in butadiene or propylene. The other two papers heard by session III treated of the connection between bond length and hyperconjugation (L. E. Sutton), and the significance of isovalent hyperconjugation (R. S. Mulliken). Two lectures were delivered in the fourth session: on hyperconjugation in aromatic cation complexes (D. A. McCaulay and A. P. Lien) and on the experimental evidence for NH and OH hyperconjugation (P. B. D. De La Mare). Session V heard two papers: that of J. W. Baker on the substituent effects upon hyperconjugation and E. Berliner's lecture dealing with the experimental explanation of the Baker-Nathan order. Recent results obtained from the application of isotopes constituted the subject of three papers read in the last, the fifth, session.

Although it is regrettable that the book does not contain at least the more important discussion which followed the lectures, it is nevertheless of high instructive value for the students of theoretical organic chemistry.

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A kiadásért felel az Akadémiai Kiadó igazgatója Műszaki szerkesztő: Farkas Sándor
A kézirat nyomdába érkezett: 1961. I. 7. — Terjedelem: 5,75 (A/5) ív, 12 ábra

1961.52704 — Akadémiai Nyomda, Budapest — Felelős vezető: Bernát György