Associative delusions and problem of their overcoming

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Abstract

Influence of associative delusions (AD) onto development of physics and mathematics is investigated. The associative delusion (AD) means a mistake, appearing from incorrect associations, when a property of one object is attributed to another one. Examples of most ancient delusions are: (1) connection of the gravitation field direction with a preferred direction in space (instead of the direction to the Earth centre), that had lead to the antipode paradox, (2) statement that the Earth (not the Sun) is the centre of the planetary system, that had lead to the Ptolemaic doctrine. Now these AD have been overcame. In the paper one considers four modern and not yet got over ADs, whose corollaries are false space-time geometry in the micro world and most of problems and difficulties of the quantum field theory (QFT). One shows that ADs have a series of interesting properties: (1) ADs appear to be long-living delusions, because they are compensated partly at the sacrifice of appearance of compensating (ptolemaic) conceptions, (2) ADs influence on scientific investigations, generating a special pragmatic style (P-style) of investigations resembling experimental trial and error method , (3) acting on investigations directly and via P-style, ADs direct the science development into a blind alley. One considers concrete properties of modern ADs and the methods of their overcoming. From viewpoint of application the paper is an analysis of mistakes, made in the quantum theory development. One analyses reasons of these mistakes and suggests methods of their correction.

1 Introduction

The present paper is devoted to study of associative delusions, their role in the natural science development and to problems of their overcoming. The associative delusion means such a situation, when associative properties of human thinking actuate incorrectly, and the natural phenomenon is attributed by properties alien to it. Usually one physical phenomenon is attributed by properties of other physical phenomenon, or properties of the physical phenomenon description are attributed to the physical phenomenon in itself. Let us illustrate this in a simple example, which is perceived now as a grotesque.

It is known that ancient Egyptians believed that all rivers flow towards the North. This delusion seems now to be nonsense. But many years ago it had weighty foundation. The ancient Egyptians lived on a vast flat plane and knew only one river the Nile, which flowed exactly towards the North and had no tributaries on the Egyptian territory. The North direction was a preferred direction for ancient Egyptians who observed motion of heavenly bodies regularly. It was direction toward the fixed North star. They did not connect direction of the river flow with the plane slope, as we do now. They connected the direction of the river flow with the preferred spatial direction towards the North. We are interested now what kind of mistake made ancient Egyptians, believing that all rivers flow towards the North, and how could they to overcome their delusion.

Their delusion was not a logical mistake, because the logic has no relation to this mistake. The delusion was connected with associative property of human thinking, when the property P is attributed to the object O on the basis that in all known cases the property P accompanies the object O. Such an association may be correct or not. If it is erroneous, as in the given case, it is very difficult to discover the mistake. At any rate it is difficult to discover the mistake by means of logic, because such associations appear before the logical analysis, and the subsequent logical analysis is carried out on the basis of the existing associations. Let us imagine that in the course of a travel an ancient Egyptian scientist arrived the Tigris, which is the nearest to Egypt river. He discovers a water stream which flows, first, not directly and, second, not towards the North. Does he discover his delusion? Most likely not. At any rate not at once. He starts to think that the water stream, flowing before him, is not a river. A ground for such a conclusion is his initial belief that "real" river is to flow, first, directly and, second, towards the North. Besides, the Nile was very important in the life of ancient Egyptians, and they were often apt to idolize the Nile. The delusion about direction of the river flow can be overcame only after that, when one has discovered sufficiently many different rivers, flowing towards different directions, and the proper analysis of this circumstance has been carried out.

Thus, to overcome the associating delusion, it is not sufficient to present another object O, which has not the property P, because one may doubt of whether the presented object is to be classified really as the object O. Another attendant circumstances are also possible.

If the established association between the object and its property is erroneous, one can say on associative delusion or on associative prejudice. The usual method of the associative delusions overcoming is a consideration of a wider set of phenomena, where the established association between the property P and the object O may appear to be violated, and the associative delusion is discovered.

In this paper the associative delusions in natural sciences, mainly in physics are discussed. The associative delusions (AD) are very stable. If they have been established, they are overcame very difficultly, because they cannot be disproved logically. But there is an additional complication. The usual mistake is overcame easily by the scientific community, as soon as it has been overcame by one of its members. The corresponding article is published, and the scientific community takes it into consideration, and the mistake is considered to be corrected.

A different situation arises with the associative delusions (AD). Discovery of the associative delusion (AD), and publication of corresponding article do not lead to acknowledgment of AD as a delusion or mistake. The scientific community continue to insist on the statement, that the considered in the article AD is not a mistake in reality, and that the author of this paper himself makes a mistake. A long controversy arises. Sometimes it leads to a conflict, as in the case of conflict between the Ptolemaic doctrine and that of Copernicus. Finally, the truth celebrates victory, but the way to this victory appears to be long and difficult.

Apparently, the reason of the AD stability lies in obviousness and habitualness of these statements, which appear to be associative delusions afterwards. On the ground of these statements one constructs scientific conceptions, which agree with experimental data and observations. Declaring these habitual statements to be a delusions, one destroys existing scientific conceptions and tries to construct new conceptions. It is always very difficult for the scientific community.

In the science history a series of associative delusions is known. Let us list them in the chronological order.

AD.1. The antipodes paradox, generated by that the gravitational field direction is connected with a preferred direction in the space, but not with the direction towards the Earth centre.

AD.2. The Ptolemaic doctrine in the celestial mechanics, where the property of being the "universe" centre was attributed to the Earth, whereas the Sun is such a centre.

AD.3. Prejudices against the Riemannian geometry in the second half of the XIX century are connected with that the Cartesian coordinate system was considered to be an attribute of any geometry, whereas it was only a method of the Euclidean geometry description.

AD.4. Impossibility of employment of the pure metrical conception of geometry, connected with the associative delusion, that the concept of the curve is considered to be a fundamental concept of any geometry, whereas the curve is only a geometrical object, used in the Euclidean and Riemannian geometry.

AD.5. Impossibility of construction of dynamical conception of statistical description (DCSD), connected with the associative delusion, that any statistical description is considered to be produced in terms of the probability theory, and the probability concept is a fundamental concept of any statistical description.

AD.6. Identification of individual particle $\mathcal{S}$ with the statistically averaged particle $\langle\mathcal{S}\rangle$, used at the conventional interpretation of quantum mechanics. Such an identification is a kind of associative delusion, when the individual particle properties $\mathcal{S}$ are attributed to the statistically averaged particle $\langle\mathcal{S}\rangle$ and vice versa. The Schrödinger cat paradox and some other quantum mechanics paradoxes, connected with the wave function reduction, are corollaries of this identification.

AD.7. The forced identification of energy and Hamiltonian, used in relativistic quantum field theory (QFT), is also an associative delusion. As any associative delusion this identification is connected with attributing properties of one object to another one. In the given case the coincidence of energy and Hamiltonian for a free nonrelativistic particle is considered to be a fundamental property of any particle without sufficient foundations. We shall denote this delusion symbolically by means of $E = H$, where E is the energy, and H is the Hamiltonian.

The first three of the seven listed delusions (AD.1 – AD.3) had been overcame to the beginning of XX century, though a detailed analysis of these overcoming is, maybe, absent in the literature. As to AD.4 – AD.7, the scientific community is yet destined to overcome them. Besides these ADs exist simultaneously, and the order of their listing corresponds basically to their importance rather than to chronology.

The purely metric conception of geometry (CG), where all information on geometry is given by means a distance between to space points, is the most general conception of geometry (CG). It generates the most complete list of geometries, suitable for the space-time description. AD.4 discriminates the purely metric CG. As a result instead of it one uses Riemannian CG, generating incomplete list of possible geometries. The true space-time geometry is absent in this list, and we are doomed to use the Minkowski geometry for the space-time description. The Minkowski geometry is incorrect geometry for small space-time scales, i.e. in the micro world. In the true space-time geometry the micro particle motion is initially stochastic, and the properties of the geometry are a reason of this stochasticity. In the Minkowski geometry the motion of any particle is deterministic, and incorrectness of the Minkowski geometry lies in this fact.

AD.5 leads to impossibility of a construction of a consecutive statistical description of the stochastically moving micro particles (electrons, positrons, etc.) , although it is doubtless that quantum mechanics, describing the regular component of this motion, is a statistical theory. AD.4 and AD.5 establish such a situation, when one is forced to use a series of additional hypotheses (quantum mechanics principles) for a correct description of observed quantum phenomena. It is much as the Ptolemeus used a series of additional construction (epicycles, differents) for explanation of observed motion of heavenly bodies. They were needed for compensation of AD.2.

Overcoming of AD.4 and AD.5 admits one to eliminate the quantum mechanics principles and to construct the quantum phenomena theory as a consecutive statistical description of stochastic micro particle motion. At such a description the micro particle stochasticity has a geometric origin, i.e. it is generated by the space-time geometry. The consecutive statistical description of the micro particles appears as a result of a construction of the dynamical conception of statistical description (DCSD), that becomes to be possible after overcoming of AD.5. The elimination of the quantum mechanics principles after overcoming of AD.4 and AD.5 resembles the eliminating of Ptolemaic epicycles, when they stopped to be necessary after overcoming AD.2 and the subsequent transition from the Ptolemaic doctrine to that of Copernicus.

AD.6 has not such a global character as AD.4 and AD.5. It concerns mainly the interpretation of the measurement concept in quantum mechanics.

AD.7 has not the global character also. It acts only in the scope of the relativistic quantum field theory (QFT). QFT in itself is only a section of the Ptolemaic conception, i.e. a conception, which uses additional hypotheses (quantum mechanics principles), compensating incorrect choice of the space-time model. AD.7 (identification of energy and Hamiltonian $E = H$) generates a series of difficulties in QFT (non-stationary vacuum, necessity of the perturbation theory and some other). In fact there is no necessity the energy – Hamiltonian identification $E = H$. The secondary quantization can be carried out without imposing this constraint [9]. The condition $E = H$ appears to be inconsistent with dynamic equations. Imposition of this constraint makes QFT to be inconsistent. On one hand, such an inconsistency leads to above mentioned difficulties, but on the other hand, such an inconsistency admits one to explain the pair production effect, because any inconsistent theory admits one to explain all what you want. One needs only to show sufficient ingenuity. On one hand, elimination of the constraint $E = H$ leads to a theory which is consequent in the scope of quantum theory and free from the above mentioned difficulties, but on the other hand, it leads to that the theory stops to describe the pair production effect. This deplorable fact means only, that the undertaken attempt of the FTP construction on the basis of unification of the relativity principles with those of quantum mechanics failed, and one should search for alternative conception.

Let us take into account that the quantum mechanics is a compensating (Ptolemaic) conception, i.e. just as the quantum mechanics principles have been invented for compensation of AD.4 and AD.5, as the Ptolemaic epicycles have been invented for compensation of AD.2. Then an attempt of unification of quantum mechanics principles with the relativity ones is as useless, as an attempt of introduction of Ptolemaic epicycles in Newtonian mechanics.

Apparently, the conception, appeared after overcoming of AD.4 and AD.5, is a reasonable alternative to QFT. Such a conception is consistently relativistic and quantum (in the sense that it contains the quantum constant $\hbar$, contained explicitly in the space-time metric). It does not contain the quantum mechanics principles, and one does not need to unite them with the relativity principles. We shall refer to this conception as the model conception of quantum phenomena, distinguishing it from conventional quantum mechanics, which will be referred to as axiomatic conception of quantum phenomena. The difference between axiomatic and model conception is much as the difference between the thermodynamics and the statistical physics. The thermodynamics may be qualified as the axiomatic conception of thermal phenomena, whereas the statistical physics may be qualified as the model conception of thermal phenomena. The transition from the axiomatic conception to the model one was carried out after a construction of the теплород model (chaotic motion of molecules) the thermodynamics axioms, describing properties of the fundamental thermodynamical object – теплород, stopped to be necessary. Concept of теплород lies not used usually in the statistical physics, but if it is introduced, its properties are determined from its model (chaotic molecular motion).

Similar situation takes place in the interrelations between the axiomatic and model conception of quantum phenomena. In the axiomatic conception there is an fundamental object, called wave function. Its properties are determined by the quantum mechanics principles. The wave function is that which differs the quantum mechanics from the classical one, where the wave function absent. In the model conception one constructs a "model of the wave function" [4]. Thereafter the wave function properties are obtained from this model, one does not need the quantum mechanics principles. Axiomatic and model conceptions lead to the saame result in the nonrelativistic case, but in the relativistic case the results are different, in general. For instance, application of the model conception to investigation of the dynamic system $\mathcal{S}_{\mathrm{D}}$, described by the Dirac equation, leads to another result [3], than investigation, produced by conventional methods in the scope of the axiomatic conception. In the first case the classical analog of the Dirac particle $\mathcal{S}_{\mathrm{D}}$ is a relativistic rotator, consisting of two charged particles, rotating around their common center of mass. In the second case the classical analog is a pointlike particle, having spin and magnetic moment.

An existence of the associative delusion does not permit one to construct a rigorous scientific conception. The constructed building appears to be a compensating (Ptolemaic) conception, where incorrect statement is compensated by means of additional suppositions. In general, the Ptolemaic conception is not true. But there is such fields of its application, where its employment leads to correct results, which agree with observations and experimental data. For instance, in the scope of the Ptolemaic doctrine one can choose such epicycles and differents for any planet, that one can calculate its motion in a sufficient long time so, that predictions agree with observations. But there is a class of the celestial mechanics problems, which could not be solved in the scope of the Ptolemaic doctrine. For instance, in the scope of this doctrine one cannot solve such a problem: when and with what velocity should one throw a stone from the Earth's surface, in order that it could drop on the Moon. In the scope of the Ptolemaic doctrine one cannot discover the gravitation law and construct the Newtonian mechanics. The associative delusion, embedded in the ground of the Ptolemaic doctrine and disguised by means of compensating hypotheses, hindered the progress of celestial mechanics. As far as in that time the celestial mechanics was the only exact natural science, AD hindered the normal development of natural sciences at all. The development of natural sciences went to a blind alley. After overcoming of AD.2 the natural sciences development was accelerated strongly.

The same situation takes place with the quantum mechanics. Although at the first acquaintance the quantum mechanics seems to be an disordered collection of rules for calculation of mathematical expectations, nevertheless, in the nonrelativistic case an employment of these rules leads to results which agree with experiments. Taking the quantum mechanical principles, the quantum mechanics as a whole is a consistent conception, which describes excellently a wide class of physical phenomena. But at the transition to the field of relativistic phenomena (pair production, elementary particles theory) the quantum principles stop to be sufficient. One is forced to introduce new suppositions. The further the quantum theory advances in the field of relativistic phenomena, the more new suppositions are to be introduced for descriptions of observed phenomena. This is an indirect indication, that the conventional way of the quantum theory development comes to a blind alley.

Investigation of possible methods of the associative delusions overcoming is a subject of this paper. On one hand, overcoming of any special associative delusion needs a knowledge of the subject of investigation and a professional approach to the investigation of the phenomenon. On the other hand, the Ptolemaic conceptions have some common properties, and a work with them has some specific character, which should be known, if we want to overcome corresponding ADs effectively.

First, it is very difficult to discover the associative delusion. Indirect indications of AD are an increasing complexity of the theory and a necessity of new additional suppositions. These indications show that the associative delusion does exist, but they do not permit one to determine, what is this AD.

Second, work with Ptolemaic conceptions, i.e. with conceptions, containing AD, generates a special pragmatic style (P-style) of investigations. the P-style lies in the fact that one sorts out all possible ways of explanation and calculation of the considered phenomenon. Of course, different versions, considered at such an approach, are restricted by the existing mathematical technique and by the possibilities of the researcher's imagination. But these restrictions are essentially less, than the restrictions imposed by the classical style (C-style) of investigations. The classical style (C-style) is the style of investigations, fully developed in the natural sciences to the end of the XIX century.

Unprejudiced reader will agree that the delusions AD.1 – AD.3, having been overcame, are delusions indeed, and that it was worth to overcame them. But it is rather doubtless that he agrees at once that AD.4 – AD.7 are also delusions and that they are to be overcame. If it were so, then AD.4 – AD.7 have been overcame many years ago. Of course, ADs are undesirable as any other delusions. One should eliminate them, if it is possible. But one should not consider them as misunderstandings, or manifestations of researcher's stupidity. ADs are inevitable attributes of the cognitive processes, as far as they are conditioned by the restriction of the field of investigated phenomena at the initial stage of investigations. ADs were in the past, they exist now, and apparently, they will exist in the future. We should know, how to live with them and to make investigation. The situation resembles the situation with a noise. We transmit information at presence of a noise, and we know that the noise is undesirable, that the noise should be removed, and that, unfortunately, it cannot be removed completely.

One should study associative delusions, their properties and the influence on the style of thinking and investigations of researchers, which are forced to work under conditions of the associative delusions presence. Investigation of ADs properties and possibilities of their overcoming is a goal of this paper. We begin with detailed investigations of AD.4 – AD.7, to make sure that they are delusions indeed and to understand how to overcome them. It is very important, because experience of overcoming of AD.2 (Ptolemaic doctrine) shows that the overcoming process is very difficult for scientific community.

Usually these difficulties are connected with a negative role of the Catholic church. B.V. Raushenbach [5] considers that the position of the Catholic church is not the case. It was incompetent in problems of celestial mechanics. It agreed simply with opinion of most of that time researchers. Most of scientists of that time were priests, and B.V. Raushenbach considers that they used the Catholic church simply as a tool for a fight against proponents of the Copernicus doctrine. Experience of the author in attempts of overcoming of AD.4 – AD.7 shows, that this is B.V. Raushenbach, who is right.

In sections 2 – 5 one considers properties of AD.4 – AD.7. In the sixth section influence of associative delusions on the style of investigations is considered. In sections from seventh to twelfth the history of overcoming of AD.4 – AD.7 is presented in the form, as the author of this paper saw it.

2 Conception of geometry and a correct choice of the space-time geometry

The conception of geometry (CG) is considered to be the method (a set of rules), by means of which the geometry is constructed. The proper Euclidean[1] geometry can be constructed on the basis of different geometric conceptions.

For instance, one can use the Euclidean axiomatic conception (Euclidean axioms), or the Riemannian conception of geometry (dimension, manifold, metric tensor, curve). One can use the topology-metric conception of geometry (topological space, metric, curve). In any case one obtains the same proper Euclidean geometry. From point of view of this geometry it is of no importance which of possible geometric conceptions is used for the geometry construction.

But if we are going to choose a geometry for the real space-time, it is very important, that the list of all possible geometries, suitable for the space-time description, would be complete. If the true space-time geometry is absent in this list, we are doomed to a choice of a false geometry independently of the method which is used for a choice of the space-time geometry. Thus, a determination of the complete list

[1]We use the term "Euclidean geometry" as a collective concept with respect to terms "proper Euclidean geometry" and "pseudoeuclidean geometry". In the first case the eigenvalues of the metric tensor matrix have similar signs, in the second case they have different signs.

of all possible geometries is a necessary condition of a correct choice of the space-time geometry. In turn the determination of the possible geometries list depends on the conception of geometry (CG), which is used for determination of the list of possible geometries. Any of possible CG contains information of two sorts: (1) non-numerical information in the form of concepts, axioms and propositions, formulated verbally, (2) numerical information in the form of numbers and numerical functions of space points. In different CG this information is presented differently.

title of CG	*non-numerical information*	*numerical information*
Euclidean CG	Euclidean axioms	$\emptyset$
Riemannian CG	Manifold, curve coordinate system	$n, \quad g_{ik}(x)$
topology-metric CG	topological space, curve	$\rho(P,Q) \geq 0,$ $\rho(P,Q) = 0, \quad \text{iff} \quad P = Q$ $\rho(P,Q) + \rho(Q,R) \geq \rho(P,R)$
purely metric CG	$\emptyset$	$\sigma(P,Q) = \frac{1}{2}\rho^2(P,Q) \in \mathbb{R}$

Varying the numerical information at fixed non-numerical one, we obtain different geometries in the scope of the same conception of geometry. Varying continuously numbers and functions, constituting numerical information of CG, one obtains a continuous set of geometries, each of them differs slightly from the narrow one. Any admissible value of numerical information is attributed some geometry in the scope of the given CG. One can also change non-numerical information, replacing one axiom by another. But at such a replacement the geometry change step-wise, and one should follow that replacements of one axiom by another does not lead to inconsistencies. It is complicated and inconvenient. It is easier to obtain new geometries in the scope of the same conception, changing only numerical information.

One can see from this table, that different CG have different capacity of numerical information and generate classes of geometries of different power. The Euclidean CG does not contain numerical information at all. Vice versa, the purely metric CG contains only numerical information and generates the most powerful class of geometries which will be referred to as tubular geometries (or briefly T-geometries).

The T-geometry has many attractive features. Firstly, it is very simple and realizes the simple attractive idea, that for determination of a geometry on a set Ω of points P it is sufficient to give the distance $\rho(P,Q)$ between all pairs $\{P,Q\}$ of points of the set Ω. In fact the distance $\rho(P,Q)$ is determined by means of the world function $\sigma = \frac{1}{2}\rho^2$ on the set $\Omega \times \Omega$. In spite of simplicity and attractiveness of this idea the existence possibility in itself of the purely metric CG was being problematic for a long time. K. Menger [6] and J.L. Blumenthal [7] tried to construct so called distance geometry, which was based on the concept of distance in a larger degree, than it is made in the topology-metric CG. But they failed to construct the purely metric CG. The reason of the failure was AD.4. The statement of necessary and sufficient conditions of the geometry Euclideness in terms of the world function σ, given on the set $\Omega \times \Omega$, was a crucial step in construction of the purely metric CG. The prove [8, 9, 10] of the fact, that the Euclidean geometry can be constructed in terms of only σ meant a possibility of construction of any T-geometry in terms of σ. It meant existence of the purely metric CG.

In the scope of purely metric CG all information on geometry is derived from the world function. In particular, if one can introduce a dimension of the space $\{\Omega, \sigma\}$, this information can be derived from the world function. From the world function one can derive information on continuity, or discontinuity of the space $\{\Omega, \sigma\}$. In the case of continuous geometry information on the coordinate systems and metric tensor can be also derived from the world function. In T-geometry there is an absolute parallelism (which is absent in Riemannian geometries). Besides the T-geometry has a new property – nondegeneracy.

The geometry is called a nondegenerate, if there are many vectors $\overrightarrow{P_0Q}$ of fixed length $|\overrightarrow{P_0Q}|$, parallel to the vector $\overrightarrow{P_0P_1}$. In the degenerate geometry there is only one such a vector $\overrightarrow{P_0Q}$.

Nondegeneracy of T-geometry may be conceived as follows. Any T-geometry can be obtained from the Euclidean geometry by means of its deformation (i.e. a change of distance $\rho(P, Q)$ between the space points). At such a deformation the geometrical objects of Euclidean geometry change their shape. If the obtained T-geometry is degenerate, the Euclidean straights transform to lines, which are curved lines, in general. But it is possible such a deformation, that the straight of n-dimensional Euclidean space converts into $(n-1)$-dimensional tube. For it would be a possible, the straight is to be defined as a set of points, possessing some property of the Euclidean straight. Definition of the straight as a curve, possessing some property of the Euclidean straight prohibits automatically deformation of the Euclidean straight into $(n-1)$-dimensional tube and discriminates nondegenerate geometries.

If one considers nondegenerate T-geometry of the space-time, the motion of free particles in such a space-time appears to be stochastic, although the geometry in itself (i.e. the world function σ) is deterministic. In other words, nondegeneracy of the space-time geometry generates indeterminism.

In the Riemannian CG the deformation, converting a line into a tube, is forbidden. It is connected with AD.4, according to which the curve is a fundamental object of geometry, and there are not to be such geometries, where the curve would be replaced by a surface. It is in this point, where AD.4 discriminate purely metric CG and T-geometries, generated by this CG. As a corollary the list of possible geometries reduces strongly. The true space-time geometry fall out of the list of possible geometries, one chooses a false model for the space-time.

In the present time one uses the Riemannian conception for obtaining the space-time geometry. In the simplest case, when one can neglect gravitation, the space time is uniform, isotropic and flat. In the scope of the Riemannian geometry there is only one flat uniform isotropic geometry. It is the Minkowski geometry, for which the world function has the form:

$$\sigma_{\mathrm{M}}(x,x') = \sigma_{\mathrm{M}}(t,\mathbf{x},t',\mathbf{x}') = \frac{1}{2}\left(c^2(t-t')^2 - (\mathbf{x}-\mathbf{x}')^2\right) \tag{2.1}$$

where c is the speed of the light, and $x = \{t,\mathbf{x}\}$ and $x' = \{t',\mathbf{x}'\}$ are coordinates of two arbitrary points in the space-time.

Thus, in the case of Riemannian CG the problem of choosing space-time geometry does not appear. It is determined uniquely.

The topology-metric CG cannot be applied to the space-time, because it supposes that $\sigma(P,Q) = \frac{1}{2}\rho^2(P,Q) \geq 0$, whereas in the space-time there are spacelike intervals, for which $\sigma(P,Q) < 0$.

The purely metric CG generates a whole class of flat uniform isotropic T-geometries, labelled by a function of one argument. In this case the world function has the form

$$\sigma(x,x') = \sigma_{\mathrm{M}}(x,x') + D(\sigma_{\mathrm{M}}(x,x')), \tag{2.2}$$

where σ_{M} is the world function for the Minkowski space (2.1), and the function D is an arbitrary function, labelling possible flat uniform isotropic geometries. These geometries differ one from another in the shape of tubes, obtained as a result of the Euclidean straight deformation. Hence, they differ in the stochasticity character of the free particles motion. For the purely metric CG the problem of choice of the space-time geometry is very important, because there are many uniform isotropic geometries. To set $D \equiv 0$ in (2.2) and choose the Minkowski geometry would be incorrect, because in the Minkowski geometry the motion of particles is deterministic. But it is well known that the motion of real micro particles (electrons, positrons, etc.) is stochastic. In other words, experiments with single particles are irreproducible. Only distributions of results, i.e. results of mass experiments with many similarly prepared particles are reproducible. These distributions of results are described by quantum mechanics, constructed on the basis of some additional hypotheses, known as principles of quantum mechanics.

When there are such space-time geometries, where the motion of particles is primordially stochastic, one cannot consider as reasonable such an approach, where at first one chooses the Minkowski geometry with deterministic motion of particles, and thereafter one introduces additional suppositions (quantum mechanics principles), providing a description of the stochastic motion of micro particles. It would be more correct choose the space-time geometry in such a way, that a statistical description of stochastic motion of micro particles describe correctly experimental data. As far as the quantum mechanics describes all nonrelativistic experiments very well, it is sufficient to choose the space-time so, that the statistical description of stochastic motion of micro particles would agree with predictions of quantum mechanics.

At first sight, it seems that the quantum effects cannot be explained by peculiarities of geometry, because intensity of quantum effects depends on the particle mass essentially, and the mass is such a characteristic of a particle, which is not connected with geometry. It seems that influence of a geometry on the particle motion is to be similar for particles of any mass. In reality the influence of geometry does not depend on particle motion only in the nondegenerate geometry (Minkowski geometry). In the space-time with the nondegenerate geometry the particle mass, as well as its momentum are geometrical characteristics.

The world tube of the particle with the mass m is described by the broken world tube $\mathcal{T}_{\mathrm{br}}$, which is determined by a sequence of the break points $\{P_i\}$, $i = 0, \pm 1, \pm 2, \ldots$. The adjacent points P_i, P_{i+1} are connected between themselves by a segment $\mathcal{T}_{[P_i P_{i+1}]}$ of the straight. This segment is determined by the relation

$$\mathcal{T}_{[P_i P_{i+1}]} = \{R | S(P_i, R) + S(R, P_{i+1}) = S(P_i, P_{i+1})\} \tag{2.3}$$

where $S(P_i, P_{i+1}) = \sqrt{2\sigma(P_i, P_{i+1})}$ is the distance between the points P_i and P_{i+1}. The set of points $\{P_i\}$, $\quad i = 0, \pm 1, \pm 2, \ldots$ will be referred to as the skeleton of the tube $\mathcal{T}_{\mathrm{br}}$.

In the proper Euclidean geometry as well in the Minkowski geometry (for timelike interval $S^2(P_i, P_{i+1}) > 0$) the set of points (2.3) forms a segment of the straight line, connecting points P_i ¨P_{i+1}. In the nondegenerate geometry the set $\mathcal{T}_{[P_i P_{i+1}]}$ forms a three-dimensional cigar-shaped surface with the ends at the points P_i, P_{i+1}.

The vector $\overrightarrow{P_i P_{i+1}} = \{P_i, P_{i+1}\}$ is interpreted as the particle 4-momentum on the segment $\mathcal{T}_{[P_i P_{i+1}]}$ of the world tube $\mathcal{T}_{\mathrm{br}}$ of the particle

$$\mathcal{T}_{\mathrm{br}} = \bigcup_i \mathcal{T}_{[P_i P_{i+1}]}. \tag{2.4}$$

The length $\left|\overrightarrow{P_i P_{i+1}}\right| = S(P_i, P_{i+1}) = \sqrt{2\sigma(P_i, P_{i+1})}$ of the vector $\overrightarrow{P_i P_{i+1}}$ is the geometrical mass μ of the particle, expressed in units of length. The universal constant b connects the geometrical mass μ with the usual mass m of the particle.

$$m = b\mu = bS(P_i, P_{i+1}), \qquad i = 0, \pm 1, \pm 2, \ldots \qquad [b] = \mathrm{g/cm} \tag{2.5}$$

All segments $\mathcal{T}_{[P_i P_{i+1}]}$, $i = 0, \pm 1, \pm 2, \ldots$ has the same length $\mu = m/b$. Thus, in general, m is a geometrical characteristic of the particle, but in the case of the Minkowski geometry one cannot determine the particle mass, using the world line shape, because one cannot determine points P_i of the world line $\mathcal{T}_{\mathrm{br}}$ skeleton on the basis of the world line shape. In the case of nondegenerate space-time geometry the points P_i of the skeleton are end points of the cigar-shaped segments $\mathcal{T}_{[P_i P_{i+1}]}$. They can be determined via intakes of the broken tube (2.4). Interval $S(P_i, P_{i+1})$ between adjacent points P_i of the skeleton determines the geometrical mass μ of the particle.

For a free particle the 4-momenta $\overrightarrow{P_i P_{i+1}}$ and $\overrightarrow{P_{i+1} P_{i+2}}$ of two adjacent segments $\mathcal{T}_{[P_i P_{i+1}]}$ and $\mathcal{T}_{[P_{i+1} P_{i+2}]}$ are parallel $\overrightarrow{P_i P_{i+1}} \uparrow\uparrow \overrightarrow{P_{i+1} P_{i+2}}$. In the Minkowski geometry there is only one vector $\overrightarrow{P_{i+1} P_{i+2}}$ of the length μ, parallel to timelike vector $\overrightarrow{P_i P_{i+1}}$. Hence, if the vector $\overrightarrow{P_0 P_1}$ is fixed, all other vectors $\overrightarrow{P_i P_{i+1}}$ $\quad i = 1, 2, \ldots$ are determined uniquely. In other words, in the Minkowski geometry the total world line $\mathcal{T}_{\mathrm{br}}$ is determined uniquely, provided one of its segments is fixed. It means that the motion of a free particle in the space-time with Minkowski geometry is deterministic.

In the space-time with nondegenerate geometry there are many vectors $\overrightarrow{P_{i+1}P_{i+2}}$ of the length μ, parallel to the timelike vector $\overrightarrow{P_iP_{i+1}}$. It means that the end P_{i+2} of the vector $\overrightarrow{P_{i+1}P_{i+2}}$ is not determined uniquely, even if the vector $\overrightarrow{P_iP_{i+1}}$ is fixed. Other points P_{i+3}, P_{i+4},... are not determined uniquely also. It means that the broken tube $\mathcal{T}_{\text{br}}$ is stochastic. Thus, the motion of a free particle in the space-time with nondegenerate geometry is stochastic. The character and intensity of the stochasticity depend on the form of the function $D(\sigma_{\text{M}})$ in the relation (2.2).

Supposing that the statistical description of stochastic world tubes gives the same result, as the quantum-mechanical description in terms of the Schrödinger equation, one can calculate the distortion function $D(\sigma_{\text{M}})$. The calculation gives [11]

$$\begin{aligned} D &= D(\sigma_{\text{M}}) = \left\{ \begin{array}{lll} d & \text{if} & \sigma_{\text{M}} > \sigma_0 \\ 0 & \text{if} & \sigma_{\text{M}} \leq 0 \end{array} \right. \\ d &= \frac{\hbar}{2bc} = \text{const} \approx 10^{-21}\text{cm}, \qquad \sigma_0 = \text{const} \approx d \end{aligned} \tag{2.6}$$

Here $\hbar$ is the quantum constant, and $b \approx 10^{-17}$g/cm is a new universal constant. Inside the interval $(0, \sigma_0)$ values of the function $D(\sigma_{\text{M}})$ are not yet determined.

From the tree-dimensional viewpoint the micro particle is a pulsating sphere. Period T of pulsations depends on the particle mass m. It is determined by the relation $T = m/(bc)$, where b is the universal constant. The maximal sphere radius $R_{\max} \approx \sqrt{d}$ does not depend on the particle mass. Approximately one can assume that in the period T the sphere radius increases from zero up to maximal value $R_{\max}$, and then it reduces to zero. In the period T the sphere centre moves along the straight line uniformly. At the collapse moment a random jump-like change of velocity takes place. In the coordinate system, where the sphere is at rest the velocity jump is equal approximately to $R_{\max}/T \approx m^{-1}(\hbar bc/2)^{1/2}$. The lesser is the particle mass the larger is the velocity jump. Besides, the period T depends on the particle mass. As a result for the particle of small mass the random velocity jumps are happens more often and have the larger magnitude. Thus, choosing the space-time geometry in the form (2.2), (2.6), one can explain all nonrelativistic quantum effects without referring to quantum principles. Such a space-time geometry is more correct, than the Minkowski geometry, because in this case one does not need additional hypotheses in the form of quantum principles. In such a geometry the quantum constant appears in the theory together with the distortion function (2.6). It is an attribute of the space-time, that agrees with the universal character of the quantum constant $\hbar$.

3 Dynamical conception of statistical description

As we have mentioned, the choice of the space-time geometry is determined by the condition that the statistical description of the stochastic motion of particles is to coincide with the norelativistic quantum-mechanical description. It means that the quantum mechanics is to be represented as a statistical description of randomly moving particles. In the end of XIX century the thermodynamics was presented as a statistical description of chaotically moving molecules. After this representation many researchers thought that something like that can be made with the quantum mechanics. It was a common practice to think that any statistical description is produced in terms of the probability theory. In this point we meet AD.5, where it is supposed that there is no statistical description without the probability theory, Attempts [12, 13] of formulating the quantum mechanics in terms of the probability theory failed. The fact is that, attempting to represent the quantum mechanics as a statistical description of stochastic particle motion, one overlooks, that the random component of the particle motion can be relativistic, whereas the regular component remains to be nonrelativistic.

The probability theory, applied successfully to the statistical physics for statistical description of chaotic molecule motion, is not suitable for a description of the stochastic motion of relativistic particles. The fact is that, the employment of the probability density supposes splitting of all possible system states into sets of simultaneous independent events. In the relativistic theory it cannot be made for continuous dynamic system, as far as there is no absolute simultaneity in the special relativity. The simultaneity at some coordinate system cannot be used also, because the coordinate system is a method of description. Application of the probability theory and of the conditional simultaneity (simultaneity at some coordinate system) means an application of the statistics to the description methods instead of the necessary calculation of the dynamic system states.

One can overcome the appeared obstacle, rejecting employment of the probability theory at the statistical description. Indeed, the term "statistical description" means only that one considers many identical, or almost identical objects. Application of the probability theory in the statistical description is not necessary, because it imposes some constraints on the method of the description, that is undesirable. For instance, the probability density must be nonnegative, and sometimes this constraint cannot be satisfied.

In the nonrelativistic physics the physical object to be statistically described is a particle, i.e. a point in the usual space or in the phase one. The density of points (particles) in the space is nonnegative, it is a ground for introduction of the probability density concept. In the relativistic theory the physical object to be statistically described is a world-line in the space-time. The density of world lines in the vicinity of some point x is a 4-vector, which cannot be a ground for introduction of the probability density. The alternative version, when any world line is considered to be a point in some space $\mathcal{V}$, admits one to introduce the concept of the probability density in the space $\mathcal{V}$ of world lines. But such a description is non-local, as far as two world lines, coinciding everywhere except for some remote regions, are represented by different points in $\mathcal{V}$, and this points are not close, in general. In other words, such an introduction of the probability is very inconvenient.

To get out of this situation, one needs to reject from employment of the probability theory at the statistical description. Instead of the probabilistic conception the dynamical conception of statistical description (DCSD) should be used. Instead of the stochastic system $\mathcal{S}_{\rm st}$, for which there are no dynamic equations, one should use a set $\mathcal{E}[N,\mathcal{S}_{\rm st}]$, consisting of large number N of identical independent systems $\mathcal{S}_{\rm st}$, known as the statistical ensemble of systems $\mathcal{S}_{\rm st}$. The statistical ensemble $\mathcal{E}[N,\mathcal{S}_{\rm st}]$ forms a deterministic dynamical system, for which there are dynamic equations, although they do not exist for elements $\mathcal{S}_{\rm st}$ of the statistical ensemble. The statistical description lies in the fact that one investigates properties of $\mathcal{E}[N,\mathcal{S}_{\rm st}]$ as a deterministic dynamic system, and on the basis of this investigation one makes some conclusions on properties of its elements (stochastic systems $\mathcal{S}_{\rm st}$)). As far as one investigates a dynamic system (statistical ensemble) and its properties, there is no necessity to use the concept of probability.

Along with the statistical ensemble $\mathcal{E}[N,\mathcal{S}]$ of systems $\mathcal{S}$, or even instead of it, one can introduce the statistically averaged dynamic system $\langle\mathcal{S}\rangle$, which is defined formally as a statistical ensemble $\mathcal{E}\left[N,\mathcal{S}\right]$, $(N\rightarrow\infty)$, normalized to one system. Mathematically it means that, if $\mathcal{A}_{\rm E}\left[N,d_N\left\{X\right\}\right]$ is the action for $\mathcal{E}\left[N,\mathcal{S}\right]$, then

$$\langle\mathcal{S}\rangle:\ \ \mathcal{A}_{\langle S\rangle}\left[d\left\{X\right\}\right]=\lim_{N\rightarrow\infty}\frac{1}{N}\mathcal{A}_{\rm E}\left[N,d_N\left\{X\right\}\right],\qquad d\left\{X\right\}=\lim_{N\rightarrow\infty}d_N\left\{X\right\}$$

is the action for $\langle\mathcal{S}\rangle$, where X is a state of a single system $\mathcal{S}$, and $d_N\left\{X\right\}$ is the distribution, describing in the limit $N\rightarrow\infty$ both the state of the statistical ensemble $\mathcal{E}\left[N,\mathcal{S}\right]$ and the state of the statistically averaged system $\langle\mathcal{S}\rangle$.

Replacement of the statistical ensemble $\mathcal{E}\left[N,\mathcal{S}\right]$ by the statistically averaged system $\langle\mathcal{S}\rangle$ is founded on the insensibility of the statistical ensemble to the number N of its elements, under condition that N is large enough. The statistically averaged system $\langle\mathcal{S}\rangle$ is a kind of a statistical ensemble. Formally it is displayed in the fact that the state of $\langle\mathcal{S}\rangle$, as well as the state of the statistical ensemble $\mathcal{E}\left[N,\mathcal{S}\right]$ is described by the distribution $d_N\left\{X\right\}$, $N\rightarrow\infty$, whereas the state of a single system $\mathcal{S}$ is described by the quantities X, but not by their distribution. Using this formal criterion, one can distinguish between the individual dynamic system $\mathcal{S}$ and the statistically averaged system $\langle\mathcal{S}\rangle$.

To obtain the quantum mechanics as a statistical description of stochastic motion of micro particles, one needs to make one important step more. It is necessary to introduce the wave function ψ, which is the main object of quantum mechanics. Usually the wave function is introduced axiomatically, i.e. as an object, satisfying a system of axioms (principles of quantum mechanics). for this reason the meaning of the wave function is obscure. To clarify it, one has to introduce the wave function as an attribute of some model.

If $\mathcal{S}$ is a particle (deterministic or random), then the statistical ensemble $\mathcal{E}\left[N,\mathcal{S}\right]$ of particles $\mathcal{S}$, or statistically averaged particle $\langle\mathcal{S}\rangle$ are continuous dynamic systems of the fluid type. It is well known [6], that the Schrödinger equation can be represented as an equation, describing irrotational flow of some ideal fluid. In other words, the wave function can be considered to be an attribute of irrotational fluid flow. One can show [4], that the reciprocal statement (any fluid flow can be described in terms of a wave function) is also valid. The rotational flow is described by a many-component wave function. In other words, at the rotational flow the spin appears.

As far as the statistically averaged particle $\langle\mathcal{S}\rangle$ is a fluid, the wave function appears to be a description method of this fluid $\langle\mathcal{S}\rangle$. For the statistical description of the particle $\mathcal{S}$ would coincide with the quantum mechanical description, it is necessary to find the state equation of the fluid $\langle\mathcal{S}\rangle$, which is determined in turn by the form of the distortion function D. Corresponding calculation was made in the paper [11]. This calculation determines the form (2.6) of the distortion function. Then one obtain the conception, which will be referred to as model conception of quantum phenomena (MCQP). For the conventional presentation of quantum mechanics the term axiomatic conception of quantum phenomena (ACQP) will be used.

Dynamical conception of statistical description (DCSD) generates a less informative description, than the probabilistic statistical description in the sense that some conclusions and estimations, which can be made at the probabilistic description, cannot be made in the scope of DCSD. One is forced to accept this, because one cannot obtain more informative description. The fact that the quantum mechanics is perceived as a dynamical (but not as a statistical, i.e. probabilistic) conception is connected with the employment of DCSD. In turn application of DCSD is conditioned by "relativistic roots" of the nonrelativistic quantum mechanics. The "dynamic perception" of quantum mechanics takes place in the scope of both conceptions MCQP and ACQP. Let us note that DCSD is an universal conception in the sense that it can by used in both relativistic and nonrelativistic cases.

4 Identification of individual particle with the statistically averaged one

"Dynamical perception" of quantum mechanics leads to the fact that the statistically averaged particle $\langle\mathcal{S}\rangle$, described by the wave function, is considered to be simply a real particle $\mathcal{S}$. The question, why the real particle $\mathcal{S}$ is described by the wave function ψ, i.e. by the continuous set of variables (but not by position and momentum as an usual particle), is answered usually, that it is conditioned by the quantum character of the particle. One refers usually to the quantum mechanics principles, according to which the quantum particle state is described by the wave function ψ, whereas the classical one is described by a position and momentum. At this point we meet AD.6, when one does not differ between the statistically averaged particle $\langle\mathcal{S}\rangle$ and the individual particle $\mathcal{S}$.

As a corollary of such an identification the properties of $\langle\mathcal{S}\rangle$ and $\mathcal{S}$ are confused, and an object with inconsistent properties appears. As long as we work with mathematical technique of quantum mechanics, dealing only with $\langle\mathcal{S}\rangle$, no contradictions and no paradoxes appear. But as soon as the measurement process is described, where both objects $\langle\mathcal{S}\rangle$ and $\mathcal{S}$ appear, the ground for inconsistencies and paradoxes come into existence. Combinations of contradictory properties may be very exotic.

There are at least two different measurement processes. The measurement (S-measurement), produced under an individual system $\mathcal{S}$, leads always to a definite result and does not influence the wave function, which is an attribute of the statistically averaged system $\langle\mathcal{S}\rangle$. The measurement ($M$-measurement), produced under the statistically averaged system $\langle\mathcal{S}\rangle$, is a set of many S-measurements, produced under individual systems $\mathcal{S}$, constituting the statistically averaged system $\langle\mathcal{S}\rangle$. The N-measurement changes the wave function of the system $\langle\mathcal{S}\rangle$ and does not lead to a definite result. It leads to a distribution of results.

The following situation the most frequently. One considers that the wave function describes the state of an individual system, and a measurement, produced under individual system, changes the state (wave function) at this system. As a result a paradox, connected with the wave function reduction and known as the Schrödinger cat, appears. A corollary of such an approach is so called many-world interpretation of quantum mechanics [15, 16].

5 Identification of Hamiltonian and energy at the secondary quantization of relativistic field

The energy of a closed dynamic system is defined as the integral from the time component T^{00} of the energy-momentum tensor

$$E = \int T^{00} d\mathbf{x} \tag{5.1}$$

The energy is a very important conservative quantity. The Hamilton function (Hamiltonian) of the system is a quantity canonically conjugate to time, i.e. the quantity, determining the time evolution of the system. By their definitions the Hamiltonian H and the energy E are quite different quantities. But in the nonrelativistic physics (classical and quantum) these quantities coincide in many cases. For instance the energy of a particle in a given potential field $U(\mathbf{x})$ has the form $E = \mathbf{p}^2/2m + U(\mathbf{x})$. The Hamiltonian of the particle has the same form. On the ground of this coincidence an illusion appears, that the energy E of dynamical system plays a role of the quantity, determining its evolution, i.e. the role of the its Hamiltonian H. An illusion appears that the energy and the Hamiltonian are synonyms, i.e. two different names of the same quantity.

Such an identification of energy and Hamiltonian is used in the relativistic quantum theory, where such an identification cannot be used. For instance, it is common practice to consider [17], that in the dynamic system $\mathcal{S}_{\mathrm{KG}}$, described by the Klein-Gordon equation, the particle energy may be both positive and negative. A ground for such an statement is the fact that the flat wave in $\mathcal{S}_{\mathrm{KG}}$ has the form

$$\psi = Ae^{ik_0 t - i\mathbf{kx}} \tag{5.2}$$

where the quantity $k_0 = \sqrt{m^2 + \mathbf{k}^2}$ is interpreted as an energy. It may be both positive and negative. The statement that the energy may be negative is made in spite of the fact that the energy-momentum tensor component

$$T^{00} = m^2\psi^*\psi + \nabla\psi^*\nabla\psi \tag{5.3}$$

which enters in the expression (5.1), takes only nonnegative values. In reality, the quantity k_0 is a time component of the canonical momentum, which can have any sign. But the particle energy is always nonnegative.

Thus in the given case one has the associative delusion (AD.7), which lies in the fact that the properties of Hamiltonian are attributed to the energy. As long as such an identification is produced on the verbal level, it leads only to a confusion in interpretation and nothing more. But in the quantum field theory (QFT) such an identification has a mathematical form, and it has far-reaching consequences for the secondary quantization of the scalar field ψ. In the relation

$$i\hbar\frac{\partial\psi}{\partial t} = H\psi - \psi H, \tag{5.4}$$

describing evolution of the dynamic variable ψ, the Hamiltonian H is replaced by the energy (5.1), i.e. the relation (5.4) is written in the form

$$i\hbar\frac{\partial\psi}{\partial t} = E\psi - \psi E \tag{5.5}$$

The relation (5.5) is an additional constraint which is not necessary for carrying out the secondary quantization. (The secondary quantization may be produced without imposing the condition (5.5)). If the additional condition is imposed, one should test its compatibility with dynamic equations. Unfortunately, there is no understanding of such a test necessity. The condition (5.5) appears to be compatible with the dynamic equations only in the trivial case of the linear field ψ. For the nonlinear field the condition (5.5) appears to be incompatible with dynamic equations that manifests itself as the form of nonstationary vacuum state.

Theory of the scalar field ψ, quantized in accord with the condition (5.5), is an inconsistent conception, which is convenient in the relation that it (as any inconsistent conception) admits one to explain some facts, which cannot be explained in the scope of a consistent conception, which does not use the constraint (5.5).

The identification of the energy and Hamiltonian $E = H$ (condition (5.5)) has been used in QFT during the second half of XX century, i.e. about 50 years. This condition is not necessary for the secondary quantization, and its incompatibility with dynamic equations is rather evident. Why was this condition not come under a storm of criticism in this long time? In QFT there are many difficulties and problems, and the QFT consistency was open to question many times. But the condition (5.5) had not to be in doubt. The author had discussed this question with experts in QFT, but any time the their reaction was similar. The author opponents did not adduce counter-arguments and agreed that the condition (5.5) is not too well, but they rejected to make conclusions, concerning the second quantization procedure. Put very simply, they searched reasons of the QFT difficulties on its surface, giving up to analyze the QFT foundations.

6 On styles of investigation

Their considerations look approximately as follows. Let us introduce an additional supposition (AS) and study its consequences for theory and experiment. If the consequences are positive, the additional supposition (AS) is accepted and is introduced into the theory. If the consequences are negative, additional supposition is removed and a new additional supposition is considered. Such additional suppositions were: normal ordering, renormalizations, increase of the space-time dimension with the subsequent compactification, strings, etc. This style of investigation: additional supposition with subsequent test of its consequences will be referred to as P-style (pragmatic style) of investigation. Such a style is characteristic not only for the QFT development. In the beginning of XX century the quantum mechanics development was carried out also by means of P-style. The quantum mechanics developed, fighting against the classical style (C-style) of investigations, established to the end of XIX century. In this fight the P-style gained a victory over the C-style, which played a role of representative of classical (nonquantum) physics. Successors of Ptolemeus used the P-style, whereas successors of Copernicus used the C-style. The competition of successors of Ptolemeus with the successors of Copernicus was at the same time a competition between P-style and C-style. Then the C-style gained the victory. C-style reached its fullest flower to the end of XIX century. At the investigations of quantum phenomena in the XX century C-style gave the way to P-style.

Why do two different styles of investigation exist? Why does the investigation C-style or the investigation P-style gain alternatively the competition? The answer is as follows.

C-style is a style of investigations in the scope of a consistent theory. It puts in the forefront the consistency of a theory. C-style restricts suggestion of additional suppositions (hypotheses), insisting, that additional suppositions be consistent with primary principles of a theory. (Let us recall Newton's words: "I do not invent hypotheses"). In virtue of its requirement rigidity the C-style has the more predictive force, than the P-style, where these requirements are not so rigid. Among the C-style requirements there are ethic requirements to researchers. For instance, an researcher, which publishes insufficiently founded paper, containing arbitrary (i.e. not following from the primary principles) suppositions, risks losing his scientific face.

Adherents of the C-style pay attention to fundamental problems of a theory, and, in particular, to results and predictions of the theory, which are important for its further development. Solutions of concrete practical is considered to be not so important, because a solution of any special problem is a formal application of primary principles and mathematical technique to conditions of the new problem, and nothing beyond this. Such a relation of the researcher, using the C-style, to a solution of special problems is founded on his confidence that the primary principles are valid and the theory is consistent.

The predictability of the C-style, rigidity of its requirements and its self-reliance are true, provided the primary principles of a theory are true. If the primary principles contain a mistake, some predictions of the theory appears to be false. They force onto searching for a mistake, which may occurs in the primary principles or in the conclusion of corollaries from them. The most frequently a mistake is discovered in incorrect application of the primary principles.

But if the mistake in conclusions of a theory (discrepancy between predictions of the theory and experiment) has not been discovered for a long time, the necessity of the cognition progress and necessity of improvement of the terminology for the experimental data description generate a more pragmatic style (P-style) of investigations.

The P-style puts in the forefront a possibility of the experimental data explanation, what is obtained usually by introduction of additional suppositions. The theory consistency is considered to be not so important. although the representatives of the P-style declare, that they tend to elimination of inconsistencies, but it does not succeeded always, and is considered to be a less defect, than impossibility of the experiment explanation. The P-style admits an introduction of additional suppositions, even if they appear to be inconsistent with primary principles. It is important only, that they were useful and lead to explanation of experimental data. The P-style imposes essentially more slight requirements to researchers. For instance, the scientific reputation of a researcher does not lack or lacks slightly, if he, writing a very good paper, writes thereafter several mediocre or even incorrect papers. Predictability of the P-style is essentially less, than that of the C-style, as far as P-style admits only a"short logic" (short logical chain). For instance, it is widely believed among researchers dealing with quantum theory that essentially new result can be obtained, only suggesting some essentially new supposition in the scope of quantum theory. The idea that a novelty may be found in the primary principles (i.e. outside the scope of quantum theory) and the new result is a corollary of a long logical chain of considerations is perceived as something unreal.

Pragmatism of the P-style manifests itself in setting in the forefront a solution of concrete practical problems. It is supposed that a young talent gifted researcher is to solve concrete problems, whereas solution of fundamental problems is supposed to be a work for elderly experienced researchers. According to such a viewpoint usually one ignores and does not discuss facts and results which are important for further development of a theory, but which do not deal directly with its practical applications. Behind such a relation one can see an uncertainty of the P-style representatives in the primary principles of a theory and in its consistency. If a practical problem fails to be solved, the P-style representatives are ready to suggest additional suppositions and even to revise the primary principles.

The P-style appears to be more effective, only if the C-style appears to be ineffective. The last takes place, if the primary principles contain either mistake or defect. In other words, the C-style is effective, than the P-style only at absence of obstacles (systematic noise). The P-style is noise-resistant, under presence of the "systematical noise" appears to be more effective, than the C-style. In the period of a long P-style dominance a theory degenerates. Accumulating many additional supposition, contradicting each other, the theory gives up step-by-step its predictable force and capacity of valid development. Situation was such in the time of dominance of the Ptolemaic doctrine. The same situation takes place now with the quantum field theory.

In general, the C-style is more effective and predictable, provided the primary principles are valid. The P-style is useful in the relation, that it works even in the case, when there is a mistake in the primary principles, and C-style cannot work. In this case the P-style admits one to introduce new adequate concepts and terminology for descriptions of experiments that cannot be explained by the theory, based on the primary principles. Finally, investigations, realized by means of the P-style, help one to discover mistake in the choice of primary principles and produce a necessary revision.

Any style of investigations is conservative. It is worked out by a researcher in the course of all his research activity. If the researcher used the P-style, i.e. he uses essentially the trial and error method, He gets accustomed hardly to rigid restrictions of the C-style. Vice versa, a researcher, using the C-style in his work, gets accustomed to work with consistent conceptions. It is very difficult for him to pass to more free P-style and to invent new additional supposition which are necessary for explanations of new experiments. Conservatism of the investigation style leads to a conflict, when the dominating investigation style changes. For instance, in the time of Ptolemeus the P-style dominated. Discovery of AD.2 needed to construct a consistent conception of the celestial mechanics which would be free of arbitrary suppositions. The conflict between the successors of Ptolemeus and those of Copernicus was in the same time a conflict between the investigation styles.

Now practically all researchers dealing with relativistic QFT use P-style. They perceive difficultly arguments of the C-style proponents, having found inconsistencies and mistakes in primary principles of the quantum theory.

7 History of the associative delusions overcoming

Associative delusions have rather unusual properties. Usually they are overcame unintentionally. As a rule, the associative delusions are discovered and realized only after they have been overcame. Overcoming of AD.2 (conflict between the Copernicus doctrine and the Ptolemaic one) continues for several decades. Both sides of the conflict understood very well the reason of disagreement, but the fact, that this reason was an associative delusion, was interesting for nobody.

The following associative delusion AD.3 (the Cartesian coordinate system as a fundamental object of geometry) had been overcame in the course of the second half of XIX century. Its overcoming appeared to be not noticed in the sense that the reason of conflicts in the scientific community remained to be obscure. In the case of AD.2 it was clear, what was a subject of disagreement. In the case of AD.3 the reason of the conflict remained to be not clear. The mathematical community did not accept non-Euclidean geometry. There are evidences of Felix Klein [18]. It is known also that the Russian mathematical community related with a prejudice to works of N.I. Lobachevski on the geometry of negative curvature and to N.I.Lobachevski himself as an author of these works [19]. Unfortunately, the reasons of this prejudice had not been analyzed (at any case such an analysis is not known for us), and this episode remained in the history of mathematics as an unclear flash of conservatism.

Associative delusions AD.4 – AD.7 are found at the stage of overcoming. On one hand, an objective analysis of methods of AD overcomings is very difficult. On the other hand, such an analysis is necessary, because it would make overcoming of AD.4 – AD.7 easier. Besides AD.4 – AD.7 are not the last associative delusions on the way of the cognition process. Investigation of properties of associative delusions could help one to overcome the next AD.

AD.4 – AD.7 were discovered and overcame by one person – the author of this paper. Discovery and overcoming of ADs is rather rare processes. In this connection it is very difficult to answer the questions of the type. With what is a discovery of ADs connected? What are the circumstances, at which the discovery of ADs takes place? Why do some researchers succeed in overcoming of ADs, whereas other do fail?

Motives and attendant circumstances are known usually only for those researchers, who participated directly in overcoming of AD (in the given case this is the author of the paper). The process of overcoming was rather long (about 40 years). The diary was not kept, and one is forced to entrust to recollections. But the human memory is not enough reliable. The human beings are apt to forget events and circumstances, especially, if they are unpleasant for them. A subconscious mythlogisation of the investigation process takes place. But recollections of the only direct participant of the process of the ADs discovery and overcoming are of certain interest for further investigations of associative delusions, even if they are subjective and strongly mythologized. Such recollections are of interest for subsequent investigators even in the case, if their author had not understood, or had understood incorrectly, what he had done in reality. They are unique and are of interest, even if their author had described instead of important circumstances some unessential details, which he remembers for some reasons.

Experience of overcoming of AD.4 – AD.7 evidences that in some case the overcoming of associative delusion happens to be accidental, or it is an accessory result of investigations, carried out with other goal. In other cases ADs appear to be connected with insufficient understanding of the existing theory (in particular, the relativity theory), and overcoming of AD is carried out consciously. However, taking part in the process of the AD overcoming, it is very difficult one to analyze objectively this process. Apparently, the objective analysis will be possible only, when the process of the AD overcoming has been over. The author can make his contribution to this analysis only by one way – he should tell, how *from his viewpoint* the process of this overcoming passed. Of course, one can produce this analyze on the ground of published papers. But in this case one cannot take into account motives of the paper writing and the important circumstance, that sometimes the obtained results differ from those ones that the author wanted to obtain. Motives and goals of the author are unessential for estimation of his contribution, but they are essential for investigation of the cognition process (analyses of the process of the AD overcoming).

In general, recollection should be printed in the memoirs, but not in the scientific paper. In the given case there is the excuse that the question is connected not only with recollections, but with recollections accompanied by formulae. Besides the question is about evidences (maybe, very subjective), concerning very rare cognition process, which is the AD overcoming.

Thus, evidences of the participant will be presented. They are presented from the first person singular, in order to underline subjective character of recollection and separate them from other part of presentation, which pretends to objectivity. The evidences of participant do pretend by no means to a review of investigations, connected with overcoming of AD.4 – AD.7, and all references to other researchers are cited so far as they influence the participant investigations and remain in his memory.

8 Beginning of AD.4 overcoming. Evidence of participant.

The idea that the space-time is described completely by interval (distance) between pairs of events appeared at once after my acquaintance with the relativity theory. (It took place in 1955, when I was a second-year student). I supposed that all experts thought the same and did not see nothing new and surprising in such a point of view. (Some physicists told me many years ago, that they hold the same viewpoint). I was somewhat surprised that the infinitesimal interval was used (but not finite). I explained this fact to myself that it is easier to work with infinitesimal interval, because it contains less information (functions of one space-time point), than the finite interval (function of two space-time points). At first, I did not understand the circumstance, that an introduction of an infinitesimal interval is impossible without introduction of the dimension and manifold. I considered the dimension and the manifold to be natural attributes of the space-time.

Some years later I undertook a study of that, to what extent the space-time could be described in terms of a finite interval. The finite interval was attractive by the fact, that it contains more information, than the infinitesimal one. For instance, a geodesic, described usually by a system of four ordinary second order differential equations, is described in terms of the finite interval $S(x,x')$ as a solution $x'=x^i(\tau)\,,\quad i=0,1,2,3$ of a system of four algebraic equations

$$\frac{\partial G(x,x')}{\partial x'^{i'}}=b_{i'}\tau,\qquad b_{i'}=\text{const},\qquad G(x,x')=\frac{1}{2}S^2(x,x')\tag{8.1}$$

The quantity, which is denoted now via σ and known as the world function, I denoted by G. For me this designation associated with gravitation. The quantities, depending on two space-time points, I denoted by capital letters.

It should note that I had worked out somewhat unusual and, as I am understanding now, unpleasant for my colleagues style of work. The style of my work was such a kind, that before undertaking an investigation I had not studied or have studied very slightly works of my predecessors. I connect appearance of such a style with the fact, that in the course of my studying at the university I had practically no scientific advisor, who could give me a definite theme for investigations and press on so that I investigated it. Of course, I had an official scientific advisor. He suggested me a theme for a diploma work and for the Ph.d. thesis. But I was a self-willed student. I ignored recommendations of my scientific advisor and investigated those subjects, which I considered as interesting and necessary. My scientific advisor, qualifying me as a "non-controlled student" did not insist on his choice of the subject of investigation and does not hinder my self-will in the choice of the investigation subject. Moreover, he supported me by all means in my work.

Usually the scientific advisor suggests the subject for investigation and corresponding literature and, doing so he guarantees that this work will not be a rediscovery of known results. I began my investigation of the finite interval properties (world function), as if such investigations had not been produced earlier. I knew nothing about them, and nobody of my colleagues could tell me anything about such investigations. Investigation was made in spring and summer of 1958, when I was five-year student of the physical faculty of Moscow state university.

Understanding possibility of rediscovery of known results, I tended subconsciously to prevent such a rediscovery. To eliminate the rediscovery, I must bring my investigation to obtaining of certainly new and interesting result. The possibility that my intermediate results appeared to overlap already known results, has not agitated me[2]. I ignored the evident way of investigation of the world function $G(x, x')$ – expansion into a series over powers of $x - x'$ and paid attention on the circumstance that the quantity $\Gamma_{kl}^{i}(x, x') \equiv G^{is'}G_{kl,s'}$ is a scalar at the point x' and the Christoffel symbol at the point x. Here the following designations are used

$$G_{kl'} \equiv \frac{\partial^2 G}{\partial x^k \partial x'^{l'}}, \qquad G_{kl,s'} \equiv \frac{\partial^3 G}{\partial x^k \partial x^l \partial x'^{s'}} \tag{8.2}$$

and $G^{is'}$ is the matrix reciprocal to the matrix $G_{kl'}$.

Moreover, it appeared that $\Gamma_{kl}^{i}(x, x')$ is the Christoffel symbol for a flat space, i.e. the Riemann – Christoffel curvature tensor for $\Gamma_{kl}^{i}(x, x')$ is equal to zero identically. As a result it appeared that one could introduce such a covariant derivative with respect to x^i, which is a covariant derivative in some flat space and which depends on a parameter – the point x'.

All this meant, that the world function and the Riemannian space V, which was described by it, are associated with a set of flat spaces $E_{x'}$, labelled by the point x' of the Riemannian space V. Investigation of this question showed that the spaces $E_{x'}$ are flat spaces tangent to the Riemannian space V at the point x'. It appeared that

[2]I guessed that it is not very ethically not to refer to my predecessors and tried not to admit this. But the fact that absence of references to predecessors disoriented the readers, which are acquainted at first with this subject, did not come to my mind. I knew about this a bit later.

the world function generates automatically some two-metric technique, realizing a geodesic mapping of the Riemannian space V onto $E_{x'}$. Moreover, it was discovered later on [20], that such a mapping is realized by any symmetric scalar function $G(x, x')$ of two points, for which one can determine the tensor $G^{is'}$. But in that time I had not known this result and did not think about a possibility of coming outside the scope of Riemannian geometry.

By the way, results of the work admitted one to make this already then. The most evident result of the work was a construction of the two-metric technique on the basis of the world function. It was the result that impressed my colleagues mostly. I myself considered that the main result of the paper was an obtaining of a system of differential equations for the world function of Riemannian space. The equation, which is satisfied by the world function σ of the Riemannian space is well known [21].

$$\frac{\partial\sigma}{\partial x^i} g^{ik}(x) \frac{\partial\sigma}{\partial x^k} = 2\sigma \tag{8.3}$$

It contains the metric tensor of the Riemannian space in an explicit form, and it cannot serve as an equation, splitting all symmetric functions $G(x, x')$ into two sets: one set contains the functions, which could be a world function of a Riemannian space, another set contains functions $G(x, x')$, which cannot play this role.

Using two-metric technique, one succeeded to eliminate the metric tensor from the equation (8.3) and obtain a system of differential equations for the world function, which did not contain metric tensor explicitly but in return it contained derivatives of the world function with respect to both arguments x and x' [22]. The world function of any Riemannian space satisfied this system of equations.

The system of equations put the question. What is described by the world function, which does not satisfy this system? Does it describe non-Riemannian geometry, or no geometry at all? In this time (in the beginning of sixtieths) I did not put this question, but I considered derivation of this system of equations as a main result of my work. My attempts to call attention of my colleagues to this result did not lead to a success. Agreeing that the development of two-metric technique was a progress, they were indifferent to the system of equations for the world function of a Riemannian space, that was considered by me as a main result.

The paper was published in slightly known journal [22] in 1962, almost three years after it had been submitted. In the time between the submission and publication some very important for me event happened. The book by J.L. Synge [21], where at first the general relativity was presented in terms of the world function, appeared in Moscow. I had learned from it that the world function was introduced at first by H.S. Ruse [23] and J.L Synge [24] practically simultaneously, and that its properties had been investigated.

But the book by J.L. Synge did not contain any information on two-metric technique and geodesic mapping on tangent spaces $E_{x'}$. My misgivings that results of my investigations would appear to be a rediscovery of known results were not justified. Moreover, my study of the book by J.L. Synge led to increase of my self-confidence. If being a student, I could without an anybody's help make an investigation and obtained the results, which had not been obtained by so experienced researcher as J.L. Synge, it meant that I was able to compete with any researchers. Further this increase of my self-confidence will help me in my investigations. This case validated my style of working, when I did not troubled myself with study of the literature, postponing this to the time, when the first results deserving publication would be obtained. I used such a style of work, because I preferred to develop a new direction, but not to continue investigations of my predecessors. At first, it was easier, and, second, an attentive study of literature influenced me very strongly. It pushed me to a beaten way and prevented from a choice of directions of investigations, natural from my viewpoint. These directions appeared sometimes to be alternative to conventional directions of investigations.

Until 1964 I had written several papers about the two-metric technique applications to gravitation and several papers on the unified field theory. Dealing with the unified field theory, I was led to conclusion that all universal phenomena and universal constants are a manifestation of the space-time properties. For instance, I was sure (and I am sure now) that multiplicity of any electric charge to elementary one was conditioned by the space-time properties, as well as the quantum constant $\hbar$ described the space-time properties. Besides to the end of 1964 I was convinced that one cannot succeed in the study of the space-time properties without understanding what is the quantization. This (and other circumstances) forced me to leave the Moscow university and to change a subject of my investigations.

9 Relativistic statistics and overcoming of AD.5, AD.6. Evidence of participant.

Since 1964 I stopped to deal with geometry, gravitation, and unified field theory. In several years I was dealing with applied works, and in the beginning of seventieths I started to develop the direction, which I called "quantum mechanics as a relativistic statistics". It is this direction, whose development is connected with overcoming of AD.5. Then I assumed that the quantum mechanics is a result of statistical description of randomly moving particles. It seemed to me very reasonable in the light of the fact that in the end of XIX century the thermal phenomena were explained successfully as a result of statistical description of chaotically moving molecules. I assumed that the quantum phenomena could be explained in the same way. This idea was not new, but numerous attempts to realize this program did not led to a success. As far as know, most of researchers consider this program to be wrong. They assumed that the quantum properties reflect "quantum nature" of the world and that they cannot be reduced to other "classical" properties.

I did assume that failure of the statistical description was connected with the nonrelativistic character of the statistical description. I assumed that although the quantum mechanics was nonrelativistic, but nevertheless, the statistical description was to be relativistic. In my opinion, the fact that the regular component of the particle motion was relativistic did not mean that the stochastic component was nonrelativistic also. To be on the safe side one was to use a relativistic statistical description, which should be valid independently of the case, whether the stochastic component of motion be relativistic or not.

Being a student, I studied the relativity theory, reading the book by V.A. Fock [25] and shared his viewpoint that the relativistic theory differed from the nonrelativistic one not only in relativistic invariance of their dynamic equations. As it is well known, the main difference between the special relativity theory (SRT) and the nonrelativistic physics lies in the fact that in the nonrelativistic physics there are the concept of simultaneity in remote points, whereas in SRT there is no such a simultaneity. It means essentially that the nonrelativistic physics is incompatible with the relativistic physics. But a compromise between the relativity and nonrelativistic physics was strongly necessary, as far the physics remained to be mainly nonrelativistic. Presentation of the whole nonrelativistic physics in the relativistic form, only to take into account small relativistic corrections, seemed to be unacceptable. One found a compromise. A concept of the relative simultaneity (simultaneity on a given coordinate system) was introduced. Thus, on one hand, the simultaneity was considered as though it were, and nonrelativists were satisfied. On the other hand the simultaneity was relative. It was considered as though it was not exist, and relativists were satisfied also. On one hand, this compromise facilitated for nonrelativists a transition to relativistic physics, admitting them to conserve nonrelativistic style of thinking at description of relativistic processes. But on the other hand, it was this style of thinking that hindered investigation of physical phenomena, whose relativistic character was not evident.

Unfortunately, this restricted character of the compromise, connected with application of relative simultaneity, was understood only by a few physicists. Practically all textbooks on the relativity theory were written on the ground of this compromise, Apparently, it is this compromise that is a reason why most of researches ignored application of space-time diagram in their investigations. Usually description was produced in terms of coordinate systems and relativistic invariance of dynamic equations. In other words, most of researchers thought in terms of Newtonian mechanics with corrections for relativistic invariance. Such an approach did not help to investigate relativistic phenomena, and J.L. Synge wrote about this in the preface to his book [21]. As for me, as well as J.L. Synge I thought in terms of space-time diagrams, i.e. in terms of space-time geometry.

I assumed that the probability theory cannot be used for statistical description of relativistic stochastic particles, as far as concept of the particle state is different in SRT and in the nonrelativistic physics. In SRT the physical object, connected with the particle is WL (world line). The fundamental concept, connected with the particle, is WL. The WL is primary, and a particle is an attribute of WL. The particle is a section of WL by the hypersurface t =const.

In the nonrelativistic physics a particle is a physical object, and world line is its attribute (history). Description of a physical object is divided into two parts: (1) the object state and (2) dynamic equations, describing the state evolution. Dynamics is insensitive to the method of decomposition into state and dynamic equations. For dynamics it is of no importance what is a physical object: WL, or particle. Vice versa, the statistical description is a calculus of states, and it is very important for it, what is the physical object and what is a state of this object. Different choice of the physical object and different decomposition into state and dynamic equations lead to different results.

The probability theory is suitable for calculus of the particle states, but not for that of WLs. But, it is a long way from understanding, that the probability theory is incompatible with the statistical description of relativistic particles, until a construction of statistical description without probability.

Understanding that the density of states of world lines is described by the 4-vector j^i, I tried to formulate the quantum mechanics in terms of 4-vector j^i, i.e. to obtain that which is called hydrodynamical interpretation of quantum mechanics. I tried to develop a mathematical technique, which could convert the state density j^i into a fundamental object of a theory, making the wave function to be an attribute of this object. I succeeded to state the statistical principle, determining construction of the statistical description [26]. Essentially, the statistical description was formulated as follows. At the statistical description one considers many independent stochastic particles. They form something like a gas or a fluid, whose state is described by the mean 4-flux of particles j^i, $\quad i = 0, 1, 2, 3$. Thus, one overcomes AD.5 (consisting in the statement that a use of the probability theory is a necessary condition of the statistical description. To derive the quantum mechanics it is necessary to pass from j^i to the wave function ψ. At this point a purely mathematical problem arose.

The fact is that the transition from the wave function to a hydrodynamics with the vector j^i, was well known [14]. But it leads to a irrotational flow, i.e. to a very special case of the flow. It was known how to construct the wave function for the irrotational flow. But how to make this in the case of arbitrary flow? It was a complicated mathematical problem, which had taken a long time and many efforts. But finally it had been solved. It appeared that it was necessary to integrate the complete system of hydrodynamic equations, i.e. the system, consisting of the Euler equations and dynamic equations, describing displacement of the fluid particles in the given velocity field (so called Lin constraints). As a result of this integration the order of the system reduced, arbitrary integration functions appeared, and the system became to be described in terms of Clebsch potentials. Thereafter one could introduce many-component wave function, which appeared as an attribute of the fluid description.

This result was obtained in the middle of eightieths, and was applied at first to hydrodynamics [27]. Thereafter the mathematical technique of dynamical conception of statistical description (DCSD) appeared. The wave functions turned to an attribute of the statistical description, and many unsolved problems became solvable.

It should note here, that there were numerous attempts to revise and to interpret the quantum mechanics in other way (see. [14, 28, 30, 31, 32] and references therein). All authors of these papers started from the wave function and Schrödinger equation, trying to give another more acceptable meaning to quantum quantities. As a result they obtained different interpretations of the quantum mechanics and nothing more, because the wave function remained to be a fundamental object of a theory. Turning the wave function to an attribute of statistical description, I deprived it the status of a fundamental object. As a result another conception, but not simply another interpretation of quantum mechanics appeared.

The solved problem was a problem of the hydrodynamics (mechanics of continuous medium) The hydrodynamics was such a field of physics, where I was not an expert. In that time I was working at the laboratory, where practically all researchers were hydrodynamicists. I reported my work on hydrodynamics at the session of a seminar of our laboratory. Hearing my report, hydrodynamicists of our laboratory were puzzled, because they did not see a result in my report. There, where I saw integration with appearance of three arbitrary functions, they saw only a complicated change of variables. There was a reason for such a viewpoint. I present briefly an essence of the problem, because it seems to me very important for understanding, what is the delusion and how it have been overcame.

Motion of ideal compressible fluid is described usually by five Euler equations for five dependent variables velocity $\mathbf{v}$, density ρ and entropy S

$$\frac{\partial\rho}{\partial t}+\boldsymbol{\nabla}(\rho\mathbf{v})=0 \tag{9.1}$$

$$\frac{\partial\mathbf{v}}{\partial t}+(\mathbf{v}\boldsymbol{\nabla})\mathbf{v}=-\frac{1}{\rho}\boldsymbol{\nabla}p, \qquad p=\rho^2\frac{\partial E}{\partial\rho} \tag{9.2}$$

$$\frac{\partial S}{\partial t}+(\mathbf{v}\boldsymbol{\nabla})S=0 \tag{9.3}$$

Here p is the pressure, and $E=E(\rho,S)$ is the internal energy per unit mass, considered to be a function of density ρ and entropy S. The internal energy $E=E(\rho,S)$ is a unique characteristic of the ideal fluid.

On one hand, the Euler equations form a closed system of dynamic equations, but on the other hand, they do not form a complete system of dynamic equations, describing dynamic system $\mathcal{S}_{\mathrm{fl}}$, called ideal fluid. To obtain a complete description, one needs to add more three equations, describing displacement of the fluid particles in the given field of velocity $\mathbf{v}(t,\mathbf{x})$. It can be made, adding so called Lin constraints [33]

$$\frac{\partial\boldsymbol{\xi}}{\partial t}+(\mathbf{v}\boldsymbol{\nabla})\boldsymbol{\xi}=0, \tag{9.4}$$

where Lagrangian variables $\boldsymbol{\xi}=\boldsymbol{\xi}(t,\mathbf{x})=\{\xi_\alpha(t,\mathbf{x})\}$, $\alpha=1,2,3$ are considered to be functions of independent variables $t,\mathbf{x}$. If one solves the equations (9.4) and determine $\boldsymbol{\xi}$ as functions of $(t,\mathbf{x})$, the finite relations

$$\boldsymbol{\xi}(t,\mathbf{x})=\boldsymbol{\xi}_{\mathrm{in}}=\mathrm{const} \tag{9.5}$$

describe implicitly the particle trajectories and motion of particles along them.

The Lagrangian coordinates $\boldsymbol{\xi}$ label the fluid particles, as far as they represent three independent integrals of the system of ordinary differential equations.

$$\frac{d\mathbf{x}}{dt} = \mathbf{v}(t, \mathbf{x}), \qquad \mathbf{x} = x(t, \boldsymbol{\xi}), \tag{9.6}$$

describing the particle displacement in the given velocity field. The Lin constraints (9.4) are equivalent to the system (9.6) of three ordinary differential equations, and hydrodynamicists do not consider them seriously, supposing that the main problem is a solution of the Euler equations. If they are solved, i.e. if the system of five partial differential equations has been solved, the subsequent solution of the system of three ordinary differential equations is essentially much easier problem. Essentially, there is no necessity to solve equations (9.6), as far as the system of Euler equations is closed, and one can find the fluid flow independently of equations (9.6). The displacement of fluid particles in itself, described by the equations (9.6), is not interesting for hydrodynamicists. All this led to that situation, when hydrodynamicists ignore the Lin constraints both in the form (9.4) and in the form (9.6). In action it led to the fact, that one tried to write the variational principle for the closed system of equations (9.1) – (9.3), but not for the dynamic system $\mathcal{S}_{\mathrm{fl}}$. It is impossible to write the variational principle for the closed system of equations (9.1) – (9.3). One succeeded to make this only after adding the Lin constraints (9.4) [34].

The complete system of dynamic equations (9.1) – (9.4) is invariant with respect to the relabelling of the fluid particles. It means, that the relabelling transformation of the fluid particles

$$\xi_\alpha \to \tilde{\xi}_\alpha = \tilde{\xi}_\alpha(\boldsymbol{\xi}), \qquad D \equiv \det \parallel \partial\tilde{\xi}_\alpha/\partial\xi_\beta \parallel \neq 0, \qquad \alpha, \beta = 1, 2, 3 \tag{9.7}$$

does not change the form of the system of equations (9.1) – (9.4), i.e. the relabelling group (9.7) is a symmetry group of the complete system of equations (9.1) – (9.4). Application of the curtailed system of equations (9.1) – (9.3) admits one to ignore labelling $\boldsymbol{\xi}$ of the fluid particles and carry out the description in terms of relabelling-invariant variables $\rho, \mathbf{v}, S$.

There is another method of simplifying the complete system of dynamic equations (9.1) – (9.4), other, than ignoring the variables $\boldsymbol{\xi}$ and description in terms of relabelling-invariant variables $\rho, \mathbf{v}, S$. Using the fact that the relabelling group (9.7) is a symmetry group of dynamic equations, one can integrate the complete system of dynamic equations (9.1) – (9.4) in the form

$$S(t, \mathbf{x}) = S_0(\boldsymbol{\xi}) \tag{9.8}$$

$$\rho(t, x) = \rho_0(\boldsymbol{\xi}) \frac{\partial(\xi_1, \xi_2, \xi_3)}{\partial(x^1, x^2, x^3)} \equiv \rho_0(\boldsymbol{\xi}) \frac{\partial(\boldsymbol{\xi})}{\partial(\mathbf{x})} \tag{9.9}$$

$$\mathbf{v}(t, \mathbf{x}) = \mathbf{u}(\varphi, \boldsymbol{\xi}, \eta, S) \equiv \boldsymbol{\nabla}\varphi + g^\alpha(\boldsymbol{\xi})\boldsymbol{\nabla}\xi_\alpha - \eta\boldsymbol{\nabla}S, \tag{9.10}$$

where $S_0(\boldsymbol{\xi})$, $\rho_0(\boldsymbol{\xi})$ and $g(\boldsymbol{\xi}) = \{g^\alpha(\boldsymbol{\xi})\}$, $\alpha = 1, 2, 3$ are arbitrary functions of the argument $\boldsymbol{\xi}$, and φ, η are new dependent variables, satisfying the dynamic equations

$$\frac{\partial\varphi}{\partial t} + \mathbf{u}(\varphi, \boldsymbol{\xi}, \eta, S)\boldsymbol{\nabla}\varphi - \frac{1}{2}[\mathbf{u}(\varphi, \boldsymbol{\xi}, \eta, S)]^2 + \frac{\partial(\rho E)}{\partial\rho} = 0 \tag{9.11}$$

$$\frac{\partial \eta}{\partial t}+\mathbf{u}(\varphi,\xi,\eta,S)\boldsymbol{\nabla}\eta=-\frac{\partial E}{\partial S}. \tag{9.12}$$

If five dependent variables φ, $\boldsymbol{\xi}$, η satisfy the system of equations (9.4), (9.11), (9.12), the five dynamic variables S, ρ, $\mathbf{v}$ (9.8)–(9.10) satisfy the dynamic equations (9.1)–(9.3). The indefinite functions $S_0(\boldsymbol{\xi})$, $\rho_0(\boldsymbol{\xi})$, and $g^\alpha(\boldsymbol{\xi})$ can be determined from initial and boundary conditions in such a way, that the initial and boundary conditions for variables φ, $\boldsymbol{\xi}$, η would be universal in the sense that they do not depend on the fluid flow.

The last condition means that at the description in terms of hydrodynamic potentials the total information on the fluid flow is contained in dynamic equations, and there is no necessity in giving the initial and boundary conditions, as soon as they may be universal. At the description in terms of relabelling-invariant variables S, ρ, $\mathbf{v}$ one needs to add initial and boundary conditions to dynamic equations to obtain all information on the fluid flow. The circumstance that dynamic equations (9.11), (9.12) contain initial and boundary conditions, introduced by means of (9.10), is unique. In accord with (9.9), (9.10) the physical quantities ρ, $\mathbf{v}$ are obtained as a result of differentiation of variables φ, $\boldsymbol{\xi}$, S, and the variables φ, $\boldsymbol{\xi}$, η may be interpreted as hydrodynamic potentials. This way of description can be called as description in terms of hydrodynamic potentials. These potentials associate with the name of Clebsch [35, 36], who has introduced them for a description of incompressible fluid.

Thus, ignoring the potentials $\boldsymbol{\xi}$, one came to a description in terms of relabelling-invariant variables $\rho,\mathbf{v},S$ by means of five equations (9.1)–(9.3). Description in terms of hydrodynamic potentials $\varphi,\boldsymbol{\xi},\eta$ led to a description by means of five equations (9.4), (9.11), (9.12). It seems that the system of equations (9.1)–(9.3) on one hand, and equations (9.4), (9.11), (9.12) on the other hand, can be derived one from other by means of a change of variables. The circumstance that the change of variables is differential in one direction, and it is integral in other direction, was not taken into account.

However, it is very important circumstance. Differentiation of potentials $\varphi,\boldsymbol{\xi},\eta$ at the change of variables leads to a loss of information on the fluid particle displacements, described by the equations (9.4). The wave function contains complete information on motion and displacement of the fluid particles, and it can be built only of potentials $\varphi,\boldsymbol{\xi},\eta$, and it cannot be built of relabelling-invariant variables $\rho,\mathbf{v},S$. Understanding of this circumstance appeared to be difficult for me and for other researchers. The last manifested itself in the fact that, when (in 1998) my paper on integration of hydrodynamic equations was submitted to Journal of Fluid Dynamics. It was declined on the ground that in the reviewer's opinion the transition from the curtailed system of dynamic equations (9.1)–(9.3) to the complete system (9.4), (9.11), (9.12) does not contain an integration, but only a change of variables.

Why did I succeeded to construct a fluid description in terms of the wave function and to solve the problem of hydrodynamics, where I was an undoubted amateur? I reflected on this question and came to the following conclusion. At first, I knew what I do search for and had made many unsuccessful attempts. In the course of these attempts I developed and improved the mathematical technique of work with Jacobians (Jacobian technique). Looking some years ago through papers of Clebsch [35, 36], I have discovered that he also used the Jacobian technique and had discovered hydrodynamic potentials. He wrote down Jacobians in the expanded form, as it was usual in that time. Due to this the formalism was rather bulky, but I think, that Clebsch was able to discover description in terms of wave functions, if there were a necessity for this. Second, I investigated the fluid simply as a dynamic system, as far as I did not know hydrodynamics. In return, I was not burdened with prejudices, connected with a knowledge of hydrodynamics. In particular, in hydrodynamics the variables $\boldsymbol{\xi}$ are well known as Lagrangian coordinates, which are used only as independent variables. I had met the Lagrangian coordinates as dependent variables only once in the book by V.A. Fock [25], when I studied the relativity theory, being a second-year student. I remember very well, that I was surprised by capacities, included in such a description.

Returning to the problem of overcoming of AD.5 and AD.6, I should like to note, that both delusions were overcame practically in the beginning of seventieths. Having perceived, that the statistical description can and must be constructed on a dynamical basis (without a use of the probability theory) meant overcoming of AD.5. Overcoming of AD.6 was a corollary of the fact, that the quantum mechanics is a statistical conception (statistical description of randomly moving particles).

10 Forced identification $E = H$. Evidence of participant on overcoming of AD.7

Overcoming of AD.7 (Forced identification $E = H$) was the first of my overcomings of AD. It took place in 1970. It was carried out consciously on basis of understanding that in the relativistic theory a physical object is world line (WL), but not a pointlike particle in the three-dimensional space. I took this truth from the book of V. A. Fock. [25]. Later I found confirmation of this viewpoint in papers of Stueckelberg [37] and Feynman [38]. In general such a viewpoint was in keeping with my style of geometrical thinking. This brought up the question: Is it possible to describe pair production in terms of classical relativistic mechanics? The pair production process is described by a turn of a world line in the time direction. It was well known. It was necessary to invent such an external field which could carry out this turn. It was clear, that adding an arbitrary field to the action of charged particle in a given electromagnetic field A_i

$$\mathcal{A}[q] = \int \left\{ -mc\sqrt{g_{ik}\dot{q}^i\dot{q}^k} + \frac{e}{c}A_i\dot{q}^i \right\} d\tau, \qquad \dot{q} \equiv \frac{d\dot{q}}{d\tau} \tag{10.1}$$

one could not carry out such a turn. The fact is that at the turn in time the world line becomes to be spacelike near the turning point. On the other hand, under the sign of radical in (10.1) must be nonnegative quantity. It means, that $g_{ik}\dot{q}^i\dot{q}^k \geq 0$

and, hence the world line is to be timelike (or null). In order the world line might be spacelike, the external field is to be introduced under sign of radical in (10.1). Then the expression under sign of radical may be positive even in the case, when $g_{ik}\dot{q}^i\dot{q}^k < 0$. I introduced the external field under the sign of radical, writing the action in the form

$$\mathcal{A}[q] = \int \left\{ -mc\sqrt{g_{ik}\dot{q}^i\dot{q}^k - \alpha f(q)} + \frac{e}{c}A_i\dot{q}^i \right\} d\tau, \tag{10.2}$$

where f is an external scalar field, and α is a small parameter, which tends to zero at the end of calculations. At the properly chosen field f the expression under the radical can be positive at $g_{ik}\dot{q}^i\dot{q}^k < 0$. It appeared that at the properly chosen field f, the world line turns in time indeed. This turn is conserved at $\alpha \to 0$. The direct calculations showed that at such a description the particle energy was positive always, but the time component p_0 of the canonical momentum and the particle charge $Q = e\,\mathrm{sgn}(\dot{q}^0)$ depended on sign of derivative $\dot{q}^0$, i.e. they were different for particle and antiparticle. It was rather sudden that the WL[3] charge Q, defined as a source of the electromagnetic field by the relation $Q = \int \delta\mathcal{A}/\delta A_0(\mathbf{x})d\mathbf{x}$, did not coincide with the constant e, incoming to the action, although at the correct description this was to be just so, as far as the particle and antiparticle had opposite sign of the charge. One can obtain coincidence of energy E and p_0, if one cuts the whole world line into segments, responsible for particles and antiparticles, and changes direction of the parameter τ increase on the segments, responsible for antiparticles, remaining τ without a change on segments, responsible for particles. But any change of the sign of $\dot{q}^0$ is to be accompanied by a change of the sign of the constant e. But the constant e is a parameter of the dynamic system, and a change of the sign of e on the segments, describing antiparticles means that particles and antiparticles are considered to be described in terms of different dynamic systems.

This simple example shows, that there are two possibilities of description

(1) To consider the world line (WL) to be a physical object. Then particle and antiparticle are two different states of WL, distinguishing by signs of the charge Q and those of the canonical momentum component p_0. The energy is positive in both cases, so $E \neq H$, in general.

(2) To consider the particle and the antiparticle to be different physical objects, described by two different dynamic systems. The parameter e is the particle charge, which is simultaneously a parameter of the dynamic system. It is different for a particle and for an antiparticle. At such a description the evolution parameter τ can be chosen in such a way, to provide fulfillment of the constraint $E = H$.

Imposition of the constraint $E = H$ provided automatically fragmentation of the world line into particles and antiparticles describing them as different physical objects, i.e. in terms of different dynamic systems. This was valid in classical physics. This must be valid in the quantum theory.

It was unclear for me, what is a use of the identification of energy with Hamiltonian. Why does one cut WL to obtain indefinite nonconservative number of particles

[3]designations WL is used for the world line, considered as a fundamental object

and antiparticles instead of fixed number of physical objects (WL)? From the formal viewpoint it is more convenient to work with constant number of object, than with alternating number of them. It was evident for me, that impossibility of working in QFT without the perturbation theory was connected directly with the fact that numbers of particles and antiparticles were not conserved separately. What for does one need to impose the condition $E = H$ and to restrict one's capacity, if one could not to impose any constraints? (Then I did not consider, that the condition $E = H$ might appear to be incompatible with dynamic equations).

It was necessary to discuss the paper with colleagues dealing with QFT, and I submitted my report to seminar of the theoretical department of the Lebedev Physical Institute, where there were many good theorists. At my report at the session I was surprised by the following circumstance. Nobody believed that the pair production effect could be described in terms of classical mechanics. Although my calculations were very simple, they casted doubt on their validity. It was decided to transfer my report to next session. One of participants of the seminar (V.Ya. Fainberg) was asked to verify my calculations and to report on the next session together with continuation of my report. Mistakes in my calculations were not found, and I completed successfully my report on the next session. After the session I seemed that the attention of participants of the seminar was attracted to the problem of possibility of pair production description in terms of classical physics, whereas the main problem, i.e. application the constraint $E = H$ in QFT, remained outside the scope. Corresponding my paper was published [39], but, as far as know, nobody payed any attention to it.

It was necessary to quantize nonlinear relativistic field without a use of the condition $E = H$ and to verify, if such a way of quantization had advantages over the conventional way, using this condition. It happened that such a quantization could be carried out without a use of normal ordering and perturbation theory [1]. The vacuum state appeared to be stationary. A possibility of quantization without the perturbation theory impressed. But I shall not be cunning and say directly, that I had no illusions about results of my work. In that time (beginning of seventieths) I assumed that the problem of the quantum mechanics relativisation (i.e. unification of quantum theory with the relativity theory) had no solution. I assumed that the quantum mechanics was something like relativistic Brownian motion, and the relativistic quantum theory should be developed in direction of statistical description of this relativistic motion [40].

My work on the secondary quantization of the nonlinear relativistic field was undertaken with the goal to manifest that the conventional way of the QFT development was a way to blind alley. The logic of my action was as follows. One quantizes the nonlinear field, using only principles of nonrelativistic quantum mechanics and ignoring any additional suppositions. One advances as far as possible. There were a hope that the quantization without the perturbation theory admitted one to clarify real problems of QFT and, maybe, to solve some of them.

The fact is that the use of the perturbation theory did not permit one both to state exactly problems of QFT and to solve them. The problems of collisions were the main problems of QFT. To state the collision problem, it is necessary to formulate exactly what is a particle and what is an antiparticle. According to quantum mechanics principles it is necessary for this to define the operator N_{p}^{i} of the 4-flux of particles and the operator N_{a}^{i} of the 4-flux of antiparticles. After such a definition one can state the problem of collisions. Surprisingly, it appeared that nobody tried to introduce these operators. Instead of this there were cloudy consideration about the interaction cut off at large time $t \rightarrow \pm\infty$. Thereafter these consideration on cut off were substituted by manipulations with *in*- and *out*-operators, that did not clarify the statement of the collision problem.

Even in the excellent mathematically rigorous book by F.A. Berezin [41] the collision problem was stated in terms of perturbed H and nonperturbed H_0 Hamiltonians of the system, that corresponds to interaction cut off at $t \rightarrow \pm\infty$. Of course, all this was only a reflection of the whole situation in QFT. I asked my colleagues dealing with QFT, how could one think in terms of the perturbation theory. They answered obscurely. I understood, that not any problem could be solved exactly. I was ready to use any methods of approximation (including the perturbation theory) by the indispensable condition, that the problem be stated exactly, but not in approximate terms. To state a problem in approximate concepts and terms was beyond my understanding.

As soon as the nonlinear field was quantized [1], results of my paper were reported on a session of the seminar of the theoretical department of Lebedev Physical Institute. Although the secondary quantization was produced without the perturbation theory, most of participants considered my results to be unsatisfactory on the ground that at the quantization one violated the condition

$$[\varphi(x), \varphi^*(x')]_- = 0, \qquad (x - x')^2 < 0 \tag{10.3}$$

which was interpreted usually as the causality condition. Indeed, if at the quantization the condition $E = H$ is not imposed, the commutator between the dynamic variables at the points, separated by a spacelike interval $x - x'$ cannot (and in some cases must not) vanish. Let me explain this in the example pair production, described in terms of classical physics, where the pair production is described by time zigzag of the world line. In this case the commutator (10.3) associates with the Poisson bracket. If the condition $E = H$ is imposed and the quantization is carried out in terms of particles and antiparticles, the dynamic variables X and X' at the points, separated by a spacelike interval $x - x'$, relate to different dynamic systems always. The corresponding Poisson bracket $\{X, X'\}$ between any dynamic variables X and X' at these points vanishes. In the case of quantization in terms of world lines the dynamic variables X and X' at the points, separated by a spacelike interval $x - x'$, can belong to the same world line, i.e. to the same dynamic system. Then the variables X and X' correspond to different values τ and τ' of evolution parameter τ. In this case the dynamic variables X at the point x are expressed via dynamic variables X' at the point x', and there exist such a dynamic variables X_1 at x and X_2' at x', that the Poison bracket $\{X_1, X_2'\}$ does not vanish. The condition (10.3) is violated with a necessity.

Thus, fulfillment or violation of the condition (10.3) is an attribute of a description. It coincides with the causality condition (i.e. with the objectively existing relation) only at imposition of the condition $E = H$. Unfortunately, I failed to convince my opponents of dependence the relation (10.3) on the way of description, although I tried to do this at the session and in discussions thereafter. Later on I had understood, that in this case we met associative delusion, when the properties of description are attributed to the object in itself. Unfortunately, it happens that many researchers meet difficulties at overcoming of AD, and as I am understanding now, the P-style used by most of researchers of QFT is a reason of these difficulties. Besides, formulating the condition (10.3) in terms of quantum theory, it is very difficult to discover that this condition is an attribute of a description, but not a causality condition.

Thus, I had overcame AD.7, but the scientific community as whole had not overcame it. I did not see a necessity in further convincing my colleagues to refuse from imposition of the condition $E = H$ at quantization. At first, I was convinced that the refusal from $E = H$ itself did not solve main problems of QFT. My belief, that QFT did not enable to solve the unification problem of quantum theory with relativity and that the statement of this problem was false in itself, became stronger. Secondly, I myself did not know exactly what must replace this problem of unification. I had only a guess on this account. I could not to convince a person, dealing with QFT and devoting essential part of his life to this, that he had chosen a wrong way. Without pointing a right way, such a convincing was useless and even cruel.

There were once more an important circumstance which influenced strongly on my interrelations with colleagues dealing with QFT. The fact is that, since I had discovered incorrectness of imposition the condition $E = H$, I met difficulties at reading papers on QFT. When I began to read any paper and discovered that the condition $E = H$ was used there (this was practically in all papers on QFT), my attention was cut off subconsciously, and I could not continue conscious reading. My reading became absent-minded, and I needed to bend my every effort to turn on my attention and continue a conscious reading. I do not know to what extent such a reaction is my individual property, but tearing off the papers using $E = H$ led gradually to my allergy to reading of papers on QFT. I stopped to read them, although I was interesting QFT always, and questioned my colleagues about QFT development at any suitable case.

Why did I overcome associative delusions comparatively easy? Apparently, it was connected with that I was an adherent of the C-style and ignored instinctively approaches, which were used by the C-style. It is difficult for me to say, whether this adherence to the C-style was innate, or it was a result of my education. But such an adherence took place undoubtedly, and the following case justifies this.

In the beginning of seventieths I had a position of the scientific secretary of the Space Research Institute. At this position I had a possibility to investigate all those problems which I wanted, and I dealt with problems of quantum mechanics and QFT. Thereafter I left this position and passed to the position of a senior scientific researcher in the department of G.I. Petrov, who was a director of the institute.

Then I should deal with problems connected to some extent with space research. I had a possibility of choosing a field of investigations with one constraint. In the course of a half a year I was to study literature on the subject, chosen by me and read a report on the seminar of G.I. Petrov to manifest my readiness for investigations in the new field.

Any field of physics, connected with the space research, was new for me, and I had chosen the problem of the pulsar magnetosphere model and investigation of the pulsar mechanism. In seventieths it was a slightly developed and perspective field of astrophysics. The pulsar phenomenon was discovered recently (in 1967), and there were the first considerations, concerning pulsar emanation formation. Studying the recent literature on pulsars, I had read a report on the session of the seminar. Despite the fact that I was a novice and amateur in this field, my report was very critical. It was accompanied by a suggestion of my own investigation program.

My pretensions to the authors of reviewed papers were in that they invented different hypotheses to avoid difficult calculations and attempted to guess a construction of the pulsar magnetosphere. In my opinion, they should to state the problem of the pulsar magnetosphere and to solve it by means of well known methods of classical physics. I considered that hypotheses might be suggested either on the first (heuristic) stage of investigations (but it was passed), or after all known methods had been used already. The problem was in the scope of the classical physics, and conceptual obstacles for its statement were absent. But most of authors did not want or was not able to state the problem correctly. Instead of this they advanced their hypotheses, attempting to explain the pulsar phenomenon at once, and argued, whose hypothesis is better. In other words, they used the P-style at the condition, when it was not effective, and it was no necessity of using it.

I did state simply the problem on the pulsar magnetosphere in such a way, as any researcher, using the C-style in his investigations, should do. I was working according this program in the course of 1976 – 1987. (see my concluding paper on the pulsar model [42]). In that time I did not think on styles of investigations. I assumed simply, that one needed to investigate a physical phenomenon honestly, but not to dodge, substituting calculations by conjectures. Maybe, my instinctive adherence to the C-style was so large, that penetrated to my subconsciousness and generated allergy to reading papers on QFT.

Maybe, me successes in overcoming of different ADs was conditioned by consecutive application of C-style, essence of which could be expressed by the Newton's words: "I do not invent hypotheses".

11 Can a curve be a fundamental object of geometry? Evidence of participant on completion of the AD.4 overcoming

My work with technique, based on the world function, showed, that such a description of geometry was insensitive to topological properties of the space. For instance, the metric tensor was the same for the Euclidean plane and for the cylinder, obtained from a band of this plane as a result of gluing its edges. But the world functions were different. This showed that the world function describes both local and global properties of geometry. Further I had discovered, that if all points of the space except for points with integer value of coordinates removed, the remaining point formed a discrete geometry, which could be described in terms of world function.

I understood also, that the world function contained complete information on geometry. In particular, in the case of the Euclidean space I could obtain information on the space dimension and construct the Cartesian coordinate system in the space, even in the case, when the space was an abstract set of points (but not a manifold). At such a construction one used a concept of the straight segment between points P and P', defined as set of points R, satisfying the equation

$$S(P,R) + S(R,P') = S(P,P') \tag{11.1}$$

where $S(P,P')$ denotes a distance between the points P and P'. I understood, that the world function of the Euclidean space possessed special extremal properties. It is these extremal properties that the equation (11.1) describes a one-dimensional line even in n-dimensional space, although at an arbitrary function $S(P,P')$ the equation (11.1) describes, in general a $(n-1)$-dimensional surface. However, then I did not understood, how important was this definition, where segment of the straight was defined without a reference to the concept of a curve.

In my representation of that time the world function was a result of integration of some expression determined by the metric tensor. This integration provided a connection between different points of the space. This connection was conserved, even if the most of space points were removed.

The property of the world function to provide a connection between points and its extremal properties seemed to me important. Realization of this fact took place in the middle of sixtieths. I wanted to write a paper about this properties, but it was not clear, where I could to publish such a paper. The fact is that this result in itself without applications seemed to me insufficiently important for its publication.

Opportunity for publication appeared only in the end of eightieths. I succeeded to publish two my articles, having no relation to geometry [43, 27], in Journal of Mathematical Physics. At the publication of these articles I was pleasantly surprised by the kind relation of editors. Then I got an idea of publishing a paper on extremal properties of world function which reflected my increasing understanding of the world function role in geometry.

The paper was entitled "Extremal properties of Synge's world function and discrete geometry." It was accepted to publication, but I was asked, first, to add references to contemporary papers and, second, to replace designation "G" which I used for the world function by conventional designation σ. Addition of reference to more recent papers did not change the paper practically. The world function was applied in papers on quantization of gravitation, where "long-range properties" of the world function were important. But nothing except for expansion over degrees of $x - x'$ was used in these papers.

But the formal replacement of designations G by another designation σ led suddenly to essential revision of the paper. The fact is that there were practically no personal computers in USSR, and my paper was written by means of typewriter. To change designations I was forced to rewrite the whole paper. As far as the paper should be retyped, I decided to include all corrections and additions which appeared in the time, when the paper was reviewed. In the course of revision I realized the importance of the definition (11.1) of the straight segment and revised the character of presentation. Essentially a paper on a construction of a new geometry appeared, although the term T-geometry was not used in the paper. I began to think about a change of the title, because the title of the paper did not reflect its content. The title should be changed.

In that time (1989) it was not simple. To submit a paper in a foreign journal one needed a permission of "Glavlit", where the title of the paper was mentioned. To obtain a permission for forwarding a manuscript with other title, it was necessary to translate the new version to Russian and submit to "Glavlit". The procedure of obtaining a new permission would need one-two months. Besides the paper was to be translated to Russian and typed. To avoid lack of time, I decided not to change the title. The paper was printed with the previous title [8].

At the transition from the Riemannian geometry to T-geometry it was very important to overcome inertia of thinking and realize, that a tube could play a role of the straight. Apparently, physical models of elementary particles helped me in overcoming of the inertia of thinking. In some models the particle was substituted by a string or by a membrane, and their world tubes were respectively two- and three-dimensional surfaces. It meant that in the real world a tube could play a role of a straight. Psychologically it was a very important step in construction of T-geometry. Another important circumstance was the fact that the straight (i.e. a natural geometrical object, determined by two different points) was defined as a set of points determined by the metrical property (11.1), i.e. without a reference to the concept of a curve. In Riemannian geometry the geodesic is defined as the shortest curve. At such a definition of geodesic, it cannot be a tube, and a use of the concept of a curve at the geometry construction discriminates nondegenerate geometries, i.e. geometries, where a tube plays a role of a straight. Note that the paper [8] had a transient character in the sense, that it described rather a transit from the Riemannian geometry to T-geometry, than the T-geometry as such.

The T-geometry appeared at first as a generalization of the Riemannian geometry and was applied as a possible space-time geometry [11]. Advantages of the space-time model based on T-geometry were evident. First, it appeared that the geometry depended only on interval between events and was insensitive to the space-time continuity, which cannot be tested. Second, in such a geometry the particle mass was geometrized, that was impossible in the Minkowski geometry. Finally, the stochastic particle motion, depending on the particle mass (i.e. on geometry), appeared in a natural way. All this testified that T-geometry was suitable for the space-time description.

The two-metric technique, developed for the Riemannian space worked very well in T-geometry, admitting to investigate it in the small [20]. But from the practical viewpoint it was unessential, because the behavior of world lines of particles with a finite mass depended on behavior of the world function at finite (but not at infinitesimal) intervals.

Although together with construction of T-geometry AD.4 was overcame in reality, realization of this fact was absent. At first, the T-geometry was derived as a generalization of Riemannian geometry, and the Riemannian geometry was considered to be a special case of T-geometry. In particular, in the paper [8] one investigated the question, what world function must be, for degeneration of world tubes into one-dimensional curves would take place and one could construct a manifold.

At first, I assumed that if the world function was given on a manifold and satisfied differential equations, which should be satisfied by the world function of a Riemannian space, then the T-geometry constructed on the base of this world function be a Riemannian geometry. I reality it was not so, because the role of the concept of a curve in the construction of the Riemannian geometry was not exhausted by a construction of geodesics. In the Riemannian geometry the parallel transport of a vector is founded also on the concept of a curve.

My interest to geometry had always an applied character. I interested in geometry as a method of description of space-time properties. I did not thought on a geometry as such, it was for me only a tool, and I troubled only with efficiency of this tool. In accord with my style of investigations I did not read papers on geometry, and I troubled only one question whether I rediscovered any known results. Before publication of the paper [8] I had consulted by a well known mathematician, who said that nobody in USSR deal with problems close to problems of T-geometry. After publication of the paper I took several attempts to discuss T-geometry with mathematicians. The mathematicians, interested in geometry in a large, discussed readily problems of T-geometry, and I had read several successive reports on T-geometry. As an "inventor" of T-geometry I was very glad, that the T-geometry was not built before, and I became gradually to understand, what was a reason of this circumstance. AD.4, i.e. belief of mathematicians that the curve was a fundamental object of geometry, was a reason of impossibility of the T-geometry construction.

My reports on seminars of mathematicians were very valuable for me. Firstly, I had reduced my geometrical ignorance. Secondly, I became gradually to understand, that T-geometry was rather a generalization of metric geometry, than that of Riemannian. I discovered that in contemporary mathematics the geometries did not classified practically, or such a classification was produced over accidental features. Moreover, there were a lack of coordination even in the definition, what is the geometry. For instance, well known Felix Klein [18] defined a geometry as a conception, containing some symmetry group. The Riemannian geometry did not fall definitely under Klein's definition. According to Klein the Riemannian geometry should be called the Riemannian topography (or geography). A.D. Alexandrov [44] used another definition of geometry.

The problem of classification is one of the most important problems in any science, and in mathematics especially. But I did not interested in this problem, because of my applied approach to geometry. I met this problem at the following circumstances. On one hand, T-geometry was a very general geometrical construction, founded only on the world function. Topological properties appeared to be derivative ones with respect to metric properties of geometry. From viewpoint of T-geometry one cannot set topological properties of the space independently. On the other hand, in the generalized Riemannian geometry [45, 46, 47] the topological properties are introduced at first, and thereafter one introduces metrical properties, what cannot do from the viewpoint of T-geometry. In general, as far as I could understand, in present time the topology is considered to be the most promising direction of the geometry development. In support of this statement one can mention the following fact. In the mechanics-mathematical faculty of Moscow university there three different chairs, whose titles contain words "geometry" and "topology" in different combinations.

As far as I saw a contradiction between the existence of T-geometry and that of topology as the most promising direction in geometry, the situation should be clarified in the process of discussions. My attempts to report T-geometry on seminars of the "most geometrical" chairs failed. Corresponding suggestions were declined on the ground that such a report would not be interesting for researchers of these chairs, who were interested in other problems. Then I prepared a report, entitled "Geometry without topology" and submitted it to Moscow Mathematical Society (MMS) with a request to hear it on one of sessions.

MMS (a very respectable organization) heard on its sessions only very important report (preliminary reviewed by experts). Review of my paper was negative. My work on T-geometry was recognized by the reviewer as insufficiently fundamental and unworthy of hearing on a session of MMS. This was not quite that, what I needed, because I needed a public discussion. Nevertheless, my goal was attained, although only by half. I obtained opinion of the expert, who was mostly competent in this problem. I had clarified from his review, that there are no classifications of geometries in mathematics, and the problem of "fundamental nature" of geometry in question is determined entirely by opinion of a reviewer.

My argued appeal was not taken into account, but the other appeared to be important. It was important, that the negative review and reviewer's argumentation forced me to think about the problem of "fundamental nature" of a geometry. As a result I came to a necessity of classification of geometries, and a key to the classification was the concept of CG (conception of geometry) as a method of a construction of a standard (Euclidean) geometry. A criterion of generality of CG was the amount of numerical information, which the given CG used for construction of the standard geometry. Such a classification of CG is presented on p. 9. It follows from this classification that purely metric CG generates a class of the most general geometries (T-geometries), whereas perspectives of topology-metric conception are in this respect unfavorable. The review was dated by the seventh November 2000. A result of my work over this review was a revision of my report, which turned to the article "Geometry without topology as a new conception of geometry" [48]. The paper was ready to the end of 2000th year. This paper was a corollary of my complete overcoming of AD.4.

Now I am presenting chronology of my overcomings of AD.4 – AD.7 and trying to find the reasons, why I have succeeded to overcame these associative delusions. AD.5 – AD.7 were overcame practically in the same time in the beginning of seventieths of XX century. This overcoming was a corollary of my understanding of the simple statement, that the physical object is WL (world line), but not a particle. Overcoming of AD.5 and AD.7 followed directly from this circumstance. Overcoming of AD.6 was a simple corollary of the fact, that the quantum mechanics is a statistical description of stochastic relativistic particles motion. Thus, finally, overcoming of AD.5 – AD.7 was *a corollary of my understanding of the relativity theory.* Overcoming of AD.4 was happening in the course of a long time. It was finished in the end of eightieths of XX century. Then it appeared in the form of a guess. This overcoming in the form of a logical corollary happened in the end of 2000 year, when *the conception of geometry* was introduced and the table, presented on p. 9 was obtained..

What is a reason of these overcoming? What properties did I have and did not have other researchers? I believe that these properties were as follows:
(1) My understanding of the relativity theory. I thought in terms of space-time diagrams, i.e. geometrically, but not in terms of relativistic invariance as most of researchers. I am connecting such a way of thinking with that, I had studied the relativity theory on the book of V.A. Fock [25].
(2) In my investigations I used the C-style and was consecutive in my research. The logical consistence of a theory was more important for me, than its practical results.
(3) In all time of my investigation activity I had not in reality any scientific divisor, i.e. there were no sufficiently authoritative for me person, who would say me, that I should use the more pragmatic P-style instead of C-style, that I used.
(4) I was sufficiently self-confident, to follow the way, that I was led by the logic. I did not pay attention to other researchers, ignored all authorities and the circumstance, so that some researchers considered me as a scientific dissident.

I present the peculiarities, which seems for me to be necessary for overcoming of associative delusions. Each of this peculiarities appears rather rare. Appearance of all these peculiarities at one person is the more rarity.

To describe my research activity briefly, one should say, that *using C-style,* I *put consecutively into effect the idea of geometrization of physics.* and this agreed completely with general of the physics development in XIX – XX centuries.

12 Concluding remarks

Thus, the associative delusions (AD) accompanied the cognition process. Although one should tend to elimination of ADs, but, apparently, the complete elimination of them is impossible. In the case of impossibility of this elimination of ADs, AD leads to appearance of additional compensating hypotheses and to a construction of compensating (Ptolemaic) conceptions. Appearance of Ptolemaic conceptions leads to a generation of a special P-style of investigations, suitable for work with Ptolemaic conceptions. The P-style is simultaneously a style of investigations and a style of thinking. On one hand, the P-style is "noise-resistant" (suitable for work with Ptolemaic constructions, containing false suppositions), but on the other hand, it is less predictable, than C-style. In the course of some time one can pursue investigations, using P-style. But, thereafter the Ptolemaic conceptions stops to be effective. It becomes necessary, returning to C-style, to find and to overcome corresponding AD. If the P-style was existing for a long time and several generations of researchers had educated on its application, the overcoming of AD and returning to the C-style will be a difficult process. One needs to be ready to this.

After discovering AD the subsequent revision of existing theory may appear to be very essential. If it concerns the space-time geometry, the revision may lead even to a change of a world outlook. Transition from the space-time with the primordially deterministic particle motion to the space-time with the primordially stochastic motion is already a ground for a change of the world outlook. If earlier it was necessary to explain the stochasticity, starting from the determinism of the world, then now one should explain deterministic phenomena on the basis of primordial stochasticity of the world.

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