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ON SOME TRENDS IN THE DEVELOPMENT OF ASTROPHYSICS

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INTRODUCTION

During my studies at the University of Leningrad (1925–1928) I paid chief attention to astronomical and mathematical courses. While I always felt the necessity of better knowledge of physics, at that time this discipline did not attract me very much. It is true that during my last two university years the logical beauty of quantum mechanics as well as of some aspects of statistical physics impressed me deeply. But even now I feel that my knowledge of physics was incomplete and insufficient for a theoretical astrophysicist.

Perhaps this circumstance, as well as a lack of physical intuition, were the reasons why during the fifty years of my scientific work I have concentrated mainly in directions where logical consistency is of greater importance than physical insight. At the same time, I have spent much time in the study of the data obtained by observers.

Modern astrophysics deals with an unusual diversity and richness of observational data, with a huge variety of cosmical bodies and systems. These bodies and systems are sometimes of different scale and properties. At the same time, one meets here a great diversity of roads of scientific investigations and ways of thinking.

Nevertheless, it happened that my personal scientific efforts have been almost completely devoted to three main directions of theoretical work: 1. invariance principles as applied to the theory of radiative transfer, 2. inverse problems of astrophysics, and 3. the empirical approach to the problems of the origin and the evolution of stars and galaxies.

In the following pages I'll give a short review of results received in each of these three directions.

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1. INVARIANCE PRINCIPLES AND THE THEORY OF RADIATIVE TRANSFER

The problem of scattering and absorption of light in a medium that consists of plane-parallel layers was considered in the classical work of Schwarzschild, Shuster, Eddington, Milne, and Chandrasekhar. In essence, their method was connected with the consideration of the balance of radiative energy in all elementary volumes inside the medium. As a result the problem in each case can be reduced to some integral equation with the kernel $\mathrm{Ei}(/\tau - t/)$ where Ei is the so-called integral logarithm

$$\mathrm{Ei}\, y = \int_1^\infty e^{-yt} \frac{dt}{t}.$$

The case of isotropic and monochromatic scattering is comparatively simple. But the general problem of anisotropic scattering with some law of redistribution of frequencies (which is important for the theory of absorption lines) is connected with many complications and difficulties.

Early, as a university student, I tried to contribute to this field. My diploma work was devoted to the integral equation of radiative equilibrium. However, the first essential results were achieved only in 1932–1933 when I worked out a method of successive analysis of Lyman-continuum and L_α radiation fields in dealing with the radiative equilibrium of planetary nebulae. Before World War II I found also a simple way of considering the monochromatic scattering problem in deep layers of a medium (for example, in deep layers of the sea) with arbitrary indicator of scattering. But all this was within the framework of classical methods. Only in 1941 did I find that there are other possibilities.

Let us consider a medium consisting of plane-parallel layers occupying the half space $z < 0$ with the boundary plane $z = 0$. The parallel beam of light of the flux density πS falling on this boundary under the angle arc cos η to the normal will penetrate into the medium and suffer there innumerable elementary processes of scattering and absorption. As a result, some part of the initial beam will be scattered back into the half-space $z = 0$ in different directions. This phenomenon is called the "diffuse reflection" of light from the medium. The intensity $I(\xi)$ of the light diffusely reflected in the direction arc cos ξ will depend both on η and ξ

$$I(\xi) = Sr(\eta, \xi).$$

According to classical methods, in order to find the function $r(\eta, \xi)$ it is necessary to solve the above-mentioned integral equation for the different values of parameter η to find the radiation field for every η as a function of depth. However, we use then only the intensities at $z = 0$ which define $r(\eta, \xi)$.

In order to avoid the calculation of the data which describe the radiation field at layers $z < 0$, I decided to try to make use of the following circumstance. The function $r(\eta, \xi)$ evidently will not change if we add to the boundary $z = 0$ a thin additional layer of the thickness ΔZ which has the same optical properties as those of the primary medium.

This means that different supplementary phenomena of scattering and absorption will in this case exactly compensate each other. Writing this condition of compensation, we can obtain an equation for $r(\eta, \xi)$. The decisive point was the understanding of the fact that no quantity immediately related to the internal layers enters into this equation. The equation contains only the unknown function $r(\eta, \xi)$. The same equation which was derived in this way was then obtained also from the integral equation of radiative equilibrium itself.

The demand that $r(\eta, \xi)$ must remain unchanged when a supplementary layer is added to the boundary is called "*invariance principle.*"

In the simplest case of monochromatic and isotropic elementary processes we obtain from this principle that $r(\eta, \xi)$ as a function of two variables must have the following structure:

$$r(\eta, \xi) = \frac{\lambda}{4}\, \eta\, \frac{\varphi(r)\varphi(\xi)}{\eta+\xi} \tag{1}$$

where λ is the ratio of the scattering coefficient to the extinction (absorption + scattering) coefficient and φ is an auxilary function *of only one variable* which satisfies a very simple functional equation

$$\varphi(\eta) = 1 + \frac{\lambda}{2}\eta \int_1^1 \frac{\varphi(r)\varphi(\xi)}{\eta+\xi}\, \mathrm{d}\xi. \tag{2}$$

Thus, instead of searching for a family of solutions of a complicated linear integral equation for different values of η, we can obtain $r(\eta, \xi)$ solving only one, very simple nonlinear functional equation.

In later work, we showed how one can treat the more complicated cases of anisotropic scattering (the case of nonspherical indicatrix), applying the same invariance principle.

It was shown also that the case of finite optical thickness τ_0 can be treated by some generalization of the same principle. Adding a layer of thickness ΔZ to one of the boundaries we must in this case subtract the layer of the same thickness from the other boundary and require that both the function $r(\eta, \xi)$, which describes the diffuse reflection, and $S(\eta, \xi)$, which describes the capability of diffuse transmission, will remain unchanged. Of course, in this

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case both these functions will depend also on the value of the finite optical thickness τ_0, which enters as a parameter.

In such a way a powerful tool was developed for the solution of the most sophisticated problems of the transfer of radiation and of neutrons, which often are of much more general nature than the problems of diffuse reflection and transmission.

Before World War II, working on the problems of galactic absorption, I introduced a formalized scheme of the absorbing dust layer in the Galaxy which causes the general absorption in this stellar system. It consists of randomly distributed discrete clouds arranged between two parallel planes. The random distribution of absorbing clouds will cause the fluctuations in the apparent distribution of surface brightness of the Milky Way. Of course, this model was constructed only for the study of brightness fluctuation and is inadequate for application to other problems, for example, to the dynamics of absorbing material.

In a wonderful way the principle of invariance has opened the possibility of reducing the theory of fluctuations of brightness in such a model to a very simple functional equation. The problem was treated further in a series of papers by S. Chandrasenhar and G. Münch in a much more complete form.

During the years after the World War, I succeeded in showing that it is also possible to apply the principle of invariance to some *nonlinear problems* of radiative transfer. However, no result of outstanding significance has been achieved here.

More recently, the Armenian mathematician R. V. Ambartsumian has shown that the invariance principle is widely applicable in the new mathematical field developed by him—the combinatorial integral geometry.

At the same time, it is necessary to admit that, notwithstanding its logical beauty and simplicity, the astrophysical applications of the invariance principle are somewhat restricted owing to some assumptions of geometrical nature (in some cases the plane layers, in others the homogeneity of the medium).

Here I shall confess that I always disapproved of the methods of ad hoc hypotheses and "models" used in astrophysics without any reasonable restriction by many theoreticians. Apparently this distrust is based on the arbitrary character of such approaches and the frequent failures of their application. Of course, the theory of radiative transfer as well as invariance principles, unlike such ad hoc models, are rather useful *mathematical and logical tools* of investigation. However, some narrow assumptions that we sometimes make developing these tools are reminiscent of "model making." This is the reason why I could not restrict my studies solely to this direction

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of thought. I always tended to find new ways for the direct use of empirical data with the purpose of finding the regularities and laws governing the astrophysical phenomena. In this connection, I was always of the opinion that the approach that is now called "inverse problems" is very promising.

2. *Inverse Problems*

Just after my graduation from the University of Leningrad my attention was attracted to the following question: in what degree does the totality of empirical data of atomic physics (the frequencies of spectral lines, the transition probabilities, etc.) define the system of laws and rules of quantum mechanics or more specifically the form of the Schrödinger equation? Very soon I came to the conclusion that rigorous solution of this problem was beyond my capabilities, and I decided to concentrate on some limited and modest problem of the same kind. I found that the following narrow question is more suitable: *in what degree do the eigenvalues of an ordinary differential operator determine the functions and parameters entering into that operator*? Even the solution of this inverse problem is connected with many difficulties. Therefore, I restricted myself to publishing in 1929 in *Zeitschrift für Physik* a paper which contained the statement of the general problem and the proof of the theorem that among all strings the homogeneous string is uniquely defined by the set of its oscillation-frequencies. Apparently during the fifteen subsequent years nobody has taken notice of that paper (when an *astronomer* is publishing a *mathematical* paper in a *physical* journal, he cannot expect to attract too many readers). However, beginning in 1944, that work was continued in papers by a number of outstanding mathematicians who have succeeded in obtaining many interesting results related to the "inverse Sturm-Lionville problem."

As regards myself I tried persistently during many years to find other cases where we can directly derive from observational data regularities and laws of nature. One cannot forget that the greatest astronomical discovery in history—the establishment of Kepler's laws of planetary motions—was in essence obtained as the solution to the following inverse problem: two planets are moving around the Sun in closed orbits of which one is completely situated inside of other (for simplicity we suppose that both orbits are in the same plane). The motions are periodical but the periods are not commensurable. The observer on the internal planet is continuously measuring the longitudes of the outer planet and of the Sun. It is necessary to determine from these observations the form and relative sizes of two orbits as well as the velocities in the different points of the orbits. As the result of the solution of this problem, Kepler's laws were found. It is true that after the trajectory of Mars was found by Kepler, the method of "trial and error" was applied in order to present this trajectory as one of known geometrical figures, but it is clear that the main results have been achieved by means of analysis and solution of *the inverse problem.*

There were also other interesting examples of solution of outstanding inverse problems in classical astronomy. However, there had been few such cases in astrophysics. A well-known example is the derivation of the space distribution of stars in a spherical stellar cluster from the observed distribution in the projection on the sky. The problem was reduced to the integral equation of Abel, which has a simple solution.

In one of his popular papers Eddington put forward the following question: is it possible to find the distribution function $\varphi(\xi, \eta, \delta)$ of the components of stellar space velocities in the solar neighborhood *from radial velocities alone* without making any special assumption on the form of φ. This problem was solved in a paper that I wrote in 1935 and was presented by A. S. Eddington for publication in *Monthly Notices of The Royal Astronomical Society.*

It was shown in that paper that mathematically the problem is reducible to the problem of finding the values of a function of three coordinates in the velocity space when the values of the integrals of this function over any plane in that space are given as a function of three parameters defining the plane. The problem is soluble in a finite form and the very first trials have shown the applicability of this method to the existing data on radial velocities. I think that now, when we have much richer catalogues of radial velocities, it is worth while to try again to apply the solution.

Quite recently the inverse problem approach has found wide application to the statistics of flare stars in open clusters and associations. Let us consider here one of the simplest problems related to these flare stars. There are serious reasons to believe that the sequence of flares of any flare star is of the type of the Poisson stationary process with some mean frequency of occurrence ν. Then it is possible to show that between the mathematical expectations n_k of the numbers of stars that have flared k times during the total duration τ of observations, we have the relation

$$n_0 = \frac{n_1^2}{2n_2}. \tag{3}$$

According to the definition, n_0 is the expectation of the number of flare stars that have not flared during the whole time of observation. In other words, it is the number of flare stars that are not yet discovered. Therefore, adding this n_0 to the sum $n_1 + n_2 + \cdots$ of all stars observed in flares (this means to the number of *known* flare stars) we can obtain the total number N of flare stars in the given stellar aggregate. Of course, in practice instead of mathematical expectations of n_1 and n_2 we use the observed numbers of stars that have flared once and twice respectively and consider the resulting value of n_0 as approximate. For the validity of (3) it is necessary to assume that all flare stars have the same mean frequency of flares. We have strong indications that this assumption is definitely wrong. It is easy to show, however, that in this case we have instead of (3) the inequality

$$n_0 \geq \frac{n_1^2}{2n_2} \tag{4}$$

which gives the possibility of estimating the lower boundary of the total number of flare stars. In this way the result was obtained that the total number of flare stars in Pleiades must be larger than one thousand. Initially this estimate was considered excessive, since the total mass of Pleiades from the virial theorem is only about 400 solar masses. But now there is no doubt about such a high number of flare stars in Pleiades.

Later, the much more complicated and subtle problem of determination of the distribution function $f(v)$ of mean frequencies of stellar flares among the stars of the stellar aggregate under consideration was treated.

It is interesting that in this case we come to an inverse problem where the distribution of the first observed flares of different stars during the whole period of monitoring of the aggregate plays the role of "known function." Since the "first flare" is at the same time the moment of the discovery of a flare star this means that the knowledge of distribution of discoveries is essential for finding $f(v)$. Thus *the chronology of discoveries* of flare stars contains important information about $f(v)$. It was shown also that the distribution of "second flares" expressed as a function of the elapsed monitoring time contains again very important information about $f(v)$. But the moment of the second flare means the moment of the confirmation of discovery. Therefore, the *chronology of confirmations* is of importance for the problem under consideration.

The significance of inverse problems for astrophysics was discussed in detail by us in a special paper presented recently at the International Symposium on the Fundamental Problems of Theoretical and Mathematical Physics in Dubna (August, 1979) which will appear in the proceedings of that symposium. There we have given other examples.

3. *The Empirical Approach to the Evolutionary Processes in the Universe*

From the very beginning of my work in astrophysics I have been interested in the problems of the origin and evolution of stars and galaxies. It was clear to me that the old approach by means of global cosmogonic hypotheses or speculative models could hardly bring serious results. It was clear that one must proceed from empirical data.

The evolutionary processes in the Universe are of exceedingly complicated and diverse nature. Therefore, there is no chance of understanding them using a small number of speculative models or hypotheses. Instead of making more or less arbitrary assumptions, we must analyze patiently the empirical data so far obtained and try to deduce from them all possible conclusions on different links of many evolutionary chains existing in reality. It was necessary to find some general idea about the ways of doing this. In the mid-1930s I decided to apply this approach in my work on these problems.

The idea was to find the cases where it is relatively easy to deduce from the present state of the astronomical body or the system the direction of changes in the state of the body (or system), in other words to find the cases where we can conclude from simple considerations the evolutionary trend at the given phase without the knowledge of all other phases. Thus, roughly speaking, we must try to gather the information on the first derivative of the state we observe. Such an approach can give us in many cases the possibility of connecting the different observed states of some objects into evolutionary chains without artificial assumptions. In some cases, such chains (or rather pieces of chains) may be themselves very short but persistent work will in due time bring success in solving more and more complicated problems. Of course, I don't consider this approach as my invention. To me it was important that I decided to follow this approach as strictly as possible.

Thus, when studying the problems of *planetary nebulae*, I found that they are not in an equilibrium state. But the decisive significance of the observed structure of the emission lines of these objects for the understanding of the direction of evolution of planetaries was formulated by Zanstra. He concluded that the only explanation of the unusual appearance of those lines is the expansion of nebulae. Thus, it soon became clear that the planetaries are the results of ejection of the outer layer of their central stars.

When I analyzed the effect on interactions of members of a *stellar cluster* from close mutual passages during their motion, the conclusion was inevitable that the clusters are subject to the process of evaporation. In the case of open clusters, this process must be relatively rapid, having the time scale of the order of 10^8–10^9 years. This is a short time compared to the time scale of the Galaxy.

Thus, it was shown that the open clusters that now exist in the Galaxy are relatively young and rapidly changing systems and that the general stellar field of the Galaxy is steadily growing in the number of stars at the cost of disintegration of clusters. At the same time, the formation of clusters from individual field stars is practically impossible.

After World War II, I found that the much more extended groups of stars and of diffuse nebulae that have received the name of *stellar associations* are much younger than the ordinary open clusters. They contain often hot giants (O and B stars) and always a large percentage of variable dwarfs (T-Tauri variables and flare stars). The age of many associations is between 10^6 and 10^7 years. Their very existence is the proof of two fundamental facts concerning the birth of stars in the Galaxy: 1. the formation of stars is a process continuing through the present epoch of the evolution of our Galaxy, and 2. the formation of stars proceeds in relatively large groups (associations and clusters).

The subsequent discovery of the fact that the stellar associations contain the multiple stars of a special type—the so-called Trapezium-type systems—has shown that, in the associations, subgroups that are younger than the associations as a whole (the age between 10^5 and 10^6 years) exist.

On the other hand, already in the 1930s I tried to study the statistics of the elements of orbits of *double stars* in the Galaxy to obtain some indications about the direction of their dynamical evolution. The final conclusion was that the wide pairs are rapidly disintegrating. Therefore, the very existence of some very wide pairs puts an upper limit on the age of the Galaxy at least in its present state. This limit is quite independent of any cosmological consideration and is of the order of 10^{10} years.

For achieving further progress in the understanding of evolutionary processes it was necessary not only to avoid the use of speculative models but also to rid ourselves of some *superstitions*, which at the first glance appear like quite natural assumptions, remaining from classical cosmogonies. First is the idea that in the first phase of any process of formation of astronomical bodies or systems their state is always the state of nebular matter. Even now this opinion prevails among many theoreticians. However, it is difficult to find direct evidence for such an assumption in observational data.

The second superstition closely connected with the first is the complete disregard of the problem of the origin of nebulae. However, considering in some detail the situation in our Galaxy as well as in the external galaxies, one can see that all kinds of nebulae (and not only planetary or cometary nebulae) are in the state of rapid change. Their lifetime must be orders of magnitude shorter than the lifetime of the majority of stars or planets. It is, therefore, quite natural to try to begin the analysis of evolutionary processes by the study of changes in nebulae. In the case of planetaries one can see almost directly their origin in the ejection of outer layers of a star

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and their final destiny in complete dissipation in the surrounding space. The radio-nebulae, of which the best example is the Crab nebula, are the result of supernovae explosions and dissipate in a similar manner. There are many evidences of expansion of some massive diffuse nebulae. The same is true for the so-called *compact H II regions.* Therefore, the fact is that almost everywhere we observe directly or indirectly the formation of nebulae by way of ejections from the stars and their groups. But the evidences in favor of the opposite processes (collapses of nebulae, accretion of nebular material) are infrequent and at times very dubious. Of course, it cannot be excluded that much more definite evidences of such type can be found in the future, but the present-day picture of the Universe is dominated by processes of explosions, ejections from massive bodies, and subsequent formation of such short-living objects as nebulae. It seems that if the problem of the origin and evolution of nebulae had been formulated earlier, the solution of many more general problems connected with evolutionary processes in the Universe could have been reached much more rapidly.

One of the most intriguing questions about stellar associations is that some of them are *expanding* or contain expanding groups of stars. In our first papers on stellar associations (1947–1951) the prediction was made that the expansion is a general phenomenon among associations. From the observed proper motions in the association Perseus II, Professor A. Blaauw concluded that it is indeed in the state of expansion. Later he found the expansion phenomenon in a part of the Scorpio-Centaurus association. At the same time, in many other associations no appreciable expansion has been found. Of course, these negative conclusions are definitive only for a number of nearby associations. Therefore, there are only one or two cases where we certainly have no simple expansion phenomenon. At the same time, the existence of at least some expanding groups is the evidence of some kind of explosion processes connected with the birth or with the early stage of evolution of young stellar groups. Here again, the empirical data are against the theories of condensation of diffuse matter into the stars.

In the years 1955–1965 I turned my attention to the phenomena in and around the nuclei of galaxies. In the past, astronomers and particularly theoreticians showed little interest in the properties of the nuclei of the galaxies. In a report delivered to the Solvay Conference of 1958 I showed that these nuclei are often centers of large scale *activity* which proceeds in different forms. It was shown that the radio galaxies are not the products of collisions of galaxies, as it was accepted at that time, but are the systems in which ejections of tremendous scale from the nucleus have taken place. As a consequence of such ejections, clouds of high energy particles are being formed.

The subsequent discovery of *quasars* added one more form of nuclear activity by which a considerable part of liberated energy is emitted as the nonstellar optical radiation of the nucleus. In such cases, the luminosity of the nucleus often exceeds 10^{11} or 10^{12} times (sometimes even more) the luminosity of our Sun.

In another important development, the astronomers B. Markarjan, E. Khatchikjan, and others who work with me at the Byurakan Observatory initiated a more systematic observational study of the optical manifestations of activity in galaxies such as the ultraviolet excess and strong emission lines. The tenfold increase of the number of known Seyfert galaxies, which was one of the results of this work, has opened new possibilities of understanding active nuclear processes.

At the symposia organised in 1966 at Byurakan Observatory and in 1970 at the Vatican Academy of Sciences, the different forms of the activity of nuclei including the phenomena in QSO's and in Seyfert galaxies were thoroughly discussed. Since then, a huge volume of observational work has been carried out at different observatories in the world in order to reach better understanding of the processes involved. However, the theoretical interpretation has made little progress as yet.

While the observed forms of the activity of nuclei speak directly in favor of the fundamental nature of explosion and expansion processes taking place in central parts of galaxies, many theoreticians are still constructing models of nuclear phenomena in which the ejection processes are preceded by some form of collapse of great amounts of diffuse matter. According to such models, the ejections are only the secondary consequences of more fundamental processes of collapse. It is hardly necessary to say that I am very skeptical about such a speculative mode of thinking. There is no evidence even for the possibility of such a course of events. It seems that such an approach is the remnant of the old notion that the evolutionary processes in the Universe are always going in the direction of contraction and condensation.

4. *Concluding Remarks*

In conclusion I would like to give my evaluation of the degree of success in each of three directions of study discussed above.

1. I am very much satisfied that during the thirty-seven years that elapsed after the publication of my first paper on the application of the invariance principle, many new important results have been achieved. The subject has been extensively developed in the brilliant work of Professor V. Sobolev and his group. Professor R. Bellman has introduced "invariant imbedding." Professor S. Chandrasekhar's participation in the development of the field has greatly inspired me as well as the young people who worked directly with me. I look forward to an even wider application of the

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invariance principle to many problems of mathematical physics and to other branches of exact science.

2. It is evident that the success in the field of inverse problems of astrophysics is very modest. It happened that from my youth I was more enthusiastic with this direction of thinking than with the two others. This shows that in all cases success depends not so much on the personal wishes or abilities of the investigator but in large degree on the general state of affairs in the scientific field under consideration and, of course, on the difficulty of the task itself. But I have no doubt that this direction is a very promising one for astrophysics. I take this opportunity to express my conviction that this direction will have in the future great significance for cosmology.

3. The observational approach to the problems of evolution in astrophysics, which remains the essence of the third direction of my studies, has in the last decades rapidly spread to the whole of astrophysics. It has penetrated deeply every domain of our science. Perhaps it is not an exaggeration to say that, partly owing to this approach and owing to the persistent work of a whole generation of astrophysicists, the science of astrophysics is now transformed into an evolutionary discipline.

Now everybody will agree that the problem of the origin and evolution of celestial bodies cannot be solved by one or by a small number of speculative models. The scope of the field will increase and widen with new discoveries in astrophysics.

My skeptical attitude to the existing formal theoretical models proposed so far is confirmed by the fact that almost all the new interesting discoveries, which were extremely abundant during the last three decades, proved to be great surprises for such models. The attempts to accommodate these models to new observational discoveries usually does not help very much. Let us consider two cases of the complete failure of the speculative approach.

(*a*) Many theoreticians are convinced that they have now a more or less comprehensive theory of stellar evolution. Hundreds of models were calculated, particularly for the early phases of evolution. However, the theory has completely failed to predict such an important phenomenon as flare stars. There is no doubt now that the majority of stars after the period of their formation (T-Tauri stage) go through this phase of evolution. Therefore, one of the first tasks of every evolutionary theory must be the explanation of pecularities of the flare processes. However, the majority of models are even now neglecting this requirement.

(*b*) The situation is even worse with the *problem of fuors* (this term is used in USSR for the stars of FU Orionis type). The fact that the stage of a fuor plays an important role in the life of at least some categories of stars is rather

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fatal for many speculative theories. But the state of affairs is more serious than it seems from first glance. It appears now that there is a whole sequence of different stars, which in their photometric behavior are more or less similar to fuors. The P Cygni star, which brightened almost four centuries ago, is an example. It is well known that in every spiral or irregular galaxy we have many supergiants of P Cygni type. Therefore, processes of the brightening of pre-P Cygni type stars are very important for the understanding of the evolution of supergiants.

There is no doubt that observational studies of such stars and processes will bring interesting empirical material for the picture of the stellar evolution.

In order to be understood correctly I would like to add some words to my criticism of speculative thinking and of models. Of course, nobody can deny the part which the model approach, as well as the speculative thinking, plays in science. The first of the directions to which I devoted my work (the invariance principle) is much nearer to the construction of models than to empirical studies. My point is, however, that in the concrete cases discussed above the building of models without taking properly into account the vast empirical material now in our possession can hardly give good results.

Nature keeps still many of its secrets. Our aim is to disclose them. It is natural to try this by *observing* the places where they are hidden. We can hardly reach our aim by only theorizing.

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