My Understanding of Stagnation in Foundation of Physics

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Abstract

Sabine Hossenfelder argues in her blog [1] that the present situation in foundation of physics should be called not crisis but stagnation. I argue that the main reason of the stagnation is that quantum theory inherited from classical one several notions which should not be present in quantum theory. In particular, quantum theory should not involve the notion of space-time background and, since nature is discrete and even finite, quantum theory should not be based on classical mathematics involving the notions of infinitely small/large and continuity. I discuss uncertainty relations, paradox with observation of stars, symmetry on quantum level, cosmological acceleration, gravity and particle theory. My main conclusion is that the most general quantum theory should be based on finite mathematics and, as a consequence: **Mathematics describing nature at the most fundamental level involves only a finite number of numbers while the notions of limit and infinitely small/large and the notions constructed from them (e.g. continuity, derivative and integral) are needed only in calculations describing nature approximately**.

Keywords: quantum theory, finite quantum theory, finite mathematics

List of abbreviations

AdS: anti-de Sitter

CC: cosmological constant

dS: de Sitter

FQT: finite quantum theory

GR: General Relativity

GWs: gravitational waves

IR: irreducible representation

NT: nonrelativistic theory

QFT: quantum field theory

RT: relativistic theory

WF: wave function

WPS: wave packet spreading

1 Introduction

Sabine Hossenfelder argues in her blog [1] that the present situation in foundation of physics should be called not crisis but stagnation. In particular, she writes: "Some have called it a crisis. But I don't think "crisis" describes the current situation well: Crisis is so optimistic. It raises the impression that theorists realized the error of their ways, that change is on the way, that they are waking up now and will abandon their flawed methodology. But I see no awakening. The self-reflection in the community is zero, zilch, nada, nichts, null. They just keep doing what they've been doing for 40 years, blathering about naturalness and multiverses and shifting their "predictions," once again, to the next larger particle collider."

In this note I discuss my understanding of the major reasons of this situation and, before discussing the reasons in greater details let me note the following. Gell-Mann taught quantum mechanics at Caltech and, according to his observation, there are three major stages in leaning this theory:

- A student solves the Schrödinger equation, finds energy levels etc., everything looks good, he is happy. This stage takes approximately half a year.
- He begins to think about the meaning of all that and is very disappointed that he cannot understand. This stage also takes approximately half a year.
- Once in the morning he wakes up and thinks: how stupid I was that suffered for so long time because there were no reasons to suffer and everything is clear.

The explanation is that he tried to explain quantum mechanics from the point of view of classical physics but this is impossible. However gradually his thinking became quantum and finally he understood that there were no problems.

When I look at thousands of papers on quantum theory the impression is that many authors did not have even stage 2, and their mentality is still classical. Of course quantum theory was a great revolution, and the founders were highly educated physicists. However, as discussed below, mentality of them was not fully quantum. As a result, quantum theory inherited from classical one several notions which are purely classical and which should not be in quantum theory. Below I consider several examples.

2 Status of the position operator in quantum theory

It has been postulated from the beginning of quantum theory that the coordinate and momentum representations of wave functions (WFs) are related to each other by the Fourier transform. One of the historical reasons was that in classical electrodynamics the coordinate and wave vector $\mathbf{k}$ representations are related analogously and we postulate that $\mathbf{p} = \hbar\mathbf{k}$ where $\mathbf{p}$ is the particle momentum. Then, although the interpretations of classical fields on one hand and WFs on the other are fully different, from mathematical point of view classical electrodynamics and quantum mechanics have much in common (and such a situation does not seem to be natural).

Another example follows. In classical electrodynamics, a wave packet moving even in empty space inevitably spreads out and this fact has been known for a long time. As a consequence of the similarity, a free quantum mechanical wave packet inevitably spreads out too. This effect is called wave packet spreading (WPS) and it is described in textbooks and many papers. In this section we argue that it plays a crucial role in drawing a conclusion on whether standard position operator is consistently defined.

The requirement that the momentum and position operators are related to each other by the Fourier transform is equivalent to standard commutation relations between these operators and to the Heisenberg uncertainty principle. A reason for choosing standard form of the position operator is described, for example, in the Dirac textbook [2]. Here Dirac argues that the momentum and position operators should be such that their commutator should be proportional to the corresponding classical Poisson bracket with the coefficient $i\hbar$. However, this argument is not convincing because only in very special cases the commutator of two physical operators is a c-number. One can check, for example, a case of momentum and position operators squared. In Ref. [3] Heisenberg argues in favor of his principle by considering *Gedankenexperiment* with Heisenberg's microscope. Since that time the problem has been investigated in many publications. A general opinion based on those investigations is that Heisenberg's arguments are problematic but the uncertainty principle is valid. However, a common assumption in those investigations is that one can consider uncertainty relations for all the components of the position and momentum operators independently. Below we argue that this assumption is not based on solid physical arguments.

Usual arguments in favor of choosing standard position and momentum operators are that these operators have correct properties in semiclassical approximation (see e.g. Ref. [4]). However, this requirement does not define the operator unambiguously. Indeed, if the operator B becomes zero in semiclassical limit then the operators A and $A + B$ have the same semiclassical limit.

At the beginning of quantum theory the WPS effect has been investigated by de Broglie, Darwin and Schrödinger. The fact that WPS is inevitable has been treated by several authors as unacceptable and as an indication that standard quantum theory should be modified. For example, de Broglie has proposed to describe a free particle not by the Schrödinger equation but by a wavelet which satisfies a nonlinear equation and does not spread out. At the same time, it has not been explicitly shown that numerical results on WPS are incompatible with experimental data. For example, it is known that for macroscopic bodies the effect of WPS is extremely small. Probably it is also believed that in experiments on the Earth with atoms and elementary particles spreading does not have enough time to manifest itself although we have not found an explicit statement on this problem in the literature. According to my observations, different physicists have different opinions on the role of WPS in different phenomena but in any case the absolute majority of physicists do not treat WPS as a drawback of the theory.

A natural problem arises what happens to photons which can travel from distant objects to Earth even for billions of years. This problem is discussed in detail in Ref. [5]. As shown here, standard theory predicts that, as a consequence of WPS, WFs of such photons will have the size of the order of light years or more. Does this contradict observations? As shown in Ref. [5], it does, and the reason of the paradox is that standard position operator is not consistently defined. Hence the inconsistent definition of the position operator is not only an academic problem but leads to the above paradox. A simple observation showing that standard definition of the position operator is not consistent follows.

Consider first a one-dimensional case. As argued in textbooks (see e.g. Ref. [4]), if the mean value of the x component of the momentum p_x is rather large, the definition of the coordinate operator $i\hbar\partial/\partial p_x$ can be justified but this definition does not have a physical meaning in situations when p_x is small or zero. This is clear even from the fact that if p_x is small then $exp(ip_x x/\hbar)$ is not a rapidly oscillating function of x.

Consider now the three-dimensional case. If all the components p_j ($j = 1,2,3$) are rather large then all the operators $i\hbar\partial/\partial p_j$ can have a physical meaning. A semiclassical WF $\chi(\mathbf{p})$ in momentum space should describe a narrow distribution around the mean value $\mathbf{p}_0$. Suppose now that coordinate axes are chosen such $\mathbf{p}_0$ is directed along the z axis. Then the mean values of the x and y components of the momentum operator equal zero and the operators $i\hbar\partial/\partial p_j$ cannot be physical for $j = 1,2$, i.e. in directions perpendicular to the particle momentum. The situation when a definition of an operator is physical or not depending on the choice of coordinate axes is not acceptable.

In summary, as shown in Ref. [5], as a consequence of inconsistent definition of the position operator, there arise paradoxes in observations of stars and in other phenomena. In this reference we have proposed a consistent definition of the position operator such that there is no WPS in directions perpendicular to the photon momentum and the paradoxes are resolved.

3 Remarks on the Schrödinger and Dirac equations

The Schrödinger and Dirac equations play an important role in the presentation of quantum field theory (QFT). In particular, the fact that those equations are in good agreement with experimental data is treated as an additional argument in favor of standard choice of the position operator.

Historically these equations have been first written in coordinate space and in textbooks they are still discussed in this form. The equations have played a great role for constructing quantum theory. However, a problem arises whether the equations are so fundamental as usually believed.

In textbooks on quantum mechanics the Schrödinger equation is discussed for different model potentials. However, the only case when this equation has been unambiguously confirmed by experimental data is the case of light atoms and especially the case of energy levels of the hydrogen atom. The successful description of those levels has been immediately treated as a great success of quantum theory.

This equation is nonrelativistic and describes the energy levels with a high accuracy because the electron in the hydrogen atom is nonrelativistic. Typical velocities of the electron in the hydrogen atom are of the order of αc where $\alpha \approx 1/137$ is the fine structure constant.

The Dirac equation for the electron in the hydrogen atom describes the fine structure of the energy levels: each Schrödinger energy level (which depends on α as α^2) splits such that the differences of fine structure energy levels for the given Schrödinger energy level are proportional to α^4. For these calculations the Dirac equation should be considered in the approximation $(v/c)^2$ while in the higher order approximation the validity of the equation is problematic for several reasons.

The field functions describing solutions of local single-particle equations do not have probabilistic interpretation. As shown by Pauli [6] (see also textbooks on QFT, e.g. Chap. 2 in Ref. [7]), in the case of fields with an integer spin there is no invariant subspace where the spectrum of the charge operator has a definite sign while in the case of fields with a half-integer spin there is no invariant subspace where the spectrum of the energy operator has a definite sign. In particular, in approximations higher than $(v/c)^2$ it is necessary to take into account solutions of the Dirac equation with negative energies. The reason is that, from mathematical point of view, a local quantum field is described by a reducible representation induced not from the little algebra irreducible representations (IRs) are induced from but from the Lorenz algebra. The local fields depend on x because the factor space of the Poincare group over the Lorentz group is Minkowski space. In that case there is no physical operator corresponding to x, i.e. x is not measurable.

As a consequence, the Dirac equation for the hydrogen energy levels is not exact. For example, the Lamb shift results in the additional splitting of fine structure energy levels such that the differences between energy levels within one fine structure energy level are proportional to α^5. The Lamb shift cannot be calculated in the single-particle approximation and can be calculated only in the framework of quantum electrodynamics (QED) which is treated as a fundamental theory describing electromagnetic interactions on quantum level.

From the point of view of the present knowledge, the Schrödinger and Dirac equations should be treated as follows. As follows from Feynman diagrams for the one-photon exchange, in the approximation up to $(v/c)^2$ the electron in the hydrogen atom can be described in the potential formalism where the potential acts on the WF in momentum space. So for calculating energy levels one should solve the eigenvalue problem for the Hamiltonian with this potential. This is an integral equation which can be solved by different methods. One of the convenient methods is to apply the Fourier transform and get standard Schrödinger or Dirac equation in coordinate representation with the Coulomb potential. Hence the fact that the results for energy levels are in good agreement with experiment shows only that QED defines the potential correctly and *standard coordinate Schrödinger and Dirac equations are only convenient mathematical ways of solving the eigenvalue problem in the approximation up to* $(v/c)^2$. For this problem the physical meaning of the position operator is not important at all. One can consider other transformations of the original integral equation and define other position operators. The fact that for non-standard choices one might obtain something different from the Coulomb potential is not important on quantum level. On classical level the interaction between two charges can be described by the Coulomb potential but this does not imply that on quantum level the potential in coordinate representation should be necessarily Coulomb.

Let us now consider a hypothetical situation: consider a Universe in which the value of α is of the order of unity or greater. Then the energy levels cannot be calculated in perturbation theory, and it is not known (even if α is small) whether the perturbation series of QED converges or not. However, the logical structure of QED remains the same. At the same time, the single-particle approximation is not valid anymore and the Schrödinger and Dirac equations do not define the hydrogen energy levels even approximately. In other words, in this situation the application of those equations for calculating the hydrogen energy level does not have a physical meaning.

The fact that in our world the Schrödinger and Dirac equations describe the hydrogen energy level with a high accuracy, is usually treated as a strong argument that the coordinate and momentum representations should be related to each other by the Fourier transform. However, as follows from the above considerations, this fact takes place only because we are lucky that the value of α in our Universe is small. Therefore this argument is not physical and cannot be used.

4 Does quantum theory need the notions of space-time background and fields?

Quantum field theory (QFT) inherited the notion of space-time background from classical field theory. However, as follows from several considerations, this notion has a direct physical meaning only on classical level. One of the reasons is that in quantum theory neither time nor coordinates can be measured with the absolute accuracy (see a more detailed discussion below). In QED, QCD and electroweak theory the Lagrangian density depends on the four-vector x which is associated with a point in Minkowski space but this is only the integration parameter which is used in the intermediate stage. The goal of the theory is to construct the S-matrix and when the theory is already constructed one can forget about Minkowski space because no physical quantity depends on x. This is in the spirit of the Heisenberg S-matrix program according to which in relativistic quantum theory it is possible to describe only transitions of states from the infinite past when $t \to -\infty$ to the distant future when $t \to +\infty$.

Note that the fact that the S-matrix is the operator in momentum space does not exclude a possibility that in some situations it is possible to have a space-time description with some accuracy but not with absolute accuracy. First of all, the problem of time is one of the most important unsolved problems of quantum theory (see e.g. Ref. [8] and references therein), and time cannot be measured with the accuracy better than $10^{-18}s$. Also, in typical situations the position operator in momentum representation exists not only in the nonrelativistic case but in the relativistic case as well. In the latter case it is known, for example, as the Newton-Wigner position operator [9] or its modification (see e.g. Ref. [5]). As pointed out even in textbooks on quantum theory, the coordinate description of elementary particles can work only in some approximations. In particular, even in most favorable scenarios, for a massive particle with the mass m its coordinate cannot be measured with the accuracy better than the particle Compton wave length $\hbar/mc$ [10].

For illustration of the background problem consider classical electrodynamics but first let me note the following. In statistical theory we do not describe the state of each particle and work with effective quantities such as temperature, pressure etc. Those quantities apply not to each particle but only to the ensemble of particles.

Analogously, electromagnetic field consists of photons but on classical level the theory does not describe the state of each photon. Each photon, as an elementary particle, is fully described by its momentum and helicity, and the notion of the photon electromagnetic field has no physical meaning. The classical electromagnetic fields $\mathbf{E}(\mathbf{r},t)$ and $\mathbf{B}(\mathbf{r},t)$ have the physical meaning only for systems of many photons. They describe the effective contribution of all photons at the point $x=(\mathbf{r},t)$ of Minkowski space, and in classical (non-quantum) theory it is assumed that the parameters $(\mathbf{r},t)$ can be measured with any desired accuracy.

On quantum level a problem arises how to define the photon coordinate wave function. For example, a section in the known textbook [7] is titled "Impossibility of introducing the photon wave function in coordinate representation". On the other hand, a detailed discussion of the photon position operator in Ref. [11] and references therein indicates that it is possible to define the photon coordinate wave function $\psi(\mathbf{r},t)$ but the description with such a wave function can have a good accuracy only in semiclassical approximation (see also Ref. [5]), and coordinates cannot be directly measured with the accuracy better than the size of the hydrogen atom.

In particle physics distances are never measured directly, and the phrase that the physics of some process is defined by characteristic distances l means only that if q is a characteristic momentum transfer in this process then $l=\hbar/q$. This conclusion is based on the assumption that coordinate and momentum representations in quantum theory are related to each other by the Fourier transform. However, as discussed in the preceding sections, this assumption is based neither on strong theoretical arguments nor on experimental data.

In contrast to classical field theory, QFT describes each particle in the field explicitly. It works with local quantized field operators $\varphi(x)$. As noted in the preceding section, solutions of local relativistic single-particle equations do not have exact probabilistic interpretation; in particular, probabilistic interpretation of the

Dirac equation is valid only in the approximation $(v/c)^2$. However, the argument x in the operators $\varphi(x)$ does not have a physical meaning at all for the following reasons.

Such operators act in the Fock space of the system under consideration. The elements of the space are secondly quantized WFs for all particles in the field. In the approximation when position operator works with a good accuracy, each particle is described by its own coordinates. But the quantity x in $\varphi(x)$ is not related to any particle, this is only a formal parameter. One of the principles of quantum theory is that any physical quantity should be described by an operator. However, since x is not related to any particle, there is no operator related to x. Therefore x cannot be directly measured and $\varphi(x)$ does not have a direct physical meaning. Strictly speaking, even the word "local" here might be misleading since x is not related to any particle.

As noted above, in classical electrodynamics the notions of background space-time and electromagnetic fields are needed for describing the effective contribution of all photons. However, in quantum theory it is possible to describe each photon separately, and, as noted above, each photon is described by its own momentum and helicity. So on quantum level the notions of background space-time and electromagnetic fields have no physical meaning and even the term "quantum field" is not quite consistent.

Foundational problems of QFT have been discussed by many authors. One of the main problems in substantiating QFT is that QFT contains products of interacting local quantized fields at the same points. As explained in textbooks (e.g. in the book [12]), such fields can be treated only as distributions, and the product of distributions at the same point is not a correct mathematical operation. As a consequence, in QFT there are divergences and other inconsistencies. It is rather strange that many physicists believe that such products are needed to preserve locality: they have nothing to do with locality because x is not a physical quantity.

As stated in the introductory section of the textbook [10], local quantum fields and Lagrangians are rudimentary notions which will disappear in the ultimate quantum theory. My observation is that now physicists usually do not believe that such words could be written in such a known textbook. The reason is that in view of successes of QCD and electroweak theory for explaining experimental data those ideas have become almost forgotten. However, although the successes are rather impressive, they do not contribute to resolving inconsistencies in QFT. Also, in the textbook [12] devoted to mathematical aspects of QFT, products of interacting quantum local fields are never used.

In QED one can formally define the operators $\mathbf{E}(x)$ and $\mathbf{B}(x)$ which are local quantized field operators acting in the Fock space for the quantum electromagnetic field. However, since x is not related to any photon, those operators do not define observable physical quantities. Those operators are used in theory such that integrals of their combinations over space-like hypersurfaces of Minkowski space define the energy-momentum and angular momentum operators of the electromagnetic field. So the situation is similar to that mentioned above when x in the Lagrangian density is only the integration parameter.

For illustration of the foundational problems of QFT, consider a photon emitted in the famous $21cm$ transition line between the hyperfine energy levels of the hydrogen atom. The phrase that the lifetime of this transition is of the order of $\tau = 10^7$ years is understood such that the width of the level is of the order of $\hbar/\tau$ i.e. the uncertainty of the photon energy is $\hbar/\tau$. In this situation a description of the system (atom + electric field) by the wave function (e.g. in the Fock space) depending on a continuous parameter t has no physical meaning (since roughly speaking the quantum of time in this process is of the order of 10^7 years).

In summary, QFT is not a consistent physical theory for several reasons:

- Local field operators $\varphi(x)$ in QFT have no physical meaning because x is not related to any particle.
- No physical quantity depends on x.
- Product of local field operators at the same point is an incorrect mathematical operation.
- The postulate that the coordinate and momentum representations are related to each other by the Fourier transform cannot be universal because such a relation is not valid in all situations.

As noted above, the goal of QFT is to construct the S-matrix in momentum representation and, after the S-matrix has been constructed one can forget about Minkowski space and local fields. It is amazing that, in spite of the above inconsistencies, QFT is in agreement with some experimental data with an unprecendented accuracy. Probably, the most famous QFT result is that for the electron and muon magnetic moments the theory gives at least eight correct digits. At the same time, this result has been obtained in the third order of perturbation theory in α, and there is no agreement with the data in higher order perturbation theory. In particular, it is not known whether perturbation series in QFT are convergent, divergent or asymptotic even if the interaction constant is small, and therefore QFT cannot solve the bound state problem.

Nevertheless, as a consequence of successes of QFT, mentality of many physicists is that agreement with the data is much more important than consistency. Although the most striking results of QFT were obtained more than 70 years ago, and none of the above inconsistencies has been resolved, many physicists still believe that fundamental problems of quantum theory (e.g. constructing quantum theory of gravity, constructing S-matrix beyond perturbation theory etc.) will be resolved in the framework of QFT or its generalizations e.g. string theory. One of its ideas is that if products of fields at the same points (zero-dimensional objects) are replaced by products where the arguments of the fields belong to strings (one-dimensional objects) then there is hope that infinities will be less singular. However, the mathematical inconsistency similar to that mentioned above exists in string theory as well and here the problem of infinities has not been solved yet.

Although in QFT the notion of space-time background has no physical meaning, M theory or string theory are based on this notion even in greater extents than QFT. Here physics depends on topology of continuous and differentiable manifolds at Planck distances $l_P \approx 10^{-35}m$. The corresponding value of q is $q \approx 10^{19}Gev/c$, i.e. much greater than the momenta which can be achieved at modern accelerators. Nevertheless, the above theories are initially formulated in coordinate representation by using continuous mathematics. However, as noted in Sec. 10, geometry and topology has a physical meaning only on classical level when sizes of atoms and elementary particles can be neglected.

5 Symmetry in quantum theory

In relativistic quantum theory the usual approach to symmetry on quantum level follows. Since the Poincare group is the group of motions of Minkowski space, quantum states should be described by representations of this group. This implies that the representation generators acting in the Hilbert space of the system under consideration commute according to the commutation relations of the Poincare group Lie algebra:

$$[P^\mu, P^\nu] = 0, \quad [P^\mu, M^{\nu\rho}] = -i(\eta^{\mu\rho}P^\nu - \eta^{\mu\nu}P^\rho),$$
$$[M^{\mu\nu}, M^{\rho\sigma}] = -i(\eta^{\mu\rho}M^{\nu\sigma} + \eta^{\nu\sigma}M^{\mu\rho} - \eta^{\mu\sigma}M^{\nu\rho} - \eta^{\nu\rho}M^{\mu\sigma}) \tag{1}$$

where $\mu, \nu = 0, 1, 2, 3$, P^μ are the operators of the four-momentum and $M^{\mu\nu}$ are the operators of Lorentz angular momenta. This approach is in the spirit of Klein's Erlangen Program in mathematics.

However, as noted above, background space-time has a physical meaning only on classical level, and in quantum theory neither time nor coordinates can be measured with the absolute accuracy. For those reasons, as argued in Ref. [13], the approach to symmetry should be the opposite to that proceeding from the Erlangen Program. Each system is described by a set of linearly independent operators. By definition, the rules how they commute with each other define the symmetry algebra. In particular, *by definition*, Poincare symmetry on quantum level means that the operators commute according to Eq. (1). This definition does not involve Minkowski space at all.

Such a definition of symmetry on quantum level has been proposed in Ref. [14] and in subsequent publications of those authors. I am very grateful to Leonid Avksent'evich Kondratyuk for explaining me this definition during our collaboration. I believe that this replacement of standard paradigm is fundamental for understanding quantum theory, and I did not succeed in finding a similar idea in the literature.

It is sometimes stated that the expressions in Eq. (1) are written in the system of units $c = \hbar = 1$. However, in relativistic quantum theory itself those quantities are not needed. As explained in Refs. [15, 16], $\hbar$ is needed only for transition to classical (i.e. non-quantum) theory because here angular momenta have the dimension $kg \cdot m^2/s$, and c is needed only for transition to nonrelativistic theory because here velocities have the dimension m/s. Then classical theory is a special degenerate case of quantum one in the formal limit $\hbar \to 0$, and nonrelativistic theory is a special degenerate case of relativistic one in the formal limit $c \to \infty$. However, relativistic quantum theory still depends on systems of units because the operators P^μ have the dimension $length^{-1}$.

By analogy with the definition of Poincare symmetry on quantum level, the definition of de Sitter (dS) symmetry on quantum level should not involve the fact that the dS group is the group of motions of dS space. Instead, *the definition* is that the operators M^{ab} $(a, b = 0, 1, 2, 3, 4,\ M^{ab} = -M^{ba})$ describing the system under consideration satisfy the commutation relations *of the dS Lie algebra* so(1,4), *i.e.*

$$[M^{ab}, M^{cd}] = -i(\eta^{ac}M^{bd} + \eta^{bd}M^{ac} - \eta^{ad}M^{bc} - \eta^{bc}M^{ad}) \tag{2}$$

where η^{ab} is the diagonal metric tensor such that $\eta^{00} = -\eta^{11} = -\eta^{22} = -\eta^{33} = -\eta^{44} = 1$. The *definition* of anti-de Sitter (AdS) symmetry on quantum level is given by the same equations but $\eta^{44} = 1$.

With such a definition of symmetry on quantum level, dS and AdS symmetries are more natural than Poincare symmetry. In the dS and AdS cases all the ten representation operators of the symmetry algebra are angular momenta while in the Poincare case only six of them are angular momenta and the remaining four operators represent standard energy and momentum. In the representation (2) all the operators are dimensionless, and the theory does not depend on the system of units. If we *define* the operators P^μ as $P^\mu = M^{4\mu}/R$ where R is a parameter with the dimension *length* then in the formal limit when $R \to \infty$, $M^{4\mu} \to \infty$ but the quantities P^μ are finite, Eqs. (2) become Eqs. (1). This procedure is called contraction and in the given case it is the same for the dS or AdS symmetries.

However, as shown in Refs. [15, 16], dS and AdS theories are not only more natural than relativistic quantum theory but more general: the latter is a special degenerate case of dS and AdS theories in the formal limit $R \to \infty$. Note that the operators in Eq. (2) do not depend on R at all. This quantity is needed only for transition from dS quantum theory to Poincare quantum theory. Although R has the dimension *length*, it has nothing to do with the radius of the background space which, as noted above, has a direct physical meaning only in classical theory: R is simply the coefficient of proportionality between the operators $M^{4\mu}$ and P^μ.

The question why the quantities $(c, \hbar, R)$ are as are does not arise because the answer is: c is as is because people's choice is to measure velocities in m/s, $\hbar$ is as is because people's choice is to measure angular momenta in $kg \cdot m^2/s$, and R is as is because people's choice is to measure distances in meters.

The fact that dS and AdS quantum theories are more general than Poincare quantum theory has been first given in the famous Dyson's paper [17] where symmetries were treated in terms of Lie groups rather than Lie algebras. This paper appeared in 1972 and, in view of Dyson's results, a question arises why general theories of elementary particles (QED, electroweak theory and QCD) are still based on Poincare symmetry and not dS or AdS symmetries. Probably "a justification" is that since the parameter of contraction R from dS or AdS theories to Poincare one is much greater that sizes of elementary particles, there is no need to use the former symmetries for description of elementary particles.

We believe that this argument is not consistent because usually more general theories shed a new light on standard concepts. For example, as shown in Refs. [18, 19], in contrast to the situation in Poincare invariant theories, where a particle and its antiparticle are described by different IRs of the Poincare algebra (or group), in dS theory a particle and its antiparticle belong to the same IR of the dS algebra. In the formal limit $R \to \infty$ one IR of the dS algebra splits into two *different* IRs of the Poincare algebra for a particle and its antiparticle. Strictly speaking, this implies that in dS theory the very notion of a particle and its antiparticle is only approximate since transitions particle$\leftrightarrow$antiparticle are not strictly prohibited. As a consequence, in dS theory the electric charge and the baryon and lepton quantum numbers are only approximately conserved. At present they are conserved with a high accuracy. However, one might think that at early stages of the Universe the quantity R was much less than now and the nonconservation of those quantum numbers was much stronger. This might be a reason of the known phenomenon of baryon asymmetry of the Universe.

We conclude that the situation with Poincare theory vs. dS and AdS theories is in the trend of modern physics that description of experimental data is more important than consistency. Of course, reformulation of fundamental particle theories in terms of dS or AdS symmetry is not a simple problem but without such a reformulation a breakthrough in particle physics in unrealistic.

6 Is the notion of interaction physical?

The fact that problems of quantum theory arise as a result of describing interactions in terms of local quantum fields poses the following dilemma. One can either modify the description of interactions or investigate whether the notion of interaction is needed at all. A reader might immediately conclude that the second option fully contradicts the existing knowledge and should be rejected right away. In this and subsequent sections I discuss whether cosmological repulsion and gravity might be not interactions but simply kinematical manifestation of dS symmetry on quantum level.

Let us consider an isolated system of two particles and pose a question of whether they interact or not. In theoretical physics there is no unambiguous criterion for answering this question. For example, in classical nonrelativistic and relativistic mechanics the criterion is clear and simple: if the relative acceleration of the particles is zero they do not interact, otherwise they interact. However, those theories are based on Galilei and Poincare symmetries, respectively and there is no reason to believe that such symmetries are exact symmetries of nature.

In quantum mechanics the criterion can be as follows. If E is the energy operator of the two-particle system and E_i $(i = 1, 2)$ is the energy operator of particle i then one can formally define the interaction operator U such that

$$E = E_1 + E_2 + U \tag{3}$$

Therefore the criterion can be such that the particles do not interact if $U = 0$, i.e. $E = E_1 + E_2$.

In QFT the criterion is also clear: the particles interact if they can exchange by virtual quanta of some fields. For example, the electromagnetic interaction between the particles means that they can exchange by virtual photons, the gravitational interaction - that they can exchange by virtual gravitons etc. In that case U in Eq. (3) is an effective operator obtained in the approximation when all degrees of freedom except those corresponding to the given particles can be integrated out.

A problem with approaches based on Eq. (3) is that the answer should be given in terms of invariant quantities while energies are reference frame dependent. Therefore one should consider the two-particle mass operator. In standard Poincare invariant theory the free mass operator is given by $M = M_0(\mathbf{q}) = (m_1^2+\mathbf{q}^2)^{1/2}+(m_2^2+\mathbf{q}^2)^{1/2}$ where the m_i are the particle masses and $\mathbf{q}$ is the relative momentum operator. In semiclassical approximation $\mathbf{q}$ becomes the relative momentum and M_0 becomes a function of $\mathbf{q}$ not depending on the relative distance r between the particles. Therefore the relative acceleration is zero and this case can be treated as noninteracting.

Consider now a two-particle system in dS invariant theory. As explained in the preceding section, on quantum level the only consistent definition of dS invariance is that the operators describing the system satisfy the commutation relations of the dS algebra. This definition does not involve GR, QFT, dS space and its geometry. Then a definition of an elementary particle is that the particle is described by an IR of the dS algebra. Therefore a possible definition of the free two-particle system can be such that the system is described by a representation where not only the energy but all other operators are given by sums of the corresponding single-particle operators. In representation theory such a representation is called the tensor products of IRs.

In other words, we consider only quantum mechanics of two free particles in dS invariant theory. In that case, as shown in Ref. [20], the two-particle mass operator can be explicitly calculated. It can be written as $M = M_0(\mathbf{q}) + V$ where V is an operator depending not only on $\mathbf{q}$. In semiclassical approximation V becomes a function depending also on the distance between the particles. As a consequence, the relative acceleration is not zero and, from the point of view of standard theory, the particle interact with each other.

In quantum theory, dS and AdS symmetries are widely used for investigating QFT in curved space-time background. However, it seems rather paradoxical that such a simple case as a free two-body system in dS invariant theory has not been widely discussed. According to my observations, such a situation is a manifestation of the fact that even physicists working on dS QFT are not familiar with basic facts about IRs of the dS algebra. It is difficult to imagine how standard Poincare invariant quantum theory can be constructed without involving known results on IRs of the Poincare algebra. Therefore it is reasonable to think that when Poincare invariance is replaced by dS one, IRs of the Poincare algebra should be replaced by IRs of the dS algebra. However, physicists working on QFT in curved space-time say that fields are more fundamental than particles and therefore there is no need to involve commutation relations (2) and IRs. In other words, they treat dS symmetry on quantum level not such that the relations (2) should be valid but such that quantum fields are constructed on dS space.

Our discussion shows that the notion of interaction depends on symmetry. For example, when we consider a system of two particles which, from the point of view of dS symmetry are free (since they are described by a tensor product of IRs), from the point of view of our experience based on Galilei or Poincare symmetries they are not free since their relative acceleration is not zero. This poses a question whether known interactions are in fact not interactions but effective interactions emerging when a higher symmetry is treated in terms of a lower one.

In particular, is it possible that quantum symmetry is such that on classical level the relative acceleration of two free particles is described by the same expression as that given by the Newton gravitational law and corrections to it? This possibility has been first discussed in Ref. [21]. It is clear that this possibility is not in mainstream according to which gravity is a manifestation of the graviton exchange. I believe that until the nature of gravity has been unambiguously understood, different possibilities should be investigated. A strong argument in favor of my approach is as follows. In contrast to theories based on Poincare and AdS symmetries, in the dS case the spectrum of the free mass operator is not bounded below by $(m_1 + m_2)$. As a consequence, it is not a problem to indicate states where the mean value of the mass operator has an additional contribution $-Gm_1m_2/r$ with possible corrections. In this approach G is not a fundamental external parameter but should be calculated. A problem is to understand reasons why macroscopic bodies have such WFs.

From the point of view of dS symmetry on quantum level, G cannot be a fundamental constant from the following considerations. The commutation relations (2) do not depend on any free parameters. One might say that this is a consequence of the choice of units where $\hbar = c = 1$. However, as noted in the preceding sections, any fundamental theory should not involve the quantities $\hbar$ and c. A theory based on the above definition of the dS symmetry on quantum level cannot involve quantities which are dimensionful in units $\hbar = c = 1$. In particular, we inevitably come to the conclusion that the gravitational and cosmological constants cannot be fundamental.

By analogy with the above discussion about gravity, one can pose a question of whether the notions of other interactions are fundamental or not. In QFT all interactions are introduced according to the same scheme. One writes the Lagrangian as a sum of free and interaction Lagrangians. The latter are proportional to interaction constants which cannot be calculated from the theory and hence can be treated only as phenomenological parameters. It is reasonable to believe that the future fundamental theory will not involve such parameters.

7 Dark energy, cosmological constant problems and inflation

Known historical facts are that first Einstein included the cosmological constant (CC) Λ because he believed that the Universe should be stationary, and this is possible only if $\Lambda \neq 0$. However, according to Gamow, after Friedman's results and Hubble's discovery of the Universe expansion, Einstein changed his mind and said that inclusion of Λ was the biggest blunder of his life.

The usual philosophy of GR is that curvature is created by matter and therefore Λ should be equal to zero. This philosophy had been advocated even in standard textbooks written before 1998. For example, the authors of Ref. [22] say that "...there are no convincing reasons, observational and theoretical, for introducing a nonzero value of Λ" and that "... introducing to the density of the Lagrange function a constant term which does not depend on the field state would mean attributing to space-time a principally ineradicable curvature which is related neither to matter nor to gravitational waves". As a consequence, many physicists accept the dogma that since curvature is created by bodies then empty space should be flat. However, the notion of empty space is physically meaningless because nothing can be measured in a space which exists only in our imagination.

The data of Ref. [23] on supernovae have shown that $\Lambda > 0$ with the accuracy better than 5%, and further investigations have improved the accuracy to 1%. For reconciling this fact with the philosophy of GR and the above dogma, the terms with Λ in the left-hand-sides of the Einstein equations have been moved to the right-hand-sides and interpreted not as the curvature of empty space-time but as a contribution of unknown matter called dark energy. Then, as follows from the experimental value of Λ, dark energy contains approximately 70% of the energy of the Universe. At present a possible nature of dark energy is discussed in a vast literature and several experiments have been proposed.

Let us note the following. In the formalism of GR the coordinates and curvature are needed for the description of real bodies. One of fundamental principles of physics is that definition of a physical quantity is the description on how this quantity should be measured. In the Copenhagen formulation of quantum theory measurement is an interaction with a classical object. Therefore since in empty space-time nothing can be measured, the coordinates and curvature of empty space-time have no physical meaning. This poses a problem whether the formal limit of GR when matter disappears but space-time remains is physical.

As noted in the preceding section, as a consequence of dS symmetry, in the system of two free particles an effective interaction arises. Let us discuss how this fact can be used for explaining the cosmological acceleration.

We will assume that distances between the bodies are much greater than their sizes, and the bodies do not have anomalously large internal angular momenta. Then, from the formal point of view, the motion of two bodies as a whole can be described by the same formulas as the motion of two elementary particles with zero spin. In quantum dS theory elementary particles are described by IRs of the dS algebra and, as shown in Refs. [18, 19], one can explicitly construct such IRs. The representation describing a system of noninteracting particles is a tensor product of single-particle representation and therefore we have a complete description of the system of free particles.

At this stage we do not have any coordinate space yet. However, if we want to describe our system in semiclassical approximation, we have to introduce the position operator. As noted in Sec. 2, there is no universal choice of position operator valid in all situations. As noted in this section, the choice of standard position operation $i\hbar\partial/\partial\mathbf{p}$ for photons from stars results in WPS and contradicts the data on observation of stars. It has been also noted that for macroscopic bodies the effect of WPS is negligible. For this reason, for describing cosmological acceleration we choose the standard form of the position operator.

Then, as shown in Refs. [18, 19], the two-body nonrelativistic mass operator equals

$$M = M(\mathbf{q},\mathbf{r}) = m_1 + m_2 + H_{nr}(\mathbf{r},\mathbf{q}), \quad H_{nr}(\mathbf{r},\mathbf{q}) = \frac{\mathbf{q}^2}{2m_{12}} - \frac{m_{12}c^2\mathbf{r}^2}{2R^2} \tag{4}$$

where m_1 and m_2 are the masses of the particles, m_{12} is the reduced two-particle mass and $\mathbf{q}$ is the relative momentum. Here the operator M acts in the space of internal WFs $\chi(\mathbf{q})$ such that $\int |\chi(\mathbf{q})|^2 d^3\mathbf{q} < \infty$ and $\mathbf{r}$ acts in this space as $\mathbf{r} = i\hbar\partial/\partial\mathbf{q}$. In semiclassical approximation $\mathbf{r}$ becomes the vector of the relative distance. We see that the last term is the dS correction to the standard nonrelativistic mass operator, and this correction disappears in the formal limit $R \to \infty$.

As follows from the Hamilton equations, in semiclassical approximation

$$\mathbf{a} = \mathbf{r}c^2/R^2 \tag{5}$$

where $\mathbf{a}$ and $\mathbf{r}$ are the relative acceleration and radius vector, respectively. Since R is very large, the relative acceleration is not negligible only at cosmological distances when $|\mathbf{r}|$ is of the order of R. This result coincides with the result of GR for the cosmological acceleration if $\Lambda = 3/R^2$. Therefore the fact that the cosmological acceleration is nonzero has nothing to do with the choice of background space and with existence or nonexistence of dark energy. We believe that our result is more general than the result of GR because any classical result should be a consequence of quantum theory in semiclassical approximation. So for the explanation of cosmological acceleration dark energy is not needed and the cosmological constant problem does not arise.

In the literature the notion of the $c\hbar G$ cube of physical theories is sometimes used. The meaning is that any relativistic theory should contain c, any quantum theory should contain $\hbar$ and any gravitation theory should contain G. The more fundamental a theory is the greater number of those parameters it contains. In particular, relativistic quantum theory of gravity is treated as the most fundamental because it contains all the three parameters c, $\hbar$ and G while nonrelativistic classical theory without gravitation is the least fundamental because it contains none of those parameters.

The history of GR is described in a vast literature. The Lagrangian of GR is linear in Riemannian curvature R_c, but from the point of view of symmetry requirements there exist infinitely many Lagrangians satisfying such requirements. For example, $f(R_c)$ theories of gravity are widely discussed, where there can be many possibilities for choosing the function f. Then the effective gravitational constant G_{eff} can considerably differ from standard gravitational constant G. It is also argued that GR is a low energy approximation of more general theories involving higher order derivatives. The nature of gravity on quantum level is a problem, and standard canonical quantum gravity is not renormalizable. For those reasons the quantity G can be treated only as a phenomenological parameter but not fundamental one.

Therefore the quantity G is not fundamental and, as follows from the above discussion, the set of parameters $(c, \hbar, R)$ is more adequate than the set $(c, \hbar, G)$. In addition, as explained in the preceding section, in contrast to usual statements, relativistic theory should not contain c, quantum theory should not contain $\hbar$ and dS or AdS theories should not contain R. Those three parameters are needed only for transitions from more general theories to less general ones. The most general dS and AdS quantum theories do not contain dimensionful quantities at all while the least general nonrelativistic classical theory contains three dimensionful quantities (kg, m, s).

Nevertheless, physicists usually believe that the quantities $(c, \hbar)$ are fundamental and do not change over time. This belief has been implemented in the modern system of units where the basic quantities are not (kg, m, s) but $(c, \hbar, s)$ and it is postulated that the quantities $(c, \hbar)$ do not change over time. By definition, it is postulated that from now on $c = 299792458m/s$ and $\hbar = 1.054571800 \cdot 10^{-34} kg \cdot m^2/s$. As a consequence, now the quantities (kg, m) are not basic ones because they can be expressed in terms of $(c, \hbar, s)$ while second remains the basic quantity.

The motivation for the modern system of units is based on several facts of quantum theory based on Poincare invariance. First of all, since it is postulated that the photon is massless, its speed c is always the same for any photons with any energies. Another postulate is that for any photon its energy is always proportional to its frequency and the coefficient of proportionality always equals $\hbar$. Let us note that this terminology might be misleading for the following reasons. Since the photon is the massless elementary particle, it is characterized only by its momentum and helicity and *is not characterized* by frequency and wave length. The latter are only classical notions characterizing a classical electromagnetic wave containing many photons. Quantum theory predicts the energy distribution of photons in blackbody radiation but experimentally we cannot follow individual photons and can measure only the frequency distribution in the radiation. Then the theory agrees with experiment if formally the photon with the energy E is attributed the frequency $\omega = E/\hbar$.

A typical theoretical justification is that the photon WF contains $exp(-iEt/\hbar)$. This agrees with the facts that in classical approximation the Schrödinger equation becomes the Hamilton-Jacobi equations and that with such a dependence of the WF on time one can describe trajectories of photons in classical approximation (see e.g. the discussion in Ref. [5]). At the same time, there is no experimental proof that this dependence takes place on quantum level and, as noted above, fundamental quantum theories proceed from the Heisenberg S-matrix program that in quantum theory one can describe only transitions of states from the infinite past when $t \to -\infty$ to the distant future when $t \to +\infty$.

Consider first IRs of the AdS algebra. For the first time the construction of such IRs has been given by Evans [24] (see also Ref. [25]). As noted above, the AdS analog of the energy operator is M^{04}. A common feature of the AdS and Poincare cases is that there are IRs containing either only positive or only negative energies and the latter can be associated with antiparticles. In the AdS case the minimum value of the energy in IRs with positive energies can be treated as the mass by analogy with the Poincare case. However, the essential difference between the AdS and Poincare cases is that IRs in the former belong to the discrete series of IRs and the photon mass cannot be exactly zero. In the AdS analog of massless Poincare IR, the AdS mass equals $m_{AdS} = 1 + s$ where s is the spin. From the point of view of Poincare symmetry, this is an extremely small quantity since the Poincare mass m equals m_{AdS}/R. However, since m_{AdS} is not exactly zero, there is a nonzero probability that the photon can be even in the rest state, i.e. its speed will be zero. In general, the speed of the photon can be in the range $[0, 1)$. Therefore, in contrast to Poincare case, there is no situation when all photons with all energies have the same speed. As a consequence, the constant c does not have the fundamental meaning as in Poincare theory.

As noted above, for IRs of the dS algebra the situation drastically differs from the Poincare case because there are no IRs with only positive and negative energies: one IR necessarily contains both positive and negative energies. Such IRs algebra are characterized by the dS mass m_{dS} such that m_{dS} cannot be zero and the relation between dS and Poincare masses is again $m_{dS} = Rm$. So even the photon is necessarily massive. In Poincare theory there is a discussion what is the upper bound for the photon mass and different authors give the values in the range $(10^{-17}ev, 10^{-14}ev)$. These seem to be extremely tiny quantities but even if $m = 10^{-17}ev$ and R is of the order of $10^{26}meters$ as usually accepted than m_{dS} is of the order of 10^{16}, i.e. a very large quantity. We conclude that in the dS case the quantity c cannot have a fundamental meaning, as well as in the AdS case.

Consider now whether the quantity $\hbar$ can be treated as fundamental in de Sitter invariant theories. For such theories it is not even clear how to define energy and time such that the WF depends on time as $exp(-iEt/\hbar)$ even in classical approximation. For example, in the dS case the operator M^{04} is on the same footing as the operators M^{0j} $(j = 1, 2, 3)$ and only in Poincare limit it becomes the energy operator.

While in the modern system of units, c and $\hbar$ are treated as exact quantities, second is treated only as an approximate quantity. Since there is no time operator, it is not even legitimate to say whether time should be discrete or continuous. The second is defined as the duration of 9192631770 periods of the radiation corresponding to the transition between the two hyperfine levels of the ground state of the cesium 133 atom. The physical quantity describing the transition is the transition energy ΔE, and the frequency of the radiation is defined as $\Delta E/\hbar$. The transition energy cannot be the exact quantity because the width of the transition energies cannot be zero. In addition, the transition energy depends on gravitational and electromagnetic fields and on other phenomena. In view of all those phenomena the accuracy of one second given in the literature is in the range $(10^{-18}s, 10^{-16}s)$, and the better accuracy cannot be obtained in principle. In summary, "continuous time" is a part of classical notion of space-time continuum and makes no sense beyond this notion.

In modern inflationary models the inflation period of the Universe lasted in the range $(10^{-36}s, 10^{-32}s)$ after the Big Bang. In addition to the fact that such times cannot be measured in principle, at this stage of the Universe there were no nuclei and atoms and so it is unclear whether time can be defined at all. The philosophy of classical physics is that any physical quantity can be measured with any desired accuracy. However the state of the Universe at that time could not be classical, and in quantum theory the definition of any physical quantity is a description how this quantity can be measured, at least in principle. In quantum theory it is not acceptable to say that "in fact" some quantity exists but cannot be measured. So description of the inflationary period by times $(10^{-36}s, 10^{-32}s)$ has no physical meaning.

In summary, since in dS and AdS theories all physical quantities are dimensionless, here no system of units is needed. Dimensionful quantities $(c, \hbar, s)$ are meaningful only at special conditions when Poincare symmetry works with a high accuracy and measurements can be performed in a system which is classical (i.e. non-quantum) with a high accuracy.

8 Gravity in General Relativity

In the mainstream literature on gravity it is stated that GR supersedes all alternative classical theories of gravity and sometimes even a question is posed whether Einstein was right on 100% or only on 99%. Let us discuss this question in greater details.

8.1 Three classical tests of GR

As seen from Earth, the precession of Mercury's orbit is measured to be 5600" per century while the contribution of GR is 43" per century. Hence the latter is less than 1% of the total contribution. The main contribution to the total precession arises as a consequence of the fact that Earth is not an inertial reference frame and when the precession is recalculated with respect to the International Celestial Reference System the value of the precession becomes (574.10 ± 0.65)" per century. Celestial mechanics states that the gravitational tugs of the other planets contribute (531.63 ± 0.69)" while all other contributions are small. Hence there is a discrepancy of 43" per century and the result of GR gives almost exactly the same value. Although there are different opinions on whether, the contribution of GR fully explains the data or not, in the overwhelming majority of the literature it is accepted that this is the case. Therefore the result of GR on the precession of Mercury's orbit is a great achievement of the theory.

The result of GR for the gravitational red shift of light is treated such that it has been confirmed in the Pound-Rebka experiment. However, the conventional interpretation of this effect has been criticized by Okun in Ref. [26]. In his opinion, "*a presumed analogy between a photon and a stone*" is wrong. The reason is that "*the energy of the photon and hence its frequency $\omega = E/\hbar$ do not depend on the distance from the gravitational body, because in the static case the gravitational potential does not depend on the time coordinate t. The reader who is not satisfied with this argument may look at Maxwell's equations as given e.g. in section 5.2 of ref. [27]. These equations with time independent metric have solutions with frequencies equal to those of the emitter*". In Ref. [26] the result of the Pound-Rebka experiment is explained such that not the photon loses its kinetic energy but the differences between the atomic energy levels on the height h are greater than on the Earth surface and "*As a result of this increase the energy of a photon emitted in a transition of an atom downstairs is not enough to excite a reverse transition upstairs. For the observer upstairs this looks like a redshift of the photon. Therefore for a competent observer the apparent redshift of the photon is a result of the blueshift of the clock.*".

As noted in Ref. [26], "*A naive (but obviously wrong!) way to derive the formula for the redshift is to ascribe to the photon with energy E a mass $m_\gamma = E/c^2$ and to apply to the photon a non-relativistic formula $\Delta E = -m_\gamma \Delta\phi$ treating it like a stone. Then the relative shift of photon energy is $\Delta E/E = -\Delta\phi/c^2$, which coincides with the correct result. But this coincidence cannot justify the absolutely thoughtless application of a nonrelativistic formula to an ultrarelativistic object.*"

However, as noted above, the notion of the photon electromagnetic field has no physical meaning. Maxwell's equations have the physical meaning only in classical theory. In QED not states but the secondly quantized operators $\mathbf{E}(x)$ and $\mathbf{B}(x)$ satisfy Maxwell's equations but, as noted above, the argument x in those operators has no physical meaning. A stone and a photon are simply particles with different masses; that is why the stone is nonrelativistic and the photon is ultrarelativistic. Therefore there is no reason to think that in contrast to the stone, the photon will not lose its kinetic energy. At the same time, we believe that Ref. [26] gives strong arguments that energy levels on the Earth surface and on the height h are different.

We believe that the following point in the arguments of Ref. [26] is not quite consistent. A stone, a photon and other particles can be characterized by their energies, momenta and other quantities for which there exist well defined operators. Those quantities might be measured in collisions of those particles with other particles. At the same time, as noted in the preceding section, the notions of "frequency of a photon" or "frequency of a stone" have no physical meaning. If a particle WF contains $exp[i(px - Et)/\hbar]$ then by analogy with the theory of classical waves one might say that the particle is a wave with the frequency $\omega = E/\hbar$ and the wave length $\lambda = 2\pi\hbar/p$. However, the fact that such defined quantities ω and λ are the real frequencies and wave lengths measured e.g. in spectroscopic experiments needs to be substantiated. Let ω and λ be frequencies and wave lengths measured in experiments with classical waves. Those quantities necessarily involve classical space and time. Then the relation $E = \hbar\omega$ between the energies of particles in classical waves and frequencies of those waves is only an assumption that those different quantities are related in such a way. This relation has been first proposed by Planck for the description of the blackbody radiation and the experimental data indicate that it is valid with a high accuracy. However, there is no guaranty that this relation is always valid with the absolute accuracy, as the author of Ref. [26] assumes. In spectroscopic experiments not energies and momenta of emitted photons are measured but wave lengths of the radiation obtained as a result of transitions between different energy levels. In particular, there is no experiment confirming that the relation $E = \hbar\omega$ is always exact, e.g. on the Earth surface and on the height h.

In summary, one may agree or disagree with Okun's conclusion that the result of the Pound-Rebka experiment is fully explained by the difference of atomic energy levels on the Earth and on the height h, but the difference should be taken into account. However, in the mainstream literature on this problem (e.g. in reviews on 100 years of GR) this problem even is not discussed, i.e. an implicit assumption is that the effect is small. Therefore the result of the experiment cannot be treated as a model-independent confirmation of GR.

Consider now the deflection of light by the Sun. This effect is described as $\theta = (1+\gamma)\cdot 0.875"$ where γ depends on the theory. The result with $\gamma = 0$ was first obtained by von Soldner in 1801 and confirmed by Einstein in 1911. The known historical facts are that in 1915 when Einstein created GR he obtained $\gamma = 1$ and in 1919 this result was confirmed in observations of the full Solar eclipse. Originally the accuracy of measurements was not high but now the quantity γ is measured with a high accuracy in experiments using the Very Long Base Interferometry (VLBI) technique and the result $\gamma = 1$ has been confirmed with the accuracy better than 1%.

The confirmation of this result is very difficult because corrections to the simple geometric picture of deflection should be investigated. For example, the density of the Solar atmosphere near the Solar surface is rather high and the assumption that the photon passes this atmosphere practically without interaction with the particles of the atmosphere seems to be problematic. In Ref. [28] the following corrections have been investigated at different radio-wave frequencies ω: the brightness distribution of the observed source, the Solar plasma correction, the Earth's atmosphere, the receiver instrumentation, and the difference in the atomic-clock readings at the two sites. All these corrections are essentially model dependent. For example, the plasma delay τ_{plas} has been approximated by $I(t)/\omega^2$ where $I(t)$ depends on the electronic content along the signal propagation paths to the two sites.

The majority of authors investigating the deflection state that the experimental data confirm the result $\gamma = 1$ but several authors (see e.g. Ref. [29]) disagree with this conclusion. It is also is not clear whether other effects might be important. In particular, a possible mechanism can be such that a photon is first absorbed by an atom and then is reemitted. Suppose that this mechanism plays an important role and photons encounter many atoms on their way. In the period of time when the atom absorbs the photon but does not reemit it yet, the atom acquires an acceleration as a result of its effective gravitational interaction with the Sun. Then the absorbed and reemitted photons will have different accelerations and the reemitted photon is expected to have a greater acceleration towards the Sun than the absorbed photon. This effect increases the deflection angle and analogously other mechanisms of interaction of photons with the interstellar matter are expected to increase the deflection angle since the matter moves with an acceleration towards the Sun.

8.2 Discussion of the problem of gravitational radiation

Three classical effects of GR are treated as phenomena where the gravitational field is weak because corrections to the Minkowskian metric are small. In recent years considerable efforts have been made for investigating phenomena where the gravitational field is treated as strong.

One of the examples is the case of binary pulsars. In contrast to planets, which are directly visible, conclusions about masses and radii of pulsars can be made only from models describing their radiation. It is believed that typically pulsars are neutron stars with masses in the range $(1.2-1.6)M_\odot$ and radii of the order of $10km$. In the case of binary pulsars, a typical situation is that the second component of the binary system is not observable (at present the only known case where the both components are pulsars is the binary pulsar J0737-3039).

The most famous case is the binary pulsar PSR B1913+16 discovered by Hulse and Taylor in 1974. A model with 18 fitted parameters for this binary system has been described in Refs. [30, 31] and references therein. In this model the masses of the pulsar and companion are approximately $1.4M_\odot$, the period of rotation around the common center of mass is 7.75 hours, the values of periastron and apastron are 1.1 and 4.8 $R_\odot$, respectively, and the orbital velocity of stars is 450 km/s and 110 km/s at periastron and apastron, respectively. Then relativistic effects are much stronger than in Solar System. For example, the precession of periastron is 4.2 degrees per year.

The most striking effect in the above model is that it predicts that the energy loss due to gravitational radiation can be extracted from the data. As noted in Ref. [30], comparison of the measured and theoretical values requires a small correction for relative acceleration between the solar system and binary pulsar system, projected onto the line of sight. The correction term depends on several rather poorly known quantities, including the distance and proper motion of the pulsar and the radius of the Sun's galactic orbit. However, with the best currently available values the agreement between the data and the Einstein quadrupole formula for the gravitational radiation is better than 1%. The rate of decrease of orbital period is 76.5 microseconds per year (i.e. one second per 14000 years).

As noted by the authors of Ref. [30], "*Even with 30 years of observations, only a small portion of the North-South extent of the emission beam has been observed. As a consequence, our model is neither unique nor particularly robust. The North-South symmetry of the model is assumed, not observed, since the line of sight has fallen on the same side of the beam axis throughout these observations. Nevertheless, accumulating data continue to support the principal features noted above.*"

The size of the invisible component is not known. The arguments that this component is a compact object are as follows [32]: "*Because the orbit is so close* $(1\,solar\,radius)$*) and because there is no evidence of an eclipse of the pulsar signal or of mass transfer from the companion, it is generally agreed that the companion is* *compact. Evolutionary arguments suggest that it is most likely a dead pulsar, while B1913+16 is a recycled pulsar. Thus the orbital motion is very clean, free from tidal or other complicating effects. Furthermore, the data acquisition is clean in the sense that by exploiting the intrinsic stability of the pulsar clock combined with the ability to maintain and transfer atomic time accurately using GPS, the observers can keep track of pulse time-of-arrival with an accuracy of* $13\mu s$ *, despite extended gaps between observing sessions (including a several-year gap in the middle 1990s for an upgrade of the Arecibo radio telescope). The pulsar has shown no evidence of glitches in its pulse period.*"

However, it is not clear whether or not there exist other reasons for substantial energy losses. For example, since the bodies have large velocities and are moving in the interstellar medium, it is not clear whether the effect of one second per 14000 years may be simply a consequence of the interaction of the components with the medium. In addition, a problem arises to what extent the effect of mass exchange in close binaries is important. The state-of-the-art review of the theory of close binaries can be found in Ref. [33] and references therein. Nevertheless, the above results are usually treated as a strong indirect confirmation of the existence of gravitational waves (GWs).

Those results have given a motivation for building powerful facilities aiming to detect GWs directly. After many years of observations no unambiguous detections of GWs have been reported [34]. However, recently the LIGO Collaboration has announced [35] the direct discovery of GWs and then several similar phenomena have been found (see e.g. Ref. [36]). On September 14, 2015 at 09:50:45 UTC the two LIGO detectors observed the event called GW150914 and treated as GWs for the following reasons.

The authors of Ref. [35] say that "*the most plausible explanation*" of the event is that the detected signals are caused by *gravitational-wave emission in the coalescence of two black holes — i.e., their orbital inspiral and merger, and subsequent final black hole ringdown*. The motivation is that the data are consistent with a system of parameters in numerical relativity models discussed in Ref. [37] and confirmed to 99.9% by an independent calculation based on Ref. [38]. The data are consistent with the model where the initial black hole masses are $(36^{+5}_{-4})M_\odot$ and $(29\pm 4)M_\odot$ and the final black hole mass is $(62\pm 4)M_\odot$ with the energy $(3.0\pm 0.5)M_\odot c^2$ radiated in GWs during approximately $0.2s$. However, the authors do not say explicitly how many initial parameters are needed in the model and do not display all the parameters.

The author of Ref. [39] describes his interviews with well known gravitational scientists. In particular, Professor Thorne, who is one of the founders of LIGO says: "*It is by far the most powerful explosion humans have ever detected except for the big bang*", and Professor Allen, who is the director of the Max Planck Institute for Gravitational Physics and leader of the Einstein@Home project for the LIGO Scientific Collaboration says: "*For a tenth of a second the collision shines brighter than all of the stars in all the galaxies. But only in gravitational waves*".

From the particle physics point of view, the existence of neutron stars is not a problem because the process $p+e\to n+\nu$ is well understood. As already noted, typical models say that the masses of neutron stars are in the range $(1.2-1.6)M_\odot$ and their radii are of the order of $10km$. It is believed that when the mass is greater then even such a dense neutron matter cannot prevent gravitational collapse. However, the existing particle theory does not know what happens to such a matter under such extreme conditions. Therefore the theory does not know what type of matter black holes consist of. In the literature several models of black holes are discussed including those where a black hole has a nonzero electric charge.

If gravity on quantum level is described in terms of gravitons then the following problem arises. A black hole is a region of space that no real particles, including photons and gravitons can escape from inside it. However, at distances much greater than the size of the region the gravitational field of the black hole is the same as for the usual star with the same mass and spin. This implies that virtual gravitons can escape from the region without problems. The difference between real and virtual gravitons is that the four-momenta squared of the latter do not equal m_g^2 where m_g is the graviton mass. However, they can be very close to m_g^2. Therefore it is not clear why the properties of real and virtual gravitons are so different.

Another problem is whether or not it is natural that the only observed manifestation of the release of such a huge amount of energy during such a short period of time was that the 4km path of the laser beam in the LIGO interferometer was stretched by the value which is less than the proton radius. From the particle physics point of view, the merger of two black holes such that $3M_\odot c^2$ is released in the form of GWs during $0.2s$ is a problem because even the type of matter black holes consist of is not known. While from the point of view of GR gravitational waves are described as ripples in space-time, in particle theory any wave is treated as a collection of particles. In particular, GWs are believed to consist of real (not virtual) gravitons. When two high energy particles smash in the accelerator, typically many different particles are produced. By analogy, one might think that in such a tremendous phenomenon, where the (unknown type of) matter experiences extremely high accelerations, a considerable part of energy should be released not only in the form of gravitons but also in the form of photons and other particles. For example, the electric charge of the neutron is zero but the neutron has a magnetic moment and consists of charged quarks. Therefore the neutron moving with a large acceleration will emit photons. So the assumption that the energy is released only in the form of GWs does not seem to be convincing and one might expect that the effect is accompanied by extremely bright flashes in different parts of the electromagnetic spectrum. A problem of the electromagnetic energy released in the black hole merger has been discussed by several authors (see e.g. Ref. [40] and references therein) who state that the problem is extremely difficult and considerably model dependent. In addition, even if the energy is released only in the form of GWs then a problem arises to what extent the orbits of Sun, Earth and Moon will be affected by such strong GWs. This problem has not been discussed in the literature.

In experiments with two detectors the position of the source of GWs cannot be identified with a good accuracy but in the experiment with three detectors [36] the area of the 90% credible region has been reduced from 1160 deg^2 to 60 deg^2. After the first LIGO announcement the authors of Ref. [41] analyzed the data of the Fermi Gamma-ray Burst Monitor obtained at the time of the LIGO event. The data reveal the presence of a weak source above $50keV$, $0.4s$ after the LIGO event was detected. Its localization is ill-constrained but consistent with the direction of the GW150914. However, in view of the weakness of the signal it is highly questionable that it is related to the LIGO event. Since the energy released in the event is approximately known and the distance to the event also is approximately known then it is easy to estimate that the energy received by Earth during $0.2s$ is by five orders of magnitude greater than the energy received from Sirius and by six orders of magnitude less than the energy received from Sun. In addition, it is usually assumed that photons and gravitons are massless particles the speed of which can be only c. Therefore $0.4s$ corresponds to the distance $120000km$. This is incompatible with the fact that the monitor resides in a low-earth circular orbit at an altitude of $550km$.

Summarizing the above discussion, we note the following. As far as three standard tests of GR are concerned: the explanation of the precession of Mercury's orbit is a great achievement of GR, but the explanation of the gravitational red shift of light and the deflection of light by the Sun involves several assumptions and therefore those explanations are model dependent. The problem of explaining strong gravitational effects is very complicated because GR, which is a pure classical theory, should be reconciled with the present understanding of particle theory, and conclusions about GWs are based on models with many fitted parameters. So the statements that those effects can be treated as strong confirmations of the existence of GWs are premature. In any case until the nature of gravity on classical and quantum level is well understood, different approaches should be investigated.

9 My proposal on gravity

Gravity is believed to be the fourth interaction which should be combined with other interactions. However, gravity is known only on macroscopic level and is not manifested at the level of elementary particles. To think that gravity is described by analogy with EM interactions of electrons or atoms is a great extrapolation. Efforts to construct quantum theory of gravity by analogy with other interactions lead to a theory which is not even renormalizable. All modern theories for constructing quantum theory of gravity (Loop quantum gravity, Noncommutative geometry, String theory and others) are based on background space-time, and even for this reason, they cannot be valid.

Any quantum theory of gravity can be tested only on macroscopic level. Hence, the problem is not only to construct quantum theory of gravity but also to understand a correct structure of the position operator on macroscopic level. However, in the literature the latter problem is not discussed because it is tacitly assumed that the position operator on macroscopic level is the same as in standard quantum theory. This is also a great extrapolation which should be substantiated.

GR is a pure classical theory involving the notions of background space-time and fields. The usual point of view is that GR is a universal theory of gravitation on classical level and that quantum theory of gravity should be a quantum generalization of GR. However, as noted in the preceding sections, quantum theory should not involve those notions. As discussed in Sec. 7 , quantum dS theory in semiclassical approximation can reproduce the result of GR on cosmological acceleration. Our result is more general than the result of GR because any classical result should be a consequence of quantum theory in semiclassical approximation and, as noted in the preceding sections, quantum dS theory does not involve any geometry (e.g. the notion of dS space and its metric and connection). So the fact that quantum theory in semiclassical approximation is in agreement with GR does not mean that quantum theory should involve the notions of background space-time and fields. Our result on cosmological acceleration has been obtained simply in dS quantum mechanics of two free bodies.

Let us now discuss whether gravity also can be explained as a consequence of dS symmetry in systems of free bodies, i.e. gravity is not an interaction but simply a manifestation of dS symmetry. As noted in Sec. 7, in the framework of this approach the cosmological acceleration can be explained if the operator of the relative distance is chosen in the standard form $\mathbf{r} = i\hbar\partial/\partial\mathbf{q}$ and then the relative acceleration is given by Eq. (5). It has been noted that this acceleration is not negligible only at cosmological distances. Therefore for the description of gravity the relative distance operator should be modified such that at non-cosmological distances it gives gravity and at cosmological distances it gives the cosmological acceleration. In Ref. [20] we propose such an operator and argue that it satisfies necessary requirements.

Then the result of calculations for the classical nonrelativistic Hamiltonian is

$$H_{nr}(\mathbf{r},\mathbf{q}) = \frac{\mathbf{q}^2}{2m_{12}} - \frac{m_1m_2RC^2}{2(m_1+m_2)r}(\frac{1}{\delta_1}+\frac{1}{\delta_2}) \tag{6}$$

where $r = |\mathbf{r}|$, C is a constant and δ_j $(j = 1, 2)$ is the width of momentum distribution for particle j. The last term in this expression is the correction to standard free two-body nonrelativistic Hamiltonian. We see that the correction disappears if the width of the dS momentum distribution for each body becomes very large. In standard theory (over complex numbers) there is no limitation on the width of distribution because in semiclassical approximation the only limitation is that the width of the dS momentum distribution should be much less than the mean value of this momentum. According to Ref. [20], in finite quantum theory (FQT) based on finite mathematics it is natural that the width of the momentum distribution for a macroscopic body is inversely proportional to its mass. Then we recover the Newton gravitational law. Namely, if

$$\delta_j = \frac{R}{m_jG'} \quad (j = 1, 2), \quad C^2G' = 2G \tag{7}$$

then

$$H_{nr}(\mathbf{r},\mathbf{q}) = \frac{\mathbf{q}^2}{2m_{12}} - G\frac{m_1m_2}{r} \tag{8}$$

We conclude that in our approach gravity is simply a dS correction to standard nonrelativistic Hamiltonian.

As shown in Ref. [20], it is also possible to derive the expression for the gravitational correction if the particles are relativistic. For example, if $m_2 \gg m_1$ then the energy of particle 1 in the c.m. frame is

$$H_{rel}(\mathbf{r}, \mathbf{q}) = (m_1^2 + \mathbf{q}^2)^{1/2}(1 - \frac{Gm_2}{r}) \tag{9}$$

and the nonrelativistic expression for this energy is

$$H_{nr}(\mathbf{r}, \mathbf{q}) = \frac{\mathbf{q}^2}{2m_1} - \frac{Gm_1 m_2}{r} \tag{10}$$

The first expression can be used for the description of deflection of light by the Sun.

In classical and quantum field theories, a system of N particles can be selfconsistently described in terms of only degrees of freedom related to those particles only in the approximation $(v/c)^2$ because in higher order approximations creation of new particles should be taken into account. However, effective interactions arising as a consequence of dS symmetry can be described exactly because in fact we deal with a system of free particles. A problem discussed in the literature is whether or not gravitational interaction is transmitted with the speed c. However, for the effective dS interactions this problem does not arise.

In the next section we consider a proof that FQT is more general than standard quantum theory based on complex numbers: the latter is a special degenerate case of the former in the formal limit $p \to \infty$ where p is the characteristic of the ring or field in FQT. The problem of calculating G is very difficult because the calculation requires knowledge of the WFs of the the bodies under consideration. However, as explained in Ref. [20], the fact that the width of momentum distribution for a macroscopic body with the mass m is inversely proportional to m is clear from qualitative considerations. A naive explanation is that if p is finite, the same set of numbers which was used for describing one body is now shared between N bodies. In other words, if in standard theory each body in the free N-body system does not feel the presence of other bodies, in FQT this is not the case. This might be treated as an effective interaction in the free N-body system.

A rough estimation for the quantity G in Ref. [20] is

$$G = \frac{Rln(Rm_N)}{m_N ln(p)} \tag{11}$$

where m_N is the nucleon mass. Then comparing this expression with the known value of the gravitational constant we get that if, for example, R is of the order of $10^{26}m$, then $ln(p)$ is of the order of 10^{80}. Therefore p is a huge number of the order of $exp(10^{80})$.

10 Quantum theory over finite mathematics

Standard quantum theory is based on classical mathematics involving such notions as infinitely small/large, continuity etc. However, this mathematics cannot be a part of the ultimate theory describing nature on the very fundamental level. One of the reasons follows. The notions of infinitely small/large, continuity etc. were proposed by Newton and Leibniz more than 300 years ago. At that time people did not know about existence of atoms and elementary particles and believed that any body can be divided by an arbitrarily large number of arbitrarily small parts. However, now it is obvious that standard division has only a limited applicability because when we reach the level of atoms and elementary particles standard division loses its meaning. For example, a glass of water contains approximately 10^{25} molecules. We can divide this water by ten, million, etc. but when we reach the level of atoms and elementary particles further division operation loses its usual meaning and we cannot obtain arbitrarily small parts. The notions of 1/2 of electron or 1/2 of neutrino are meaningless. In nature there are no continuous curves and surfaces. For example, if we draw a line on a sheet of paper and look at this line by a microscope then we will see that the line is strongly discontinuous because it consists of atoms. So, as far as applications of mathematics to physics are concerned, classical mathematics is only an approximation which in many cases works with very high accuracy but the ultimate quantum theory cannot be based on classical mathematics.

And it is especially strange that modern particle theories and string theory are based on classical mathematics. For me it is very strange that many physicists believe that at distances $10^{-35}m$ there can be any geometry and topology (to say nothing about the fact that such distances cannot exist in principle). It is clear from the above remarks that geometry or topology can describe nature only in the approximation when sizes of atoms and elementary particles are neglected.

As proved in Refs. [15, 16, 20], standard quantum theory is a special degenerate case of a quantum theory based on finite mathematics (FQT) in the formal limit $p \to \infty$ where p is the characteristic of the ring or field in FQT. In this theory, states are elements of not complex Hilbert spaces but spaces over a finite ring of field of characteristic p, and operators of physical quantities are operators in such spaces.

My observation is that majority of physicists do not have even very basic knowledge in finite math. This is not a drawback because everybody knows something and does not know something, and it's impossible to know everything. However many such physicists characterize finite math as "Sache an sich" (if I remember Kant correctly), philosophy, pathology and/or exotics which has nothing to do with physics and that's why in their opinion papers investigating quantum theory over finite mathematics should not be published.

In conversations with physicists some of them agree that in nature there are no infinitely small objects. For example, they say that dx/dt should be understood as $\Delta x/\Delta t$ where Δx and Δt are small but not infinitely small. I tell them: "but you are using math with dx/dt not $\Delta x/\Delta t$". Their answer typically is that standard math is only a technique which works and there is no need to philosophize and use something else (what they don't know). In view of efforts to describe discrete nature by continuous mathematics, my friend told me the following joke. A group of monkeys is ordered to reach the Moon. For solving this problem each monkey climbs a tree. The monkey who has reached the highest point believes that he has made the greatest progress and is closer to the goal than the other monkeys.

FQT does not contain divergences at all and all operators are automatically well defined. In my discussions with physicists, some of them commented this fact as follows. This is an approach where a cutoff (the characteristic p of the finite ring or field) is introduced from the beginning and for this reason there is nothing strange in the fact that the theory does not have infinities. It has a large number p instead and this number can be practically treated as infinite.

The inconsistency of this argument is clear from the following analogy. It is not correct to say that relativistic theory is simply nonrelativistic one with the cutoff c for velocities. As a consequence of the fact that c is finite, relativistic theory considerably differs from nonrelativistic one in several aspects. The difference between finite rings or fields on one hand and usual complex numbers on the other is not only that the former are finite and the latter are infinite. Since in finite mathematics the rules of arithmetic are different then, as a result, FQT has many unusual features which have no analogs in standard theory.

The fact that at the present stage of the Universe p is a huge number explains why in many cases classical mathematics describes natural phenomena with a very high accuracy. **At the same time, as shown in Refs. [13, 16, 20], the explanation of several phenomena can be given only in the theory where p is finite.**

One of the examples is that in our approach gravity is a manifestation of the fact that p is finite. As follows from Eq. (11), in the formal limit $p \to \infty$ gravity disappears. Although p is a huge number of the order of $exp(10^{80})$, $ln(p)$ is "only" of the order of 10^{80} and the existence of p is observable.

A typical situation in physics is as follows:

Definition: *Let theory A contain a finite parameter and theory B be obtained from theory A in the formal limit when the parameter goes to zero or infinity. Suppose that with any desired accuracy theory A can reproduce any result of theory B by choosing a value of the parameter. On the contrary, when the limit is already taken then one cannot return back to theory A, and theory B cannot reproduce all results of theory A. Then theory A is more general than theory B and theory B is a special degenerated case of theory A.*

One of the known examples is the comparison of nonrelativistic theory (NT) with relativistic one (RT). Usually RT is treated as more general than NT for several reasons. One of the arguments is that RT contains a finite parameter c and NT can be treated as a special degenerated case of RT in the formal limit $c \to \infty$. Therefore, by choosing a large value of c, RT can reproduce any result of NT with a high accuracy. On the contrary, when the limit is already taken one cannot return back from NT to RT and NT cannot reproduce all results of RT. It can reproduce only results obtained when $v \ll c$. Other known examples are that classical theory is a special degenerate case of quantum one in the formal limit $\hbar \to 0$ and RT is a special degenerated case of dS and AdS invariant theories in the formal limit $R \to \infty$ where R is the parameter of contraction from the dS or AdS algebras to the Poincare algebra (see Sec. 5).

In Refs. [15, 16, 20] we have proved that those facts are valid not only from physical but also from pure mathematical consideration. We have also proved mathematically that the relation between standard quantum theory and FQT satisfies the requirements specified in **Definition**: standard quantum theory is a special degenerate case of FQT in the formal limit $p \to \infty$.

However, in my discussion with physicists I have realized that for many of them the facts that FQT does not contain divergences and that some phenomena can be explained only if p is finite are not convincing. Typically their objections against FQT are the following:

A_1) It is unnatural that a value of a quantity can be $p-1$ but cannot be $p+1$. However, it is not consistent to extrapolate our experience with numbers much less than p to the area where the numbers are comparable to p.

B_2) If $n_1 = n_2 = p/2 + 10$ then one might think that $(n_1 + n_2)$ exceeds p. However, in finite rings or fields of characteristic p, all operations are modulo p, and therefore the result of any operation cannot exceed p.

C_2) One might pose a question why p is as is. According to FQT, nature is described by finite mathematics with some value of p. A problem whether p is a fundamental quantity which is always the same during the whole history of the Universe or this quantity can be different at different periods of the history. The first possibility seems unrealistic because it is not clear what was the reason for nature to prefer a particular value of p. Note that the problem of time is one of the most fundamental problems in quantum theory. In Ref. [8] we discussed a conjecture that not p changes with time because time is only a classical notion but vice versa the manifestation of time is a consequence of the fact that p changes.

I believe it is interesting to compare those objectives with typical objections against special relativity (SR). Although SR is now accepted by majority of physicists, some of them still have doubts that (if tahyons are not considered) speed of light c is the maximum possible speed for any particle. Typical objections are as follows.

A_2) It seems unnatural that velocity $0.999c$ is allowed while velocity $1.001c$ is not. This objection arises as a consequence of the fact that it is not consistent to extrapolate the everyday experience with velocities much less than c to the area where they are comparable to c. So the objection A_2 is analogous to the objection A_1.

B_2) Let the reference frame K is moving relative to the observer O along the positive direction of the x axis with the speed $V = 0.6c$. Suppose also that, in this reference frame a particle is moving in the same direction with the speed $v_K = 0.6c$. Then, from the point of view of standard experience, one might think that the particle is moving relative to O with the speed $v = V + v_K = 1.2c$ while the formula of SR for addition of velocities gives $v \approx 0.882c$. Analogously, if $V = 0.99c$ and $v_K = 0.99c$ then SR gives not $v = 1.98c$ but $v \approx 0.9999495c$. The reason of the fictitious inconsistency of SR is again that it is not correct to extrapolate our experience with velocities much less than c to the area where they are comparable to c. So the objection B_2 is analogous to the objection B_1.

C_2) It is not clear why $c \approx 3 \cdot 10^8 m/s$ and not say $c = 10^9 m/s$. However, as explained e.g. in Ref. [16], relativistic quantum theory itself does not need the value of c at all. The value of c in m/s arises because people's choice is to measure velocities in such units, and the question why c is as is does not arise. In the modern system of units it is postulated that the value of c in m/s does not change with time. However, as discussed in Ref. [20] and Sec. 7, this postulate is not based on rigorous physical principles. So the objection C_2 is analogous to the objection C_1.

In summary, the above arguments clearly show that FQT is a "better" theory than standard quantum theory and moreover there is no logic justifying that the present quantum theory is based on continuous mathematics. The only "justification" is that physicists used to work with continuous mathematics, they are not familiar with finite mathematics and that's why they are reluctant to swith to finite mathematics. But I believe that sooner or later physicists will recognize that for constructing new quantum theory this is necessary.

Of course, although it has been mathematically proved that FQT reproduces all results of standard quantum theory in the formal limit $p \to \infty$ it might be technically difficult to demonstrate this. One of the examples: the usual mentality is that integral is something fundamental and Riemannian sums for the integral is only something auxiliary. But in fact the situation is the opposite: nature can be described only by finite sums and in some cases integrals can be good approximations for such sums. And in many cases it suffices to work with standard theory.

As an example, consider the following hypothetical situation. Heisenberg or Schrödinger sent to a journal their paper on quantum mechanics, and the referee says: "I want you to show that your theory correctly describes the motion of the Moon". In other words he wants to see that the Schrödinger equation correctly describes the motion of the Moon. However, it is not reasonable to apply this equation to the Moon. We know that in classical approximation the Schrödinger equation becomes the Hamilton-Jacobi one, and the Moon is a strongly classical system. So it's quite sufficient to describe the motion of the Moon by the Hamilton-Jacobi equation, not the Schrödinger one.

In my cases referees said that they want to see how FQT can recover the φ^4 theory in QFT or reproduce the results with renormalization group, I gave them the above example but this did not help.

11 Why finite mathematics is more general than classical one

In the preceding section we argue that FQT is more general than standard quantum theory. The goal of this section is to explain that, even regardless of physics, finite mathematics is more general than classical one, and the latter is a special degenerate case of the former in the formal limit $p \to \infty$ where p is the characteristic of the ring or field in finite mathematics. First we make some notes on standard arithmetic.

11.1 Remarks on arithmetic

In the 20s of the 20th century the Viennese circle of philosophers under the leadership of Schlick developed an approach called logical positivism which contains verification principle: *A proposition is only cognitively meaningful if it can be definitively and conclusively determined to be either true or false* (see e.g. References [**?**, **?**, **?**]). On the other hand, as noted by Grayling [**?**], *"The general laws of science are not, even in principle, verifiable, if verifying means furnishing conclusive proof of their truth. They can be strongly supported by repeated experiments and accumulated evidence but they cannot be verified completely"*. Popper proposed the concept of falsificationism [**?**]: *If no cases where a claim is false can be found, then the hypothesis is accepted as provisionally true.*

According to the principles of quantum theory, there should be no statements accepted without proof and based on belief in their correctness (i.e. axioms). The theory should contain only those statements that can be verified, at least in principle, where by "verified" physicists mean experiments involving only a finite number of steps. So the philosophy of quantum theory is similar to verificationism, not falsificationism. Note that Popper was a strong opponent of quantum theory and supported Einstein in his dispute with Bohr.

The verification principle does not work in standard classical mathematics. For example, it cannot be determined whether the statement that $a+b=b+a$ for all natural numbers a and b is true or false. According to falsificationism, this statement is provisionally true until one has found some numbers a and b for which $a+b \neq b+a$. There exist different theories of arithmetic (e.g. finitistic arithmetic, Peano arithmetic or Robinson arithmetic) aiming to solve foundational problems of standard arithmetic. However, those theories are incomplete and are not used in applications.

From the point of view of verificationism and principles of quantum theory, classical mathematics is not well defined not only because it contains an infinite number of numbers. For example, let us pose a problem whether 10+20 equals 30. Then one should describe an experiment which gives the answer to this problem. Any computing device can operate only with a finite amount of resources and can perform calculations only modulo some number p. Say $p=40$, then the experiment will confirm that 10+20=30 while if $p=25$ then one will get that 10+20=5. *So the statements that 10+20=30 and even that $2 \cdot 2 = 4$ are ambiguous because they do not contain information on how they should be verified.* On the other hands, the statements

$$10+20=30\,(mod\,40),\;\; 10+20=5\,(mod\,25),$$

$$2\cdot 2=4\,(mod\,5),\;\; 2\cdot 2=2\,(mod\,2)$$

are well defined because they do contain such an information. So, from the point of view of verificationism and principles of quantum theory, only operations modulo a number are well defined.

We believe the following observation is very important: although classical mathematics (including its constructive version) is a part of our everyday life, people typically do not realize that *classical mathematics is implicitly based on the assumption that one can have any desired amount of resources*. In other words, standard operations with natural numbers are implicitly treated as limits of operations modulo p when $p \to \infty$. Usually in mathematics, legitimacy of every limit is thoroughly investigated, but in the simplest case of standard operations with natural numbers it is not even mentioned that those operations can be treated as limits of operations modulo p. In real life such limits even might not exist if, for example, the Universe contains a finite number of elementary particles.

Classical mathematics proceeds from standard arithmetic which does not contain operations modulo a number while finite mathematics necessarily involves such operations. In what follows we explain that, regardless of philosophical preferences, finite mathematics is more general than classical one.

11.2 The problem of potential vs. actual infinity

According to Wikipedia: "In the philosophy of mathematics, the abstraction of actual infinity involves the acceptance (if the axiom of infinity is included) of infinite entities, such as the set of all natural numbers or an infinite sequence of rational numbers, as given, actual, completed objects. This is contrasted with potential infinity, in which a non-terminating process (such as "add 1 to the previous number") produces a sequence with no last element, and each individual result is finite and is achieved in a finite number of steps.".

The *technique* of classical mathematics involves only potential infinity, i.e. infinity is understood only as a limit and, as a rule, legitimacy of every limit is thoroughly investigated. However, *the basis* of classical mathematics does involve actual infinity: this mathematics starts from the set infinite of natural numbers N and from the infinite ring of integers $Z = (0, \pm 1, \pm 2, ...)$ which is the starting point for constructing infinite sets with different cardinalities, and, even in standard textbooks on classical mathematics, it is not even posed a problem whether Z can be treated as a limit of finite sets.

On the other hand, by definition, finite mathematics deals only with finite sets and finite numbers of elements (for example, in finitistic mathematics all natural numbers are considered but only finite sets are allowed). Known examples are theories of finite fields and finite rings described in a vast literature. Finite mathematics starts from the ring $R_p = (0, 1, ...p - 1)$ where all operations are modulo p. In the literature the notation Z/p for R_p is often used. We believe that this notation is not quite consistent because it might give a wrong impression that finite mathematics starts from the infinite set Z and that Z is more general than R_p.

As shown in Refs. [15, 16, 20], the first stage in proving that FQT is more general than standard quantum theory is the proof that $R_p \to Z$ when $p \to \infty$. This analogous to the fact mentioned above several times that RT becomes NT when

$c \to \infty$. Let us recall again that RT is more general than NT because RT can reproduce all result of NT if c is chosen to be sufficiently large but in RT there are effects which take place because c is finite and NT cannot reproduce those effects. The relation between R_p and Z is analogous: any result in Z can be reproduced in R_p if p is chosen to be sufficiently large but when R_p is replaced by Z then we obtain a degenerate theory because all operations modulo a number are lost and therefore in Z it is not possible to reproduce all results in R_p. This observation shows that, even from pure mathematical point of view, introducing infinity results in the degenerate theory.

I asked mathematicians to tell me where is the proof that $R_p \to Z$ when $p \to \infty$. Their typical respons was that this is obvious and with this motivation my paper was rejected from a journal. However, in mathematics there should not be an argument that something is obvious: either a proof or a reference to the proof should be given. And I asked mathematicians: if this is obvious then, in my understanding, every textbook on calculus should begin with this proof. But, as noted above, classical math starts from the infinite set Z and it is not even posed a question whether Z is a limit of a finite set. But those mathematicians gave neither the proof nor references to the proof. And only Professor Zelmanov (who is the Fields Medal laureate) told me that "infinite fields of zero characteristic (and Z) can be embedded in ultraproducts of finite fields". However, the theory of ultraproducts described in a wide literature is essentially based on classical results on infinite sets involving actual infinity. In particular, the theory is based on Łoŝ' theorem involving the axiom of choice. Therefore theory of ultraproducts cannot be used in proving that finite mathematics is more general than classical one. The proof should not be based on actual infinity, it should be analogous to standard proof when a sequence (a_n) goes to infinity if $n \to \infty$, i.e. the proof should involve only potential infinity.

When R_p is replaced by Z, we obtain classical mathematics which is not only a degenerate theory but a theory with foundational problems which cannot be resolved, as follows e.g. from Gödel's incompleteness theorems. They state that no system of axioms can ensure that all facts about natural numbers can be proven and the system of axioms in classical mathematics cannot demonstrate its own consistency. The foundational problems of classical mathematics arise as a consequence of the fact that the number of natural numbers is infinite. On the other hand, since finite mathematics deals only with a finite number of elements, it does not have foundational problems because here every statement can be directly verified, at least in principle.

The efforts of many great mathematicians to resolve foundational problems of classical mathematics have not been successful yet. The philosophy of Cantor, Fraenkel, Gödel, Hilbert, Kronecker, Russell, Zermelo and other great mathematicians was based on macroscopic experience in which the notions of infinitely small, infinitely large, continuity and standard division are natural. However, as noted above, those notions contradict the existence of elementary particles and are not natural in quantum theory. The illusion of continuity arises when one neglects discrete structure of matter.

As follows even from the fact that classical mathematics has foundational problems, ultimate physical theory will not be based on classical mathematics. Since quantum theory is the most general theory describing nature (because all other theories follow from quantum one), and FQT is more general than standard quantum theory, we conclude that:

Mathematics describing nature at the most fundamental level involves only a finite number of numbers while the notions of limit and infinitely small/large and the notions constructed from them (e.g. continuity, derivative and integral) are needed only in calculations describing nature approximately.

Let me also make the following remarks. Classical computer science is based on finite mathematics for obvious reasons. Any computer can operate only with a finite number of bits, a bit represents the minimum amount of information and the notions of one half of the bit, one third of the bit etc. are meaningless. So a bit is an analog of elementary particle. However, theory of quantum computing is based on the notion of qubit which is a quantum superposition of bits with complex coefficients. As follows from the above discussion, such a definition of qubit is not natural: instead, the coefficients should be elements of a finite ring or field.

12 Conclusion

The motivation to describe my understanding of the reasons of the present situation in foundation of physics came from Sabine Hossenfelder's blog [1] where she argues that this situation should be called not crisis but stagnation. Here are some extracts from the blog:

- The problem is also not that we lack data. We have data in abundance. ... The current theories are incomplete. We know this both because dark matter is merely a placeholder for something we don't understand, and because the mathematical formulation of particle physics is incompatible with the math we use for gravity.

- Another comment-not-a-question I constantly have to endure is that I supposedly only complain but don't have any better advice for what physicists should do. First, it's a stupid criticism that tells you more about the person criticizing than the person being criticized. ... Second, it's not true. I have spelled out many times very clearly what theoretical physicists should do differently. It's just that they don't like my answer. They should stop trying to solve problems that don't exist. ... Focus on mathematically well-defined problems.

- I am afraid there is nothing that can stop them. They review each other's papers. They review each other's grant proposals. And they constantly tell each other that what they are doing is good science. Why should they stop? For them, all is going well. They hold conferences, they publish papers, they discuss their great new ideas. From the inside, it looks like business as usual, just that nothing comes out of it. This is not a problem that will go away by itself. If you want to know more about what is going wrong with the foundations of physics, read my book "Lost in Math: How Beauty Leads Physics Astray".

The advice: "Focus on mathematically well-defined problems" is in the spirit of Dirac's advice given in Ref. [42]: *"I learned to distrust all physical concepts as a basis for a theory. Instead one should put one's trust in a mathematical scheme, even if the scheme does not appear at first sight to be connected with physics. One should concentrate on getting an interesting mathematics."*

I understand Dirac's advice such that our macroscopic experience and physical intuition do not work on quantum level and hence here we can rely only on solid mathematics. However, many physicists do not think so and believe that Dirac was "The Strangest Man" (this is the title of the book by Graham Farmelo about Dirac).

As discussed in the present notes, mentality of majority of physicists is that agreement with experimental data is much more important than mathematical consistency. Probably one of main arguments in favor of such a mentality is the successes of QED at the end of the fourties of the 20th century when theory explained the magnetic moments of the electron and muon with the accuracy seven digits and the Lamb shift with the accuracy five digits. Those results were obtained in inconsistent mathematics when one infinity was subtracted from the other. In turn, the inconsistency is a consequence of using the notion of space-time background which is only a classical notion. However, this notion is a basic one in efforts to construct quantum theory of gravity, string theory and other modern theories.

Let us note, however, that the above successes of QED were achieved in the third order in perturbation theory over the fine structure constant α which is small. There are no agreement with the data in higher orders, and the theory does not know whether the perturbation series is convergent, divergent or asymptotic even if the interaction constant is small. So there are no serious reasons to believe that ultimate quantum theory will be constructed by analogy with QED. In Sec. 4 I explain in detail that **the notion of space-time background is pure classical and should not be present in quantum theory. Moreover, presence of this notion results in several mathematical inconsistencies.**

The goal of fundamental quantum theories (QED, QCD and electroweak theory) is to construct the S-matrix in momentum space and, after this construction has been performed, one can forget about Minkowski space and local quantum fields. For this construction the theory involves the principle that coordinate and momentum representations are related to each other by the Fourier transform. As noted in Sec. 2, this principle is inherited from classical electrodynamics. It is known that in quantum theory this principle works in semiclassical approximations but there are no convincing theoretical arguments or experimental data that this principle is valid beyond semiclassical approximation. As noted in Sec. 3, the fact that the coordinate Schrödinger and Dirac equations are in good agreement with experimental data is only a consequence of the fact that α is small. **As explained in Sec. 2, applying Heisenberg's principle to photons from stars results in paradoxes**

in observation of stars. The paradoxes are resolved with a nonstandard choice of the position operator such that the coordinate and momentum representations are not related to each other by the Fourier transform.

As noted in Sec. 5, the fact that Poincare symmetry is more general that Galilei one and that dS and AdS symmetries are more general than Poincare one follows not only from physical but even from pure mathematical consideration. For the first time this fact has been pointed out by Dyson in his famous paper [17]. This paper appeared in 1972 and, in view of Dyson's results, a question arises why general theories of elementary particles (QED, electroweak theory and QCD) are still based on Poincare symmetry and not dS or AdS symmetries. Probably, mentality of many particle physicists is that dS and AdS symmetries is something what can be used only at cosmological scales while using those symmetries in particle theory is not necessary. A "justification" might be such that the parameter of contraction R from dS and AdS symmetries to Poincare one is much greater than sizes of elementary particles and atoms, and for this reason in particle theory this parameter can be treated as infinite. As explained in Sec. 5, such a "justification" is not consistent, and, especially in dS symmetry, the notions of particle-antiparticle and conservation laws fully differ from the corresponding notions in Poincare invariant theories. That's why **extending particle theory to dS and AdS symmetries is a must.**

As argued in Sec. 6, the notion of interaction is problematic for several reasons and, probably, the future quantum theory will not involve this notion. As an illustration of this point we discuss the cosmological acceleration and gravity.

The existence of cosmological acceleration is stated on the basis of the results of Ref. [23] and other references where those results have been confirmed. On the other hand, the results are based on cosmological models with several fitted parameters, and some authors oppose the conclusion that the cosmological acceleration exists. Suppose, however, that it does exist. Then, as noted in Sec. 7, the phenomenon of the cosmological acceleration can be explained as a consequence of dS symmetry on quantum level in semiclassical approximation. The explanation has nothing to do with existence or nonexistence of dark energy, and the cosmological constant problem does not arise.

The notion of dark energy has arisen on the basis of the dogma that, since curvature is created by matter, empty space-time should be flat. However, the notion of curvature in GR is needed for the description of real bodies while empty space-time does not have a physical meaning because nothing can be measured in space-time which exists only in our imagination. This also indicates that the formal limit of GR when matter disappears is nonphysical because in this limit space-time remains - it becomes dS space if $\Lambda > 0$, Minkowski space if $\Lambda = 0$ and dS space if $\Lambda < 0$.

Our result for the cosmological acceleration is in agreement with the result of GR. It is often stated that quantum theory of gravity should be a quantum generalization of GR. However, our result on cosmological acceleration is an argument that **quantum theory of gravity should not be a quantum generalization of GR**. Indeed, while GR is a pure classical theory with geometry of space-time background, our result is simply a consequence of dS quantum mechanics for a two-body system in semiclassical approximation. The result does not involve dS background space-time and its geometry (metric and connection). We believe that our result is more important than the result of GR because any classical result should be a consequence of quantum theory in semiclassical approximation.

In modern inflationary models the inflation period of the Universe lasted in the range $(10^{-36}s, 10^{-32}s)$ after the Big Bang. As noted in Sec. 7, such times have no physical meaning, especially in the situation when there are no nuclei and atoms. **So description of the inflationary period by times $(10^{-36}s, 10^{-32}s)$ has no physical meaning.**

As noted in Sec. 10, continuity, **geometry and topology can describe nature only on classical level when sizes of atoms and elementary particles are neglected**. Physicists working on inflationary models usually acknowledge that considering inflationary stage of the Universe is meaningful only on quantum level. However, at present there is no theory describing such distances. Moreover, even the notion of distances of the order $10^{-25}m$ has no physical meaning. Nevertheless, physicists describe this stage by continuous mathematics. For example, they say that quantum effects should be taken into account and modify the Lagrangian of GR at such distances. However such a theory remains pure classical with space-time background etc. Physicists introduce the inflaton field with fitted parameters and manage to get parameters describing the present stage of the Universe.

This is a common trend in modern physics that if an agreement with the data has been achieved than questions about consistency of the derivation are not discussed seriously, even if the derivation involves many fitted parameters. The examples are the inflationary models and, as discussed in Sec. 8.2, the statements that GR explains several data as a manifestation of gravitational radiation. In my opinion, in such situations it would be fair, in the spirit of the blog [1], to acknowledge that this is a crisis and to try to find solutions of the problems. However, those situations can be described by extract from this blog quoted above: **"They should stop trying to solve problems that don't exist. ... And they constantly tell each other that what they are doing is good science. Why should they stop? For them, all is going well. They hold conferences, they publish papers, they discuss their great new ideas. From the inside, it looks like business as usual, just that nothing comes out of it".**

Constructing quantum theory of gravity is treated as one of the most challenging problems of quantum theory. In Sec. 9 I note why existing modern approaches in this direction will not be successful. In addition, I have the following argument related to a known story. At a seminar where a physicist presented his theory, the majority of comments were such that the theory cannot be correct because it is crazy. But Bohr said that vice versa, the theory cannot be correct because it is not crazy enough. As noted in Sec. 9, all modern approaches to this problem are based on the dogma that the theory should involve space-time background, and, as noted in the blog [1], **mathematical formulation of particle physics is incompatible with the math we use for gravity**.

In this section I describe my approach to quantum gravity. Here gravity is simply a kinematical consequence of quantum de Sitter symmetry over a finite ring or field. The main problem in proving validity of the approach is to understand the structure of WFs of macroscopic bodies. My approach obviously is not in the spirit of all mainstream approaches to quantum gravity. However, I believe that judgements on a new theory should be based not on the criterion whether or not the theory is in mainstream but whether or not it is based on solid physical and mathematical principles. My approach does not involve fictitious notions like space-time background in quantum theory, and is not based on classical mathematics because, as explained in Sec. 10, quantum theory should not be based on this mathematics.

As noted in Sec. 10, in my approach quantum theory (FQT) is based on finite mathematics involving a ring or field with characteristic p. In many cases standard classical mathematics works with a high accuracy because at the present stage of the Universe the number p is huge. For example, as follows from my estimation of the gravitational constant, p is of the order of $exp(10^{80})$ or more. At the same time, as shown in Ref. [20] and other my publications, there are phenomena where it is important that p is finite and not infinite. In particular, in my estimation of the gravitational constant, this quantity is proportional to $1/ln(p)$. Therefore in the formal limit $p \to \infty$ gravity disappears, i.e. **gravity is a consequence of finiteness of nature**.

As noted in Sec. 11, finite mathematics is not only more natural and "physical" than classical one but more general. It can be proved mathematically that standard quantum theory is a special degenerate case of FQT in the formal limit $p \to \infty$ and classical mathematics is a special degenerate case of finite one in the same limit. As a consequence,

Mathematics describing nature at the most fundamental level involves only a finite number of numbers while the notions of limit and infinitely small/large and the notions constructed from them (e.g. continuity, derivative and integral) are needed only in calculations describing nature approximately.

In these notes I tried to describe (as simply as possible) my understanding of major reasons of stagnation in foundation of physics. However, my observation is that for majority of physicists my arguments are not convincing. A typical mentality of such physicists is as follows: I don't care that classical mathematics has foundational problems, I believe that finite mathematics is a pathology or exotics which has nothing to do with physics, and I believe that elementary particles will be obtained as discrete solutions of QFT or string theory equations (by analogy with the fact that discrete energy levels of the hydrogen atom are obtained from the Schrödinger differential equation). Of course those physicists have a right to have such an opinion. Moreover, in science different opinions should be welcome. However, many such physicists do not think so and aggressively oppose publications of approaches which are not in the spirit of their dogmas. And this is an additional very serious reason for the stagnation.

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