A new approach to the foundation of quantum theory and mathematics

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Abstract

Philip Gibbs created a website and named it vixra. This name is obtained by reading the word "arxiv" in reverse order. Philip believed that the moderation system that arXiv uses is not consistent with the principles of scientific ethics. The events that I describe below show that not only arXiv, but many known journals also do not follow the principles of scientific ethics. In this abstract, I briefly describe what is most important in my approach and what problems I encountered in trying to publish my scientific results.

The concept of infinitesimals was proposed by Newton and Leibniz. In those days, people knew nothing about elementary particles and atoms and thought that, in principle, any substance can be divided into any number of parts. But now it is clear that as soon as we reach the level of elementary particles, further division is impossible. After all, even the very name "elementary particle" says that such a particle has no parts, that is, it cannot be divided into 2, 3, etc. So, there are no infinitesimals in nature, and the usual division is not universal: it makes sense only up to some limit.

Would it seem obvious? And then it is clear that fundamental quantum physics must be built without infinitesimals. It would seem that everyone understands that the construction of such a physics is far from being an easy task, and attempts at such a construction should be encouraged. However, my stories, described below, show that, as a rule, the establishment not only does not encourage such attempts, but does everything to ensure that the results in this direction are not published.

What's more amazing. As a rule, physicists even pronounce words that in nature there are small, but not infinitesimals. And, it would seem, from this it is obvious that standard mathematics with infinitesimals, continuity, etc. cannot be the theory on which the most fundamental physics is based; it can only be a good approximation. But physicists say that since standard mathematics generally works, then why philosophize and involve something else. As a rule, most physicists do not know finite mathematics, and when they hear something like a Galois field, then, for peace of mind, it is easier for them to consider that this is some kind of exotic or pathological.

I understand that, as a rule, physicists face problems that can be solved within the framework of conventional approaches. And I am by no means suggesting that all physicists should switch to finite mathematics. But, in any case, I think that physicists should not be aggressively opposed to attempts to build quantum physics without infinitesimals. But my stories show that, for some reason, many physicists are aggressively against and sometimes even fight to the death against publications with attempts to consider approaches with finite mathematics.

When I studied at the Moscow Institute of Physics and Technology and listened to lectures by well-known mathematicians, it seemed to me that rigor is the highest priority for mathematicians. But then, talking to mathematicians, I was surprised that many of them know about Gödel's theorems and the problems with the foundation of mathematics, but their way of thinking is that since standard mathematics works in many cases, then there is no need to worry about problems in its foundation. In this sense, their way of thinking is similar to the way of thinking of physicists, who think that since the theory works in many cases, there is no need to impose rigor. But still, mathematicians generally know finite mathematics, and I hoped that it would be interesting for them to know that finite mathematics is more general than standard. And, since there are no problems with foundation in finite mathematics, mathematicians, in any case, should not be aggressively against my publications. But, as I describe, it is very strange that even many "finite" mathematicians are aggressively opposed, and standard mathematicians even more so.

In addition to the infinitesimal problem, I describe other problems in which I proposed new approaches, but since they are not in the spirit of what the establishment does, I had big

problems with the publication. But, of all these tasks, there is one that probably overshadows all the others. This is a dark energy problem.

It would seem that it is generally accepted in physics that when new experimental data appear, one must first try to explain them on the basis of existing science. Only if this does not work out, then you can attract some kind of exotic. But here it's the opposite: they immediately began to attract dark energy, quintessence and other exotic. There is a lot of activity, writing articles, holding conferences, planning expensive experiments and even giving Nobel Prizes. And in all my articles on this topic (for example, in the last popular article [17]) and in my book [22] I explain that there are no problems with explaining the cosmological acceleration, everything is explained based on known science, and therefore dark energy and quintessence are nonsense. It would seem that if the establishment is honest, then they should at least read [17] and directly say that I do not understand something or they. But they pretend that they do not notice my publications on this topic.

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Chapter 1

Foreword

One of the main goals of this paper is to describe, at the simplest possible level, my approach to the foundation of quantum theory and mathematics. I believe that this approach is fundamentally new. So, I hope that physicists and mathematicians may be interested in what I think about modern physics and mathematics.

Here a question arises. Let's say you think you've done something worthwhile and naturally want to publish it. It would seem that there is every opportunity for this, there are many journals in the editorial policy of which the editors swear that your article will be considered based on the highest criteria. Many people who are far from science think that, as a rule, scientists are decent, they argue about fundamental problems, and so on. In this connection, the discussions between Einstein and Bohr immediately come to mind. And one of the main goals of these notes is to show that science is now degrading and try to understand why it happened. I met many decent scientists, and they played a big role in my life. But a significant part of the establishment (that is, those who decide something) do not proceed from scientific ethics and do not worry that their actions will be known, and their reputation will suffer. In order not to be told that my opinion is not based on anything, I decided to describe my stories about relations with editors and scientists. I understand that reading all this can be boring, but, if this is not described, then there will always be those who will say that, in fact, everything is at a high level, and my statements are the result of a sick imagination.

In spite of my disadventures with many journals, I have succeeded in publishing my results in the book [22] and in other publications.

Chapter 2

My thoughts on fundamental physics

2.1 On classical electrodynamics

At one time I was worried about the question of the substantiation of classical electrodynamics. This issue is discussed, for example, in "Field Theory" by Landau and Lifshitz and in many other publications. The main problem here is the following. Because it is assumed that elementary particles exist, then, at the classical level, such particles can only be point particles. Then the problem arises that a point charged particle has infinite energy, problems arise with retardation by radiation, and so on. Words are uttered that, for example, for an electron, classical electrodynamics works only up to distances of the electron classical radius, and at smaller distances it is already necessary to apply quantum theory.

I think that here there is no problem here at all because in classical theory there can be no point charge. In Maxwell's equations, there is no concept of charge at all, there is only charge density and current density. Formally, the charge can be defined as an integral of the charge density over the volume, and the volume cannot be zero, since the integral over a set of measure zero is equal to zero. When the charge is formally written as a delta function and the integral of it over a point volume is said to be finite, then, as is known from the theory of distributions, such an operation is defined incorrectly.

So, classical electrodynamics itself does not contain any internal contradictions. Justification problems arise artificially when we introduce point charges and delta functions with which illegal operations are made. It must simply be said that, as is known, classical electrodynamics does not describe all experimental data; it may be only a good approximation in some problems.

2.2 On General Relativity

Another famous classical (i.e., non-quantum) theory is General Relativity (GR). In their Course in Theoretical Physics, Landau and Lifshitz write that GR "is perhaps the most beautiful physical theory in existence". That is, although GR is a purely classical theory, they consider it more beautiful than quantum theory. In his gradation of great scientists, Landau puts Einstein in the undisputed first place, i.e., above the scientists who created the quantum theory. And in popular literature, Einstein is portrayed almost as a god. It looks natural since writers of popular literature do not know what Heisenberg, Dirac, Pauli and other quantum physicists did, while black holes and Big Bang seem to be the fundamental achievements of science against the primitiveness of ordinary life. There is no doubt that Einstein is a truly great scientist who made great contributions to various branches of physics. But one might get the impression from the literature that the creation of GR is far superior in importance to everything else.

The standard phrase is that GR treats gravity as a curvature of space-time. What is space and time? In mathematics, you can come up with different spaces, but in physics, you can talk about spaces only if there is a fundamental possibility to measure the coordinates of this space, because one of the principles of physics says that the definition of a physical quantity is the definition of a way for measuring it. This principle is explicitly taken as the basis of the Copenhagen interpretation of quantum theory, but implicitly it is used throughout physics.

One of the obvious physical contradictions of GR is this. The curvature of space is a formal technique to describe the motion of bodies. Therefore, if there are no bodies (empty space), then the curvature has no physical meaning, although mathematically any space can be considered. The left-hand side of Einstein's equations contains the Ricci tensor, which characterizes the curvature of space-time, and the right-hand side - the energy-momentum tensor of matter. It would seem that in the limit when matter disappears (formally, this happens when the energy-momentum tensor on the right-hand side of the Einstein equations becomes equal to zero), then the concept of space should lose its meaning, because, from the point of view of physics, space without matter is nonsense. But in GR the space does not disappear in this limit: the left-hand side remains and describes the flat Minkowski space if the cosmological constant Λ equals zero, the de Sitter space if $\Lambda > 0$ and the anti-de Sitter space if $\Lambda < 0$. And because empty spaces are non-physical, then the limit of GR when matter disappears also has no physical meaning.

I think the following remark is important. So far, there is no theory that works under all conditions. For example, classical mechanics works well at speeds much less than the speed of light, but it cannot be extrapolated to where the speeds are comparable to the speed of light. Another example is that classical mechanics cannot be extrapolated to describe the levels of the hydrogen atom. GR is a theory that describes well some phenomena at the macroscopic level where there are large masses (stars or planets), but it does not follow from anywhere that GR can be extrapolated to the limit when matter disappears. Meanwhile, this limit is used in the so-called dark energy problem (see below).

In addition, from the point of view of physics, it is meaningless to talk about empty space, because one cannot measure the coordinates of a space that exists only in our imagination. In particular, in GR, coordinates and time can only characterize measurable quantities for real particles. The problem is how to measure them. Landau and Lifshitz define the frame of reference in GR as a set of weightless bodies equipped with three numbers (coordinates) and each of these bodies has a (weightless) clock. Of course, from the point of view of our understanding of physics, weightless bodies do not make sense. But since GR is a purely classical theory, perhaps with some accuracy one can speak of a system of weightless bodies, although this looks rather artificial. It is believed that, in all available experiments, GR is confirmed with very high accuracy. A typical work in mainstream literature is when big calculations are made and it is concluded that GR is correct and this is another confirmation that Einstein is great. In such literature, no doubts about GR are allowed; the only thing that can be discussed (there is even an article with that title) is whether Einstein was 100% right or only 99% right.

It is stated that there are two types of experiments that confirm GR: three or four classical tests, in which the GR corrections are very small (my friend, who did not want me to give his name, called it flea catching) and experiments in which the effects of are GR strong.

A redshift experiment consists in sending light of a certain frequency from the surface of the Earth, and then measuring its frequency at a certain height. It is believed that the famous experiment of Pound and Rebka is a good confirmation of GR. But the interpretation of the experiment is far from unambiguous. It is generally believed that a photon loses energy like a stone thrown upwards from the ground. But Okun offers a completely different explanation, that the photon does not lose energy, and the effect is explained by the fact that the atomic levels on the surface of the earth and at a certain height are different. At the same time, he gets the same answer as in the standard interpretation, and he also concludes that GR is correct here. He writes that a photon cannot be compared with a stone. He writes the Weinberg wave equation for the photon and concludes that the photon does not lose energy. But the photon and the stone are just different particles, the stone is non-relativistic and the photon is relativistic. So, it is not clear why the photon should not lose energy. On the other hand, Okun's observation, that the energies of atomic levels on the ground and height H are different, also seems obvious. Apparently, both effects play a role, so the question of confirming GR depends on which effect is more important - the loss of photon energy or the difference in levels at height H. The strangest thing is that even in textbooks and large reviews dedicated to the centenary of GR, the question that the atomic levels on the ground and height H are different is not even discussed, as if this issue does not exist, and the effect is explained only by the fact that the photon loses energy.

The second famous effect is the deflection of a photon in the field of the Sun. The effect is that light from a distant star that travels past the edge of the Sun deviates from a straight path. The first deflection result of 0.875 seconds was obtained by von Soldner in 1801 and this result was confirmed by Einstein in 1911. But in 1915, when Einstein created GR, he got a result twice as large. In 1919, Eddington organized several expeditions to measure the total solar eclipse. Although the accuracy of the experiments was small, he concluded that the result was more consistent with Einstein's latest calculation. This immediately made Einstein much more famous.

After that, many experiments were carried out, and although their accuracy in the optical range is not very high, it was concluded that the result of GR was correct. Now it is believed that this result is confirmed with an accuracy better than 0.11% in experiment based on Very Long Based Interferometry (VLBI) in the radio band. There is a quasar, the radio beam from which every year in October passes the edge of the Sun and it is registered by two radio telescopes, one of which is in Massachusetts, and the second in California. When the public is told that the processing of the experimental data of these two radio telescopes confirms GR with an accuracy better than 0.1%, then it is practically impossible to verify this (because the only way to check is to check how the experimental data were obtained and to carry out numerical calculations of these data yourself) and all that remains is to believe.

This issue raises a question. The solar corona is very dense, and the standard result described in textbooks is obtained from the problem of two bodies - the Sun and a photon, and the corona is not taken into account. It seems rather strange to think that a photon passes through the solar corona practically without interacting with it. Probably, it is not necessary to mention the corona in textbooks, but even in the latest large reviews nothing is said about the corona, as if it does not exist.

A common practice for corona accounting is to measure the deflection at two radio bands. But this still does not guarantee correct accounting. The authors of the VLBI experiments write that they conducted the experiments when there was little corona activity. Even so, they write that "The confirmation of the result $\gamma = 1$ in VLBI experiments 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".

Further, the authors of the article Lebach et. al. (1995) write: "In Ref. [109] the following corrections have been investigated at different radio-wave frequencies: 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". So, the authors acknowledge that the answer is highly model dependent. Next, they describe a model for taking into account the beam delay in the coronal plasma.

So, it turns out to be a strange situation. On the one hand, the authors admit that the fundamental question of the correctness of GR in this case is strongly model dependent. On the other hand, they say that it is possible to choose models for different effects in such a way that the result of GR will be confirmed with an accuracy of 0.11%. Can the experiment be considered a strong confirmation of GR? It is clear that in mainstream literature one can only publish papers with the assertion that this is a strong confirmation of GR. And not in the mainstream literature there are papers where the authors claim the exact opposite. But these papers are hardly recognized.

The third classical effect of GR is the displacement of the perihelion of Mercury. Usually, the problem is described in such a way that it shifts by 43 seconds in a hundred years, the classical theory cannot explain this, and GR just gives a correction equal to these 43 seconds. There is such a point here that Le Verrier claimed that the results of observations from 1697 to 1848 gave a value of 38 seconds, but then scientists decided that 43 seconds is a more correct value than 38 seconds. In reality, the real deviation is 5600 seconds, but most of it arises from the fact that the Earth is a non-inertial frame of reference. If this effect is taken into account, then approximately 574 seconds remain. Calculations of celestial mechanics show that due to the interaction of Mercury with other planets, a correction of approximately 531 seconds occurs, and the remaining effects are small. So the remaining 43 seconds is less than one percent of the full offset. Some authors argue that this 43 second problem contains both experimental and theoretical uncertainties. However, in mainstream literature, anything that might be perceived as an attempt to tarnish Einstein's authority is not accepted.

As I already noted, in the three classical tests of GR, we are talking about very small corrections. In addition to them, there are effects that are interpreted in such a way that the effects of GR are strong in them. One of these known effects - gravitational radiation from a binary pulsar. The problem is this. Objects called pulsars are treated as neutron stars with a mass on the order of the sun and a radius of the order of 10 km. It is clear that such objects cannot be observed, for example, like planets. Here you can only register some radiation and study which models best describe it. In some cases, the best models indicate that we are dealing not with a single pulsar, but with a binary system in which one of the objects is a pulsar and the other is an ordinary star. These two objects revolve around a common center of mass and, according to GR, such a system should radiate gravitational waves. If they are close enough to each other, then they rotate with large accelerations and there is hope that the gravitational radiation of such a system can be registered.

The best-known case of such a binary system is PSR B1913+16, discovered by Halse and Taylor in 1974. The generally accepted model of this system contains 18 fitting parameters. In addition, it is necessary to take into account corrections that depend on quantities known with low accuracy. If we take for these quantities the values that are considered the most realistic, then the observational data in such a model show that the binary system emits gravitational waves, which are described by Einstein's quadrupole formula with an accuracy of better than 1%. Because of this, the system loses energy, and the rate of decrease in the orbital period is 76.5 microseconds per year, i.e., one second in 14,000 years.

So, with the help of many fitting (more precisely, adjustable) parameters, it is possible to adjust the description of the data in such a way that, allegedly, we observe gravitational radiation, and this is considered another triumph of GR. Even the authors of the model write that far from everything is clear in it. The following natural question also arises. The result on energy losses for gravitational radiation was obtained in the approximation when the problem of two point bodies is considered. But these bodies do not move in empty space, but in the interstellar medium, and they move quickly, so that they can lose energy due to deceleration in the interstellar medium. But words are spoken that since the radii of objects are small, then such an approximation is legal. Halse and Taylor received the Nobel Prize in 1993 and the formal formulation was that for the observation of a double pulsar. But everyone understands that it is implicitly meant that their observations are interpreted as indirect confirmation of the existence of gravitational waves.

The next step is this. It is said that the data on binary pulsars is an indirect confirmation of the existence of gravitational radiation, but it would be nice to detect it directly, because GR predicts that it inevitably exists. Therefore, multi-kilometer installations were built for the direct detection of gravitational waves. After more than 10 years passed after the promised discovery and nothing was found, they tried to explain it in such a way that there is radiation, but due to various reasons it is unobservable. It is clear that in this case no doubts about the infallibility of GR were allowed.

But on February 11, 2016, LIGO announced that on September 14, 2015 two of its installations - in the state of Louisiana and the state of Washington - detected gravitational waves directly. In fact, these installations registered some fluctuations. If you take these two curves and combine them, they are similar, but not quite the same. The difference in time is such that it looks like there was a wave at the speed of light, i.e., for example, a seismic cause is probably ruled out. I.e., they really found something.

Now how to understand what. They take the model that there are two black holes whose masses are $35M_\odot$ and $29M_\odot$, where $M_\odot$ is the mass of the Sun. They quickly rotate around each other and merge in 0.2 seconds, forming one hole with a mass of $62M_\odot$. That is, during these 0.2 seconds, $3M_\odot$ turns into gravitational radiation. Calculations can only be carried out numerically because speeds are of the order of $0.5c$, and the post-Newtonian approximation does not work. In this paper in Physical Review Letters, they refer to the calculations, but do not explicitly say how many fitting parameters are in the problem and what parameters. People on the Internet are wondering if it's 11 or more.

The fact that in 0.2 seconds $3M_\odot$ turned into gravitational wave energy is, of course, a grandiose event. LIGO co-founder Thorne says, "It is by far the most powerful explosion humans have ever detected except for the big bang," and Allen, 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." Therefore, something super grandiose has happened, and the only observable effect of this is that the path of the laser beam has changed by an amount less than the radius of the proton.

Several questions immediately arise here. First, nowhere in the literature have I found a sensible explanation of what a black hole consists of. They say that when gravity compresses a star, at first, due to reaction $p + e \to n + \nu$ a neutron star is formed. This is acceptable because the reaction $p + e \to n + \nu$ is known. But they say that if the mass is greater than $1.6M_\odot$, then even this packet of neutrons cannot resist gravity. I.e., a black hole no longer consists of neutrons, but then what? Nuclear physics cannot say what happens to a packet of neutrons under such gravity, i.e., this is some unknown type of matter (words are pronounced that quark-gluon plasma or something else unusual). And there are models that a black hole can have an electric charge, which is incomprehensible at all.

That is, we have an incomprehensible substance that has an enormous density and an anomalously small size. It is clear that standard classical theory does not work under such conditions, and the problem can be solved only by quantum theory, which is not constructed for such conditions. But the concept of a black hole is obtained from the Schwarzschild solution in GR, i.e., this concept is derived from a purely classical theory, which does not work under these conditions. And model calculations, which allegedly confirm that a black hole merger has occurred, were made within the framework of a purely classical GR.

The standard dogma is that gravity is the fourth force to be combined with the strong, the electromagnetic, and the weak. Strong interaction - exchange of virtual gluons, electromagnetic - exchange of virtual photons, weak - exchange of virtual W and Z bosons, and gravity - exchange of virtual gravitons. But then it's not clear what's going on. No real particles, including gravitons, can leave the Schwarzschild radius. But at distances much greater than the Schwarzschild radius, the gravitational field of a black hole is the same as that of an ordinary star with the same mass and spin. This means that virtual gravitons easily leave the Schwarzschild radius for very long distances. The only difference between real and virtual gravitons is that for real ones the square of the 4-momentum is equal to the square of the graviton mass, while for virtual ones it can be any.

But it can also be very close to the square of the graviton mass (and have a long lifetime). So, it's not clear.

So, it turns out that $3M_{\odot}$ of some incomprehensible substance was annihilated and all the annihilation energy went only into gravitational waves. There are no photons or other particles. After all, even, say, a neutron, although it is electrically neutral, has a magnetic moment and supposedly consists of charged quarks. Therefore, at such accelerations it will emit photons. Because there are only two LIGO installations, they cannot determine where the signal came from. They say that when they build the third one in India, they will determine it by three points. But the Fermi gamma-ray telescope simultaneously sees 70% of the sky. After this LIGO report, the people at Fermi wrote a paper that on September 14 there was some kind of weak signal 0.4 seconds after LIGO. But with such a grand event, a weak signal looks strange. In addition, 0.4 seconds is equivalent to 120,000 km, and the telescope is in orbit at a height of 500 km, i.e., does not agree.

We know the energy release of the Sun and it is 8 light minutes away from us. And this event was (supposedly) a billion light years away. Therefore, it is easy to estimate that during these 0.2 seconds the energy received by us is 1,000,000 times less than from the Sun. If we take an assessment that the energy release of Sirius is 10 times greater than that of the Sun, then we have received energy 100,000 times more than from Sirius. But no one saw anything and no traces. And even if everything really went only into gravitational waves, what, such a super-grand event would not affect anything?

I asked physicists if they believed this could happen. The answer depended on how the respondent treated GR. Proponents of GR believe that almost all of the energy has indeed gone into gravitational waves, while others doubt it. But since it can neither be proved nor disproved, then any point of view has the right to exist.

And finally, such a remark. Let's even assume that this explanation of the experiment is correct. This means that the next event can be registered only if it is as grandiose in scale as what (allegedly) was. How long to wait for this event? Nobody knows for sure. About a billion dollars have already been spent on LIGO and more will be spent. And if it doesn't happen?

But they have already announced the second event, which occurred on December 26, 2015, and also at a distance of about 1 billion light years from us. Here the scale is somewhat paler: the masses of black holes are approximately $14.2M_{\odot}$ and $7.5M_{\odot}$, and in one second "only" one mass of the sun was released. And, of course, again everything went only into gravitational waves, and no one saw anything. And again, although the model depends on (an incomprehensible number) fitting parameters, it is also declared that of all theories of gravity, the event is best described in the framework of GR. This is natural since the fitting parameters are chosen based on GR. So, most likely, such a scenario emerges that from time-to-time LIGO will announce the next detection of gravitational waves.

So far, this scenario is confirmed, and in 2017 LIGO received the Nobel Prize for these experiments. Probably, from a technical point of view, these experiments are really very complex. But, it seems, the Nobel Prize in physics should be given not for technical complexity, but for fundamental discoveries. The usual practice was that after the announcement of fundamental discoveries, they waited for many years for the discovery to be generally recognized. And here they waited a little more than a year, although the belief that this discovery is fundamental is far from universal.

I think that this story with the Nobel Prize for experiments in which there are many uncertainties and ambiguities is one of the indicators of the current state of science, when it is recognized not only that it is clearly fundamentally new (that is, of great importance for the development of science), but that what is supported by the establishment, which receives positions, grants, etc. for this.

2.3 On the problem of dark energy

If we proceed from the standard GR approach that the Lagrangian is linear in scalar curvature, then the resulting Einstein equations depend on two arbitrary constants: the gravitational constant G and the cosmological constant Λ. In the framework of GR, these constants cannot be calculated; they have the status of phenomenological constants, which must be chosen from the condition of the best description of the experiment.

A term with Λ leads to the so-called cosmological force, which, unlike the gravitational force, is directly proportional to the distance. If we formally set $\Lambda = 0$, then in the nonrelativistic approximation and in the linear approximation in G, Einstein's equations give Newton's law of universal gravitation, which describes well the observed data in the solar system. Therefore, it is natural to think that the quantity Λ is small enough so that the cosmological force is also small within the solar system. However, it cannot be ruled out that at much greater distances this force is not small. From a purely mathematical point of view, if the solution depends on two arbitrary constants, then there is no reason to assume that one of them is equal to zero. Some authors ask the question that since we accept a theory with one arbitrary constant G, then why can't we accept a theory with two arbitrary constants - G and Λ.

However, here comes into play the generally accepted philosophy of GR, according to which the curvature of space is created by matter. Therefore, in the absence of matter, empty space must be flat, and therefore Λ must be equal to zero. This issue was the subject of a dispute between Einstein and de Sitter, who considered scenarios for the development of the universe under the assumption that Λ is not equal to zero and introduced the spaces that are now called de Sitter spaces. It is a known historical fact that at first Einstein wrote his equations without Λ, but then, as follows from Friedmann's solution, the Universe is non-stationary. Thinking that it must be stationary, Einstein introduced Λ. But when Hubble discovered that galaxies were receding, Einstein said that the introduction of Λ was the biggest blunder of his life.

The generally accepted philosophy of GR is accepted in almost all textbooks written before 1998. For example, Landau and Lifshitz write in "Field Theory": "The introduction of a constant term into the density of the Lagrangian function, which does not depend at all on the state of the field, would mean attributing to space-time an irremovable curvature that is not associated with either matter or gravitational waves." However, in 1998, data were obtained that are interpreted in such a way that Λ is not equal to zero. As a result of further observations, it was concluded that Λ is positive and is determined with an accuracy better than 1%. This result posed a choice problem for experts in GR:

1) Recognize as incorrect the previous statements that only $\Lambda = 0$ is a physical choice (and, in particular, recognize that Einstein's statement that the introduction of Λ was the biggest blunder of his life is also wrong).

2) Try to explain the data based on previous dogmas that only $\Lambda = 0$ is allowed.

In view of what has been said above, and even proceeding from human psychology, one should not be surprised that the choice was made in favor of 2). The following "explanation" was offered. The term with Λ in the Einstein equations was moved from the left-hand side (describing the curvature of space) to the right-hand side (describing matter) and declared that this term describes some invisible matter, which was called dark energy. Then, based on the observed data, it turns out that dark energy contains about 70% of the entire energy of the Universe. After that, a large field of activity appears for researching various models of dark energy, conferences are held, grants are given, experiments are being prepared for future discoveries, etc.

The fact that the accepted philosophy of GR is not physical follows from several considerations. Let us note that currently there is no physical theory which works under all conditions. For example, it is not correct to extrapolate nonrelativistic theory to the cases when speeds are comparable to c, and it is not correct to extrapolate classical physics for describing energy levels of the hydrogen atom. GR is a successful classical (i.e., non-quantum) theory for describing macroscopic phenomena where large masses are present, but extrapolation of GR to the case when matter disappears is not physical. One of the principles of physics is that a definition of a physical quantity is a description of how this quantity should be measured. The concepts of space and its curvature are pure mathematical. Their aim is to describe the motion of real bodies. But the concepts of empty space and its curvature should not be used in physics because nothing can be measured in a space which exists only in our imagination. Indeed, in the limit of GR when matter disappears, space remains and has a curvature (zero curvature when $\Lambda = 0$, positive curvature when $\Lambda > 0$ and negative curvature when $\Lambda < 0$) while, since space is only a mathematical concept for describing matter, a reasonable approach should be such that in this limit space should disappear too.

It is generally accepted in physics that when new experimental data appear, one should first try to explain them on the basis of existing theories, and only when it becomes clear that this is not possible, one can look for exotic explanations. But in this story with dark energy, the situation was (and remains) completely opposite: the absolute majority of the establishment immediately supported dark energy, quintessence and other exotics, and there were almost no attempts to explain the data in terms of known non-exotic theories.

And the saddest thing is not even that, but that no other opinions are allowed in mainstream literature. As I noted above, one of the reasons for this situation is understandable - since Einstein said that empty space must be flat, then deviations from this are not allowed (and you can ignore the fact that empty space is physical nonsense). Well, another reason is that such exoticism opens up a large field of activity for new experiments, grants, etc.

My personal opinion is that dark energy is complete nonsense and, as shown in my publications, even from the principles of quantum theory it follows that Λ should not be equal to zero. Some of my papers where dark energy was not the main topic, were published even in mainstream journals (for example, even in Phys. Rev. D). But when I wrote papers that dealt only with the problem of dark energy, they were only published in those journals that do not belong to the mainstream. More on this below.

Finally, by analogy with the Nobel Prize in 1993, the Nobel Prize in 2011 was formally given with the wording of what kind of experimental research, but everyone understands that it was implicitly given because the data is interpreted as the discovery of dark energy. And in 2019, the Nobel Prize was given to J. Peebles. As members of the Nobel Committee said, he opened our eyes to the fact that we know only 5% of the matter in the universe, because approximately 69% is dark energy, and 25% is dark matter.

But, as explained in detail below, those 70% are far-fetched, they just don't exist. As for dark matter, the issue here is more complicated. The concept of dark matter arose due to the fact that the behavior of galaxies cannot be explained using ordinary concepts, and the explanation is obtained if we assume that these galaxies contain some unknown substance, which was called dark matter. Now many theorists and experimenters are exploring how to find particles from dark matter. This is a very serious activity and, of course, if dark matter is found, it will be a fundamental advance in our understanding of nature. On the other hand, what happens in galaxies is a complex issue and it is unlikely that we understand everything here. So, let's see where science comes in.

I think that the discussion in Sections 2.2 and 2.4 shows that GR has almost become a religion, and those who doubt GR have no chance of getting into the mainstream community. And this is despite the fact that GR is a purely classical theory, proposed more than 100 years ago, when people knew nothing about quantum theory, but when they did, Einstein became its great opponent. In one of his letters to Heisenberg, Pauli wrote that every time Einstein talks about quantum theory, "it's a disaster" (this is a translation because it is clear that Pauli wrote to Heisenberg in German).

One of my friends explains this situation as follows: the point is not that they love Einstein very much, but that for those who do not have their own ideas, GR makes it possible to live, because one can infinitely improve GR and set up experiments to test it.

I remember myself in my youth. All the time I was among those for whom Einstein's authority was indisputable. I went to the seminars of Ginzburg and Zeldovich just at the time when

Logunov and his co-authors proposed their alternative version of the theory of gravity. They wrote that, like classical electrodynamics, such a theory should be in the spirit of Faraday and Maxwell. At these seminars, Logunov's work was ridiculed all the time. Once I looked at a paper by Logunov, where there was such a phrase: "These two great scientists (meaning Einstein and Hilbert) dragged many generations of physicists into the wilds of Riemannian geometry." I thought, how is it that some Logunov who is almost nothing in comparison with Einstein is opposing him. But now I think that such an opinion is not necessarily sedition. Personally, I don't like Logunov's philosophy, but I think this phrase is absolutely correct.

2.4 Why quantum theory is more realistic than classical one

The usual explanation for the need for quantum theory is that some experiments cannot be explained in terms of classical theory, but quantum theory explains them. But I think that the main thing is not even this, but the fact that quantum theory is more natural than classical one.

The philosophy of classical theory is as follows We proceed from standard continuous mathematics and implicitly assume that all the symbols with which we describe physics (for example, x, t, dx/dt, etc.) refer to physical quantities that, in principle, can be measured with any accuracy. The existing quantum theory is also far from ideal, it has a problem of interpretation and other problems. But, at least, quantum theory tries to somehow answer the question of what a physical quantity is and with what accuracy the quantity can be measured. In particular, only those quantities are physical to which self-adjoint operators correspond.

However, although quantum theory has been around for almost a hundred years, there are problems in teaching it, and many of those who formally work on quantum theory do not understand it. I think that the situation is well characterized by this Gell-Mann observation. He taught quantum mechanics at Caltech and, according to his observations, there are three stages in its study:

1) The student solves the Schrödinger equations, finds energy levels and feels good. This stage lasts about half a year.

2) He begins to think what the meaning of all this is and suffers that he cannot understand. This stage also lasts about half a year.

3) One fine morning, he wakes up and wonders why he suffered because everything is clear and there are no problems.

The explanation is that he was trying to understand quantum theory from the classical point of view, which is impossible. But gradually he developed quantum thinking.

It seems to me that this observation applies not only to students, but also to many scientists who are formally considered quantum physicists. When I read thousands of articles on quantum theory, the impression is that many authors did not even have the second stage.

One example is the modern Big Bang theories. Here the task is to explain several parameters that characterize the modern universe. For this, models are created, where not only there are many parameters, but it is also assumed that the inflaton field is responsible for inflation, the particles of which no one has ever observed. Then the current state of the Universe is explained by the fact that there was once inflation, i.e., the universe expanded very quickly. For example, in one of the scenarios, the size of the Universe changed from $10^{-26}m$ to $1m$ and this happened in $10^{-35}s$. To describe this scenario, the quantum theory of the inflaton field and GR are used. So, it is believed that although general relativity is a purely classical theory, it can be applied at distances of $10^{-26}m$ and times of $10^{-35}s$. So, in the spirit of classical physics, that when we write $x = 10^{-26}m$ or $t = 10^{-35}s$, then we think that these expressions make sense. However, the concepts of coordinates and time originated from classical physics. These are quantities that can be measured with an accuracy no better than the size of an atom and $10^{-18}s$, respectively.

It is believed that the best accuracy in measuring time, $10^{-15}s$, is obtained by using the transition in the $Cesium^{133}$ atom, and there are claims that the accuracy can be improved to $10^{-18}s$. In inflationary models of the Universe, it is believed that inflation occurred when there were not only atoms, but even nuclei in the Universe, and then it is not clear whether time makes sense in such situations. In quantum theory, it is meaningless to say that "in fact" some physical quantity exists but cannot be measured.

From the point of view of quantum theory, it makes no sense to talk about coordinates $10^{-26}m$ and times $10^{-35}s$, because it is not known whether there is a coordinate operator on such scales and the problem of time is one of the fundamental unsolved problems of quantum theory. Moreover, for example, in the Copenhagen interpretation of quantum theory, measurement is an interaction with a classical object, and at this stage of the universe there can be no classical objects. But in the theory of the inflationary universe, these problems are not even discussed.

For example, words are spoken that quantum effects are important at the inflationary stage of the universe. But how can they be taken into account if there is no quantum theory under such conditions? For example, A. Starobinsky adds a new term to the classical GR Lagrangian, which he calls the quantum correction. But the fact that some term was added to the classical Lagrangian does not mean that the theory has become quantum. It remained completely classical because classical space and time and the classical principle of least action remained in it.

Another example is string theory or M-theory, which is claimed as the theory of everything. Here it is assumed that all physics will be derived from the topology of smooth manifolds on Planck lengths $10^{-35}m$. But in particle physics, distances are not measured directly. When it is said that some process occurs at distances l, it means that the momentum transferred in this process is of the order of $\hbar/l$. Then the Planck lengths correspond to momenta of the order of $10^{19}Gev/c$, which, probably, will never be achievable on accelerators. In addition, this assumes that the coordinate and momentum representations are related by the Fourier transform, and as shown in my publications, this assumption is not based on either the available data or reliable physical principles. Meanwhile, string theory and M-theory start from the basis in the coordinate representation, although the experience of quantum theory shows that the concept of continuous coordinates becomes problematic already at distances much larger than the Planck ones.

I also think that Big Bang and string theories can't be right based on Bohr's famous remarks. Somehow, during a discussion of a report at a seminar where he was present, someone said that the author's theory could not be correct because it's too crazy. To which Bohr objected that this theory could not be correct because it was not crazy enough. Big Bang and string theories are clearly not crazy enough because they assume that existing concepts operate at energies much greater than those we know.

But in general, it seems to me that the situation with the inflationary universe and string theory, like the situations discussed above with the so-called detection of gravitational waves and dark energy, characterize the degradation of modern physics when the establishment supports not fundamental theories but something exotic that has a chance to get (under the existing system) positions, grants, etc. True, as far as I know, for the inflationary universe and string theory (yet?) the Nobel Prize was not given, but other prizes were. For example, the Milner Prize of 3 million dollars is more than the Nobel prize. But there can be no objections here: Milner can give any bonuses from his own pocket to whomever he wants.

2.5 On mathematics in quantum theory

The title of Wigner's famous paper [1] is "The unreasonable effectiveness of mathematics in the natural sciences", and the paper ends as follows:

"The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it and hope that it will remain valid in future research and that it will extend, for better or

for worse, to our pleasure, even though perhaps also to our bafflement, to wide branches of learning."

Thus, Wigner considers mathematics not as an abstract science, but as an apparatus for describing nature. Since almost all my life I was among physicists, I also thought so. But recently, when I was preparing an article for Open Mathematics and talking with some mathematicians, I saw that they consider mathematics as a purely abstract science for which it does not matter whether it has applications for describing nature.

In principle, such an approach also has the right to exist, and history shows that many mathematical results, which at one time were considered purely abstract, eventually found their application in physics. But even if some results will not be of use, they may have a purely aesthetic value. For we do not demand that poetry or music have any applications for the description of nature. In poetry and music, the main thing is beauty, which cannot be expressed in words. In mathematics, as Dirac said, the main thing is the beauty of formulas. But there are some criteria here. Under the influence of M.A. Naimark's lectures at MIPT, I thought that the rigor of mathematical proofs is sacred for mathematicians, and they will never sacrifice this. But is it?

Classical mathematics uses the concepts of infinitely large and infinitesimal, which were first proposed by Newton and Leibniz over 300 years ago. Then people did not know about atoms and elementary particles and, based on everyday experience, they thought that any body can be divided into an arbitrarily large number of arbitrarily small parts. But from the very fact of the existence of elementary particles it follows that the usual division has a limited applicability. We can divide any macroscopic body into ten, a thousand, a million, but when we get to atoms and elementary particles, further division loses its meaning. For example, the energies of electrons in accelerators are millions of times greater than their rest energy, and such electrons experience many collisions with different particles. If the electron could be divided, then this would have been noticed long ago.

From this simple and known consideration, it would seem that it immediately follows that it is at least unnatural to apply classical mathematics to quantum theory. Therefore, a natural question arises whether quantum theory should be built on the basis of another mathematics. We can say that this problem arises if we consider that mathematics should describe nature, but if we consider mathematics as a purely abstract science (as many mathematicians believe), then this problem does not matter, and the main thing is that everything be rigorous.

In this approach to mathematics (let's call it Hilbert's approach), the goal of mathematics is to find a complete and self-consistent system of axioms in which it will be possible to conclude whether each mathematical statement is correct or not. This problem is formulated as the Entscheidungsproblem, which deals with statements and "Yes" or "No" answers, depending on whether the given statement is legal in any structure that satisfies the axioms. Is it possible to find such a system of axioms?

Classical mathematics contains facts that seem to contradict common sense. For example, the function $tg(x)$ is a one-to-one mapping of the interval $(-\pi/2, \pi/2)$ to $(-\infty, \infty)$, the function $2x$ is a one-to-one mapping of the interval $(0, 1)$ to $(0, 2)$ etc. Therefore, the part has as many elements as a whole. Another example is the paradox of Gilbert's Grand Hotel. But in Hilbert's approach, these examples are not considered contradictory.

Classical mathematics proceeds from axioms that are taken for granted, without proof. It would seem that since science is not a religion, then it should not contain statements taken for granted. Moreover, as follows from Gödel's theorems, any mathematics based on the set of all natural numbers contains statements that cannot be proved, and such mathematics cannot prove that it is self-consistent.

I asked mathematicians that if they claim to come from a rigorous science, then what about Gödel's theorems, which say that standard mathematics is not rigorous? But the usual answer is that since a theory based on the axioms of standard mathematics describes nature well, then such an approach is acceptable, and the entire history of mankind is considered to be confirmation of the assertion that classical mathematics, in principle, can describe any natural phenomena. That is, here mathematicians already abandon Hilbert's approach and believe that mathematics is not just an abstract science, but a science that describes nature. And, as I already wrote, the philosophy of many physicists is even more oak-headed. Although the existing quantum physics is based on classical mathematics, they believe that even the generally accepted rigor in this mathematics is not necessary, and most importantly, that the theory describes the experiment.

I asked physicists and mathematicians that since there are no infinitesimals in nature, then it turns out that the derivative is a non-strict concept. Some mathematicians answer that sooner or later the electron will be divided and this will prove that infinitesimals exist. Physicists generally agree that there are no infinitesimals in nature. They say that dx/dt should be understood as $\Delta x/\Delta t$ where Δx and Δt are small, but not infinitesimal. I tell them: but you are using math with dx/dt, not with $\Delta x/\Delta t$. And they say that since mathematics with derivatives works well, then there is no need to philosophize and invent something else (and they don't know other mathematics).

The history of physics shows that sooner or later the argument that if something works well, then there is nothing to philosophize, turns out to be wrong. For example, non-relativistic mechanics works well in 99.9...% of cases. But now we know that this is because in these cases the speeds are much less than the speed of light. And in cases where speeds are comparable to the speed of light, non-relativistic mechanics does not work. And since there are no infinitesimals in nature, sooner or later there will be cases when classical mathematics does not work. I discuss such cases below.

From the fact that nature is made up of atoms, it follows that standard geometric concepts (such as continuous curves and surfaces) can only work in an approximation where the size of atoms is neglected. For example, if we draw a supposedly continuous curve on paper and look at it through a microscope, we will see that the curve is highly discontinuous since it consists of atoms.

Historically, the founders of quantum theory and physics, who made a great contribution to this theory, although they were highly qualified scientists, their thinking was based on classical mathematics, and, say, discrete and finite mathematics were not included (and still are not included) into the program of standard physical education.

If classical mathematics correctly described all experiments, then, probably, one could come to terms with the fact that there are Gödel's theorems and hope that sooner or later they could be bypassed, and Hilbert's program could be executed. But the development of quantum theory has shown that within the framework of classical mathematics there are big problems in constructing what is called the ultimate quantum theory.

The main problem is that infinite expressions arise in the theory. In renormalizable theories (for example, in quantum electrodynamics, quantum chromodynamics, and electroweak theory), infinities can be eliminated by multiplying one singularity by another. But, for example, quantum gravity based on quantum field theory is a non-renormalizable theory and infinities cannot be eliminated in it.

As the famous physicist and Nobel laureate Weinberg writes about the problem of infinities in his textbook [2]: *Disappointingly this problem appeared with even greater severity in the early days of quantum theory, and although greatly improved by subsequent improvements in the theory, it remains with us to the present day*". Title of Weinberg's paper [3] is "Living with infinities".

2.6 On quantum field theory

Quantum field theory (which in the literature is called QFT) has no analogues in the history of science because, on the one hand, it describes some data with amazing accuracy, and on the other hand, it is based on incorrect mathematics. This theory is based on two main principles: 1) it comes from classical mathematics; 2) it proceeds from the concept of a quantized field on space-time. In the previous section, I argued that the most fundamental quantum theory cannot come from 1).

And now I will give arguments that such a theory also cannot proceed from 2).

What is classical field theory? Consider, for example, classical electrodynamics. It describes the classical electromagnetic field by the functions $\mathbf{E}(t,\mathbf{x})$ and $\mathbf{B}(t,\mathbf{x})$, where $(t,\mathbf{x})$ are Minkowski space coordinates . There are no spaces in nature; there are only particles, and when there are many of them, the illusion arises that they are in some space. In particular, the Minkowski space is only a purely mathematical concept. We know that the electromagnetic field is made up of photons. In the approximation when the coordinate operator works, each photon has its own coordinates.

But in classical electrodynamics, each photon is not considered separately, and all photons are described together by the functions $\mathbf{E}(t,\mathbf{x})$ and $\mathbf{B}(t,\mathbf{x})$. This is analogous to the fact that statistical physics does not consider each particle separately but describes ensembles of many particles with functions (temperature, pressure, etc.) that do not make sense for each particle. It is clear that such a description can only be approximate.

Now let's discuss QFT. In quantum theory, there is information about each individual particle. In particular, in the approximation when the coordinate operator works with good accuracy, each particle is described by its own coordinate. In this approximation, the wave function of a system of N particles is described by the function $\psi(\mathbf{x}_1,\mathbf{x}_2,...\mathbf{x}_N)$ and there is no $\mathbf{x}$ coordinate common to all particles.

In QFT textbooks, the logic is as follows: special relativity is made on the Minkowski space, and the Poincare group is a group of motions of this space, then in quantum theory transformations of states must be described by representations of the Poincare group, which means that the generators of such transformations must satisfy the commutation relations of the Lie algebra of the Poincare group. This approach is in the spirit of Felix Klein's Erlangen Program.

Here there is an analogy with the situation in GR. The Erlangen Program was proposed even earlier than GR - in 1872, when there was no trace of quantum theory. But, as noted above, from the point of view of quantum theory, the concept of background space does not make sense, since there is no coordinate $\mathbf{x}$ common to all particles. But this concept is still widely used in the so-called fundamental quantum theories - QFT and string theory.

My supervisor, Leonid Avksent'evich Kondratyuk, explained to me that the logic should be the opposite of that applied in the spirit of the Erlangen Program. What are usually called generators are just the main physical operators - energy, momentum, angular and Lorentz angular momentum operators. The Poincare symmetry is not because there is a Minkowski space (which is a purely classical concept), but because the basic physical operators satisfy the commutation relations of the Poincare algebra and therefore transformations and the Minkowski space arise at the classical level (and only at this level).

So, at the fundamental quantum level, symmetry is specified not by space, but by the algebra of commutation relations, and at this level there are no spaces and their transformations. They arise only in the classical approximation, since in this approximation, space appears not as an abstract empty space, but as an event space for bodies. Maybe this idea is implicit in Dirac's paper [4], but it is not formulated there as explicitly as Leonid Avksent'evich's. When later I met Skiff Nikolaevich Sokolov, he also said that he had come up with such an idea.

In QFT, elementary particles are described by irreducible representations of the Poincare algebra. In such a description, there are no coordinates and Minkowski space at all, but there are only momenta, angular momentum and spins. At the same time, there is a probabilistic interpretation, since the operators of physical quantities are self-adjoint operators. But, as proved in representation theory, from a mathematical point of view, there is often a correspondence between the representations of a certain Lie algebra by self-adjoint operators and the unitary representations of the Lie group corresponding to this algebra.

But QFT also considers the description of particles using field functions $\Psi(x)=\Psi(t,\mathbf{x})$ satisfying covariant equations (Dirac, Klein-Gordon, etc.) on the Minkowski space. Such functions arise from non-unitary representations of the Poincare group induced from non-unitary representations of the Lorentz group, and the dependence of such functions on $(t, \mathbf{x})$ arises from the fact that the Minkowski space is the quotient space of the Poincare group by the Lorentz group. Due to the fact that such representations are nonunitary, a problem arises with their probabilistic interpretation.

Pauli showed that for equations describing fields with half-integer spin there are no invariant subspaces in which the sign of the energy is the same for all states, and for equations describing fields with integer spin there are no invariant subspaces in which the charge sign is the same for all states. Therefore, non-quantized fields describing particles have no physical meaning. Moreover, since for the fields $\Psi(x)$ there is no probabilistic interpretation, then the coordinates x are not operators of any physical quantities. The great success of the Dirac equation is that, in the $(v/c)^2$ approximation, the equation describes fine levels of the hydrogen atom with great accuracy. But, in higher approximations, it does not work. For example, it cannot describe the Lamb shift.

A big event in particle physics was Dirac's result that his equation has a solution with both positive and negative energies. This fact was interpreted as the existence of antiparticles, and indeed, the positron was soon found. But here such contradictions arise.

If m is the particle's mass and $\mathbf{p}$ is its momentum, then the energy is defined as $\omega(\mathbf{p}) = (m^2 + \mathbf{p}^2)^{1/2}$, moreover, from a purely formal point of view, the sign of the root can be either positive or negative. But this sign must be the same for all particles. Indeed, consider a system of two particles whose masses are the same and whose momenta are $\mathbf{p}_1$ and $\mathbf{p}_2$ such that $\mathbf{p}_1 + \mathbf{p}_2 = 0$. Then, if for one particle the root is taken with a plus sign, and for another with a minus sign, then the total 4-momentum of the system will be equal to zero, which contradicts the experiment.

Another contradiction is the following. Since the Dirac equation is linear, the superposition of solutions with positive and negative energies is also a solution, and this corresponds to the principle of superposition in quantum theory. But from the charge conservation requirement, it follows that the superposition of electronic and positron states is forbidden.

These contradictions are solved with the help of second quantization. But then a problem arises. The quantized field $\Psi(x)$ is the operator in the Fock space consisting of an infinite number of particles. Each particle has its own coordinates (in the approximation when operators of such coordinates exist). Therefore, the argument of the function $\Psi(x)$ does not refer to any particle, it is just a purely formal parameter arising from the second quantization of the non-quantized field $\Psi(x)$. Therefore, the argument cannot even be called a coordinate, it is simply an integration parameter when the Lagrangian is written as an integral of the fields. That is, in the quantum case, the argument has no physical meaning. But all the same (it is not clear why), physicists think that the argument has the meaning of a coordinate.

In QFT, the field functions $\Psi(x)$ appear only in the integrals of the Lagrangian over d^4x for the S-matrix, that is, x is only an integration parameter and there are no physical quantities depending on x. The goal of QFT is to calculate the S-matrix in momentum representation, and all observable quantities in QFT are defined by the S-matrix. Once it is calculated, we can forget about x. This corresponds to Heisenberg's S-matrix program, that in quantum theory it is possible to describe not the states at each moment of time t but only transformations from the infinitely distant past to the infinitely distant future. The fact that the S-matrix is calculated in the momentum representation does not mean that in QFT one cannot have a coordinate description. It exists in the approximation when for each particle there is a coordinate operator in the momentum representation.

Summarizing the discussion in this and the previous sections, we note the following. QFT rests on two whales indicated in 1) and 2). The fact that 1) is not a fundamental physical requirement was noted in the previous section, and in this section, it is explained that the concept of quantized fields on background space is also not fundamental. The concept of background space originated from classical field theory, and for quantized fields it has no physical meaning, since the argument x in quantized fields does not refer to any particle and therefore has no physical meaning. There is no physical law that the S-matrix must necessarily be determined by integrals over d^4x of the quantized fields $\Psi(x)$. Historically, QFT with such integrals gave a good fit for many experimental data, but as discussed below, such a theory also has fundamental problems. Therefore, there is no reason to think that the ultimate quantum theory will be based on QFT. This issue is discussed in the next section.

2.7 Successes and problems of QFT

As explained above, a theory based on 1) and 2) cannot be fundamental. But, besides this problem, the following one arises in QFT. The theory is based on local quantized fields $\Psi(x)$, which are multiplied at one point. As a rule, physicists do not care that, as noted, for example, in the book by Bogolyubov et al. [5], $\Psi(x)$ is a distribution, and, as is known from the theory of such functions, they cannot be multiplied at one point. But many physicists do not even think about it and multiply in order, as they think, to preserve locality, although, as noted above, x does not refer to any particle and therefore has no physical meaning. As a result, ill-defined expressions, anomalies and divergences that are struggled with are obtained. That is, physicists themselves created problems and are now struggling with them.

We can say that an ideal science should not start from such mathematics. But here a deadly argument arises: with such mathematics, the theoretical result for the magnetic moments of the electron and muon agrees with the experiment with an accuracy of 8 digits, the Lamb shift with an accuracy of 5 digits, and so on. There is no such agreement between theory and experiment in any field of science.

These results were obtained in quantum electrodynamics (QED) at the end of the 40s, and those who did it (Feynman, Schwinger, Tomonaga, Bethe, Karplus, Klein, Kroll, Sommerfield, etc.) produce impression not even of people, but of supermen. But still, although history does not know the subjunctive mood, let me ask a seditious question: the fact that these amazing results were obtained turned out to be good for science or not?

Firstly, these results immediately convinced many physicists that rigorous mathematics is useless, and most importantly, that the experiment should be well described. Secondly, many have decided that now the whole relativistic quantum theory can be made similar to QED. However, despite the amazing agreement with experiments, these results can hardly be considered fundamental. They are obtained based on the fact that the fine structure constant α is small (it is approximately equal to 1/137). Therefore, perturbation theory with respect to α can be applied. The result for the anomalous moments of the electron and muon is obtained by taking into account corrections up to α^3 inclusive. But in theories where the interaction constant is large, one must either work without perturbation theory or calculate the entire series of perturbation theory, which is unrealistic (and it is also not clear whether the series converges or not).

After such a triumph, physicists tried to consider other theories by analogy. In the previous section, I noted problems with the classical and quantum field $\Psi(x)$, with the interpretation of the argument of this function, with the Dirac equation, and so on. By the end of the 1960s, there was an opinion that something had to be changed. Weiskopf wrote that quantum field theory should be buried with full honors. In 1968 the 4th volume of the Course of Theoretical Physics was published, which was written by Berestetsky, Lifshits and Pitaevsky. In the introductory chapter, they explained that if quantum theory is combined with relativism, then even the coordinate itself cannot be accurately measured, and in chapter II they wrote: "The auxiliary character of the concept of the field of free particles should be emphasized".

But despite these problems, QFT rose from the ashes: in the 70s, quantum chromodynamics was created, in 1981, W and Z bosons were found, and finally, the Standard Model was created. In it, based on 20 parameters, many experimental data from particle physics are described. The model did not solve any fundamental problem of QFT. It still comes from the Lagrangian in which the fields are multiplied at one point. Even when I studied at ITEP, everyone knew the catch phrase of K.A. Ter-Martirosyan that if a theory contains 25 free parameters and describes 1000 experimental data, then this is a good theory. So, in that kind of philosophy, the Standard Model is a big achievement.

Now the year is 2023, and can we say that there is some progress in creating a unified theory? It seems to me that, again, such a period has come when, by analogy with the end of the 60s, it became clear that a unified theory cannot be built on the ideas of QFT. There was a big buzz that string theory would become TOE. Above, I argued that this is very doubtful. In the spirit of Bohr's phrase above, one could say that string theory is not a crazy enough generalization of QFT. One of its ideas is that because string fields are multiplied not at a point (a zero-dimensional object), but on a string (a one-dimensional object), there is hope that the singularities will "smear out" and it will be possible to work with them. However, from the point of view of mathematics, multiplication on a string is also not a correct operation, and the problem of infinities has not been solved in string theory either. In connection with string theory, the known phrase comes to mind that you can fool many people for a short time or few people for a long time, but you cannot fool many people for a long time. It seems to me that string theory has refuted this assertion, since it managed to deceive many people for a long time. In many departments of physics, it has become impossible to get a job if you don't work with strings. As Dyson said, if in the past it was necessary to do expensive experiments to show that the department was dealing with fundamental problems, now it is enough to take one or two string theorists instead.

String theorists might say that it has already shown its importance because many strong mathematical results have already been obtained in it. Maybe it is, I can't judge. Many mathematicians in topology, smooth manifolds, and so on, have gone into this theory. If mathematicians have found many interesting problems for themselves in this, then, as they say, good luck. Mathematicians do not need to think about whether smooth manifolds on Planck lengths have any physical meaning. But then it's just math and there's no need to proclaim that it will be TOE.

Besides string theory, there have been other attempts to generalize the standard QFT, such as in the noncommutative geometry approach and in loop quantum gravity (LQG). In all these attempts background space is a required attribute. As I already noted, this is a purely classical concept. The history of quantum theory says that there is no need to drag concepts from classical theory here. For example, even in a non-relativistic theory it is impossible to measure the momentum and coordinate independently, and in a relativistic one it is impossible to measure exactly the coordinate even by itself.

Many physicists who build quantum theories of gravity think that background space in quantum theory should be such that in the classical limit it becomes the background space of GR. For example, LQG is based on such a philosophy. But my result on cosmological acceleration (see Chapter 5) shows that the result of GR in the semiclassical approximation is obtained without any background space in quantum theory.

Among physicists there are many supporters of the Landau philosophy, that rigorous mathematics is useless, and the main thing is to describe the experiment. But here I would make such a remark. There are many memoirs about Landau, where he is described by no means as a pleasant person in all respects. I can't judge because communicated only with his students. Many of them, probably, adopted such traits of him as peremptory and intolerant of other approaches. But there is no doubt that Landau was honest and wrote what he thinks, regardless of the situation. For example, while investigating the so-called zero-charge problem, Landau and Pomeranchuk concluded that QED cannot be a self-consistent theory, and this contradicts the philosophy of many current Landauites (and many of them do not even know that in connection with the problem of zero -charge Landau made such a conclusion).

Perhaps the Standard Model is the pinnacle of the "Landau" approach, but no further progress is in sight. And string theory is, in a sense, the opposite of this approach because it does not have any experimental confirmation, and moreover, it is not even clear in which experiments it can be verified.

Chapter 3

Remarks on the development of science

As Weinberg said, the new theory may be "centuries away". This point of view differs from the assertions of "stringers" (which, however, have greatly subsided recently) that string theory will be TOE. Most likely, Weinberg is right. Indeed, all the resounding successes of fundamental quantum theory (for example, the anomalous magnetic moments of the electron and muon, the Lamb shift, etc.) were obtained in perturbation theory. And where it does not work (for example, to calculate the masses of elementary particles), the situation looks like complete hopelessness.

It would seem that since such a situation has existed for many decades, something is wrong in the Kingdom of Denmark and fundamentally new approaches must be sought. That something is wrong is evident, for example, from the remarks in section 2.5 that describing quantum phenomena using continuous mathematics is unnatural to say the least.

Many physicists belonging to the establishment recognize that something is amiss. For example, Weinberg in his three-volume book on QFT writes that it should be considered "in the way it is", but at the same time it can be "a low energy approximation to a deeper theory that may not even be a field theory, but something different like a string theory". But from this quote it is clear that Weinberg thinks that something more fundamental will be done within the same continuous mathematics. And so thinks almost the entire establishment.

I.e., the establishment thinks that, in general, everything is going well, and that fundamental physics should develop along an evolutionary path, not a revolutionary one. And the main money allocated for the so-called fundamental physics go to numerous experiments, which in fact have nothing to do with fundamental physics.

For example, GR has been around for more than a century, it is constantly being tested and discussed whether it is the best (classical) theory of gravity or not. Let's assume that after another 100 years, after numerous experiments, it will be established that it is really the best and the answer to the question whether Einstein was 100% right or only 99% right will be: 100%. What will it give for the development of fundamental physics?

When GR was created, there was no quantum theory yet, but now we know that the ideas of classical physics look at least naive. For example, from the point of view of quantum theory, the discussion of the question of the curvature of space looks very strange. Therefore, it would be just interesting if it turned out that even at the classical level, the nature of gravity is not the same as it appears in GR. But since all efforts of establishment are aimed at proving the correctness of GR, then it is unlikely that anything else will break through here.

Another example is from quantum physics. The search for supersymmetry and the Higgs boson were announced as the main goals of the LHC accelerator. Supersymmetry has not been found, and there is a lot of controversy around the alleged discovery of the Higgs boson, and many more experiments will be carried out. But however, the question of the Higgs boson is resolved, it does not matter for understanding why there are divergences in the theory and how to go beyond perturbation theory.

So, with the current establishment approach to fundamental physics, there are no prospects that any fundamental discoveries will be made. Therefore, by analogy with Russian political discussions, two key questions arise: who is to blame and what to do?

It would seem natural to think that in order for science to develop, scientists must have opportunities to engage in science, i.e., at least, scientists must be paid money for doing science. It is immediately clear that it is impossible to pay everyone who said that he is a scientist and wants to do science, i.e., there must be some kind of selection system. Who should decide who gets paid and who doesn't? People who judge science based solely on what they have read in popular literature probably think that these issues are decided by established scientists with high moral character and that scientists are, as a rule, decent people. For example, one of the famous examples is how Einstein and Bohr argued about quantum theory, how physicists argued at the Solvay Congresses, etc.

It would seem that the principle that "truth is born in disputes" is obvious to science and that in science there should not be a situation like in the former USSR, where only communist ideology had the right to exist. If you look at what is being done in science from a purely formal point of view, then you might think that all high moral criteria are met here. Formally, important decisions are made at scientific councils, where there can be different points of view, there are many scientific journals that swear in their editorial policy that everything will be considered honestly, etc. In this regard, a comparison arises that the Stalin constitution of 1936 in the USSR was very democratic, there were freedom of speech, assembly, etc. But everyone knew what it really meant.

Probably, the judgment of each scientist about these issues is mainly determined by how his life turned out, what kind of people he met, who helped him, who hindered him, etc. Maybe for many, everything really worked out as it should in theory, and in their case the system worked according to the highest moral standards. In my scientific career I have met many scientists and many of them were indeed people of the highest moral standards. For example, as I wrote, Leonid Avksent'evich Kondratyuk and Skiff Nikolaevich Sokolov had a great influence on me. But, as I show below with many examples, in my case, the vast majority of people from the establishment, on whom I depended, turned out to be people without great moral principlest. And it turned out that what is written in the so-called editorial policy of prestigious journal very often - just a lie and no one is going to carry out these editorial policies. And yet it turned out that the so-called big establishment scientists who do bad things don't care at all if their actions get known, their reputation suffers, and so on. In chapters 5-9, my relationship with these people and with the so-called prestigious journals are described in detail, and the reader himself will be able to judge whether my conclusions are correct or not.

It would seem that those who are offered to be reviewers should first look at the editorial policy and decide whether they can write a review in accordance with this policy. But, apparently, for the vast majority of reviewers, editorial policy does not matter much, and they think they know better what papers can be published. And the problem is that in pure science (for example, in theoretical physics or mathematics) there are no clear criteria for which papers should be published. Therefore, it all depends on which reviewer you get. I wrote negative reviews only when I clearly showed where the error was in mathematics and the results of the article depended on these errors. But even in these cases, articles were sometimes published because, apparently, the author "had a connection", i.e., there was someone powerful behind him. And when I started working in a software company, I saw that the criteria here are completely different and very simple: if your programs are sold, then they are good, and it doesn't matter if the program is beautiful, what language it is written in, etc.

I think that the main reason for such an impasse in modern quantum theory as it is now is not even that new ideas are difficult to come by, but that with such a degradation of moral principles as it is now, new ideas have practically no chance to somehow break through. Big people from the establishment, who have received their positions and decide who to give or not positions and grants, stand to the death not to miss at least something that (as they think) can cast at least some shadow on the dogmas on which they achieved their position. Therefore, I think that the answer to the first question in the title of this section is this: the system in which science now exists is to blame. Accordingly, the answer to the second question is: to change the system. But a natural question arises: how?

The system should be such that people who commit dishonorable acts should know that this will be known, their reputation will suffer, and they will become worse from this. But now their reputation is almost irrelevant. The problem is that the absolute majority of scientists put up with such a situation. The generally accepted system is such that if you are not considered a great scientist, then you should deal with some particular tasks and not go into high matters. If you do not follow this unwritten rule, then you will not be included in the grant, you will not be given a position, etc.

It seemed to me that if someone received a Nobel or other prestigious prize, then after that he can afford the luxury of not doing dishonorable acts. But, as shown in chapters 5-9, this is not always the case. Maybe the fact is that these people received their bonuses within a system where high moral qualities are not quoted. In addition, the seditious question arises whether they really received their awards for great scientific achievements. More than 30 years have passed since the discovery of the W and Z bosons, and since then no basic dogmas have changed. Everything also revolves around GR and QFT, and theories that generalize or unite them have not yet been built. It seems to me that the only hope in this situation is if some fundamental discovery nevertheless breaks through and those who do it turn out to be decent people.

The plan for further presentation is as follows. In chapter 4 I will describe the main ideas on which my results are based. I hope that those who want to understand will agree that the ideas are reasonable and radically new. Therefore, it would seem that the development of approaches based on these ideas has the right to exist. In chapters 5-9 I will describe my attempts to publish works based on these ideas and this story confirms what was said above.

Chapter 4

The main ideas on which my works are based

For my work, it turned out to be important that, as described in Sec. 2.6, Leonid Avksent'evich Kondratyuk explained me that at the fundamental quantum level, symmetry is specified not by space, but by the algebra of commutation relations, and at this level there are no spaces and their transformations. From what follows it will be clear why this idea is fundamental.

4.1 de Sitter symmetry

A story that had an impact on me was this. My then head of the laboratory N.V. Kuznetsov had Dyson's printed paper "Missed Opportunities" in Russian. This article was wrapped in paper that had a photo of a pretty girl in a bikini and N.V. Kuznetsov joked that the photo illustrates well the title of the article. As far as I understand, the main idea of the paper is as follows. Dyson dealt with both physical and mathematical problems. He writes that when he was doing mathematical problems, his brain worked like a mathematician and he passed by important physical ideas, and similarly, when he was doing physical problems, he passed by important mathematical ideas.

For example, relativistic theory is more general than non-relativistic one, not only from physical considerations, but simply because the Galilean group is a special case of the Poincare group: the Galilean group is obtained from the Poincare group by contraction. And the de Sitter group is more general than the Poincare group, since the Poincare group is obtained from the de Sitter group by contraction. And since the de Sitter group is semisimple, it cannot be generalized further. It would seem that it should immediately follow from this that theories claiming to be considered fundamental (for example, QFT) should be built on de Sitter symmetry, and not on Poincare symmetry.

There have been some attempts in this direction. For example, I remember that I was at a lecture by V.G. Kadyshevsky at the Polytechnical Museum, where he said that for de Sitter, divergences are eliminated better. Now many are engaged in de Sitter theory, but how? More on that below. But Dyson's paper appeared in 1972, i.e., more than 50 years have passed, and textbooks on QFT are still based on relativistic invariance (i.e., Poincare symmetry) and all the most high-profile projects are based on this invariance.

From my discussions with particle physicists, I have this impression of a probable cause. Many of them know that the de Sitter symmetry is formally more general than the Poincare symmetry, and that the latter is obtained from the former in the formal limit $R \to \infty$, where at the classical level, R is treated as the radius of the universe. And because this radius is much greater than the size of elementary particles, they think that de Sitter symmetry may make sense in cosmology, but there is absolutely no need to apply it to elementary particles. However, a more general theory may shed a very different light on the standard concepts and, as described below, in the case of de Sitter symmetry this is indeed the case even for elementary particles.

Many years later I wrote to Dyson that his paper had impressed me and, in the spirit of that paper, finite mathematics (which is discussed in Sec. 4.3) is more fundamental than classical. I also wrote in particular: "*Most physicists and mathematicians believe that standard continuum math is fundamental while finite math is something inferior. They do not care much that standard math has foundational problems and even such beautiful minds as Cantor, Gödel, Hilbert, Zermelo and many others could not solve them. I give simple arguments that the situation is the opposite: standard math is only a special case of finite one in the formal limit when the characteristic of the ring or field in finite math goes to infinity. So the foundational problems of standard math are not fundamental. Maybe this is not politically correct to say but I believe that by introducing infinities people created a headache for themselves and now heroic efforts are needed to get rid of this headache.*"

I hoped that he would support me. But his answer was this: "*No useful comments. Whether you prefer Galois fields or a continuum is a matter of taste. To my taste, Galois fields are beautiful but the continuum is even more beautiful. Yours, Freeman Dyson.*" Well, thank you for that. In any case, he did not say that I wrote nonsense, encroach on the sacred, etc. But I was disappointed that even such an educated physicist and mathematician does not recognize, as it seems to me, the obvious. What then can be expected from others? I will return to this issue below. Now I'm trying to remember when I read this paper by Dyson. I think it was around 1977. This assessment is based on the fact that the paper was read in the apartment of N.V. Kuznetsov in Khabarovsk, where he asked to live during his departure. I began to live in Khabarovsk after defending my Ph.D. at the end of 1976, and at the beginning of 1978 the Institute gave me some kind of housing, so I had no need to live with someone. And then the question may arise. I criticize physicists for not switching from Poincare to de Sitter immediately after Dyson's paper, but why didn't I immediately switch myself? I'll try to make some excuses.

I used to think that after defending my Ph.D. I would not even try to do a Dr. Sci. thesis and will do what I want. When a PhD scientist received the position of senior researcher, his salary in Khabarovsk was 360 rubles per month, because the base salary was 300 and the Far Eastern coefficient was 1.2. With such money it was quite possible to live well and not think about anything. But N.V. Kuznetsov did not want to give me a senior researcher and, moreover, life began to deteriorate. Therefore, I began to think that I would have to do a Dr. Sci. thesis. And because I lived far from Moscow, then the possibilities of contacts with scientists were limited, and I decided that the only real opportunity for me to do a Dr. Sci. was the theory of relativistic direct interactions, which I wrote about above. It took almost all the time and therefore it was impossible to seriously engage in something else.

But an additional impetus to de Sitter was given by a conversation with my relative and then boss, Edik Mirmovich. Once he told me about his idea that the fundamental physical quantities are angular momentum. I tried to understand what he meant. I remember I told him that there are 10 generators in the Poincare group, 6 of them describe ordinary and Lorentzian rotations, but the remaining 4 - energy and momentum - are no longer rotations. Asked if he meant de Sitter. Here all 10 generators are angular momentum. Of these, 6 are the same as in Poincare, and the remaining 4, under de Sitter contraction in Poincare, pass into energy and momentum. So, at the quantum level, this idea is exactly what is written in Dyson's paper.

After this conversation, I had a hope that I would be able to study de Sitter not only in my free time, but also during work hours. Alas, it turned out to be only a hope and I will not describe why. But I managed to publish several papers in the Journal of Physics A: Mathematical and General, which was very decent then, but now it has become condo (see below). Perhaps the most important result is this. In the spirit of Wigner's famous paper, elementary particles are described by irreducible representations of the symmetry group. So, in the Poincare invariant theory, these are the representations of the Poincare group, and in the de Sitter invariant theory, the representations of the de Sitter group. Even more precisely, in the spirit of L.A. Kondratyuk, one should consider not representations of groups, but representations of the corresponding Lie algebras.

In representations of the Poincare algebra, the spectrum of the energy operator is either strictly positive or strictly negative. The first representations are associated with particles, and the second with antiparticles. But in the so(1,4) de Sitter algebra, one irreducible representation contains states with both positive and negative energies. In the limit $R \to \infty$, one irreducible representation of the $so(1,4)$ algebra splits into two irreducible representations of the Poincare algebra for a particle and its antiparticle. Therefore, from the point of view of de Sitter symmetry, the very concepts of particle and antiparticle are only approximate. And the laws of conservation of electric charge, baryon and lepton quantum numbers can only be approximate. Now they work well because at this stage of the evolution of the Universe, the value of R is very large. But if the universe came from something small, then in its early stages, R was not large and all these conservation laws did not take place. It is possible that the explanation of the problem of the baryon asymmetry of the universe is just such (see chapter 10). In any case, this example shows that, whenever possible, one must deal with a more general theory, even if it seems that a less general theory is a sufficient approximation.

4.2 On physical dimensions

Before describing my approach based on finite mathematics, I will make such a remark. In physics based on finite mathematics, all physical quantities can only be discrete. In such a situation, it is not clear whether the dimensions of physical quantities and the relationship between different units of measurement make sense. Dimensions have been around for 300 years or more and are still talked about a lot. But quantum theory and relativism clearly hint (although they don't even write it in textbooks) that there may be a different view on dimensions. For example, quantum theory says that angular momentum can only be integer or half-integer in units $\hbar$. Historically, angular momentum has been measured in units of m^2kg/sec. But this is optional. At a fundamental level, angular momentum is simply an integer or half-integer, i.e., you can forget about $\hbar$ altogether. Many write that they work in a system of units, where $\hbar = 1$. This is clouding because gives the impression that we are converting from one unit to another. And in fact, this means that $\hbar$ can be forgotten. The transition from quantum theory to classical is not $\hbar \to 0$, but simply when the angular momentum is very large. This example is also instructive in that it shows that when a discrete quantity is large, it appears to be continuous.

Another example is that relativism says that c is a fundamental constant and that no velocity v can be greater than c (not counting tachyons). But this means that in the continuous relativistic theory the velocities can be considered dimensionless. Roughly speaking, they can be measured in units of v/c. But in fact, this means that in such a theory, velocities should be measured in quantities less than one, and c can be forgotten altogether. Then the transition to nonrelativism is not when $c \to \infty$, but a particular case of the situation when all $v \ll 1$.

Finally, in the de Sitter invariant theory there are only angular momenta and there all operators have the same dimensions - they are all dimensionless and the masses are dimensionless. De Sitter mass μ and standard mass m are related by (implicitly assuming that $c = \hbar = 1$) $\mu = mR$, where R is the contraction parameter from the de Sitter algebra to the Poincare algebra. This parameter can be called the de Sitter radius (radius of the world), but in general this parameter has nothing to do with the radius of the de Sitter space; as shown in my papers, this takes place only in the semiclassical approximation. It is said that de Sitter passes into Poincare in the formal $R \to \infty$ limit. But in fact, this means that one can forget about the length parameter R and the formal transition from de Sitter to Poincare is obtained when the de Sitter analogues of ordinary energy and ordinary momentum are very large.

So, in the most general approach, when we have a quantum theory with de Sitter symmetry, there are no dimensions at all, and all physical quantities are measured simply by numbers. It is usually believed that there are no parameters in classical theory, c appears in relativism, $\hbar$ in quantum theory, and G in gravity, and these are three fundamental constants. Okun wrote a paper about the cube of physical theories, where there is a cube whose vertices have coordinates determined by the quantities $(0, c, \hbar, G)$ and the theory is the more general the more it depends on $(c, \hbar, G)$. And the most general theory is at the very last vertex of the cube with coordinates $(c, \hbar, G)$.

But in fact, the situation is the opposite. There are no parameters in the general theory, but in the classics there are three parameters - kg, m, sec. These parameters were invented by people many years ago and called the system of units. There is no fundamental meaning in these parameters, it just happened historically. The conclusion - the concept of units of measurement - is far-fetched, it arose only due to historical reasons. For example, one might ask why $c = 300000km/sec$ and not $100000km/sec$. The answer is: because we want to measure velocities in km/sec. Likewise, it doesn't make sense to ask why $(\hbar, R)$ are as are. Therefore, I do not see much point in reasonings about the importance of one or another unit of measurement.

4.3 Why finite mathematics is the most fundamental and fundamental quantum theory will be based on finite mathematics

The idea has always been in the air that the only way to avoid divergences in quantum theory is if the theory is based on finite mathematics. There were some attempts in this direction, but they were not popular with physicists. Probably one of the main reasons is that, as a rule, physicists do not even know the very basics of finite mathematics. Mathematical education at physics departments proceeds from the fact that physicists need mathematics only for applications. And since all physics, including quantum physics, is based on standard classical mathematics, there is no need to teach physicists finite mathematics. As I noted in Sec. 2.5, from the very fact of the existence of atoms and elementary particles it immediately becomes obvious that the standard division cannot be a fundamental concept. This means that the concepts of infinitesimals, infinitely large, continuity, differentiability, etc. can only be approximate, and the fundamental quantum theory should not be based on them. I asked physicists if they agreed with this. The philosophy of the vast majority of physicists is such that since standard mathematics works, there is no need to philosophize and invent something else, especially since they do not know anything else.

This philosophy is generally accepted despite the fact that the problem of divergences, as it arose in the 40s of the 20th century, still exists. In renormalizable theories, this problem can somehow be circumvented, but in quantum gravity this is not possible. However, most physicists do not consider the problem of divergences to be serious. In their opinion, since the theory gives 8 correct digits for the anomalous magnetic moments of the electron and muon, 5 correct digits for the Lamb shift, etc., then sooner or later all other problems will also be solved using standard mathematics. For example, as I already noted, Weinberg, who has made major contributions to QFT, writes that QFT should be considered "in the way it is," but at the same time it is "low energy approximation to a deeper theory that may not even be a field theory, but something different like a string theory". So, he acknowledges that problems exist and thinks that they will be solved in some theory that generalizes QFT, but which again will be based on standard continuous mathematics.

Thus, a strange situation takes place: everyone seems to agree that nature is discrete, and even the term "quantum theory" speaks of this. But all the problems of the theory are trying to be solved with the help of continuous mathematics. So, everything turns out like in a joke that my friend Tolya Shtilkind told me: "A group of monkeys received the task to reach the moon. After that, all the monkeys began to climb trees. The monkey that climbed the highest thinks that he has the most progress, and he is closer to the goal than the rest of the monkeys." I even quoted this anecdote in my paper [6]. This anecdote also contains a moral that to reach the moon, you must first get down from the trees. I did not give this moral, considering it obvious.

It is clear from what has been said that the need for finite mathematics can arise for physicists only in two cases: a) they become convinced that problems cannot be solved with the help of standard mathematics alone (i.e., until the thunder breaks out, the peasant will not cross himself); 2) with the help of finite mathematics, important physical results will be obtained that cannot be obtained in continuous mathematics.

Like most physicists, I did not know the very basics of finite mathematics. Purely by chance, when I was about 40, I came across a book (I don't remember which one) that seemed interesting to me. From it I learned about Galois fields and was surprised that physicists do not know them, although they can be taught already in the first or second grade (for example, after they have passed the division).

A simple example of a Galois field is the F_5 set of five elements (0, 1, 2, 3, 4), in which actions are defined as follows. Addition is defined as usual, but modulo 5. For example, 1+1=2, 2+2=4 as usual, but 2+3=0 or 4+4=3. If a is an element of the set F_5, then the opposite element $b = -a$ is defined so that $a + b = 0$ in F_5. For example, -1=4, -2=3, etc. So we have addition and subtraction. The product is defined as usual, but modulo 5. For example, $2 \cdot 2 = 4$, but $2 \cdot 4 = 3$. Finally, the opposite element $b = 1/a$ is defined so that $a \cdot b = 1$ in F_5. For example, $1/2 = 3$, $1/4 = 4$, etc.

A more general example of a Galois field is the set F_p of p elements $(0, 1, 2, \cdots p - 1)$, where actions are defined modulo p. Then, if p is prime, then all four arithmetic operations are possible in F_p.

The reader might say that the example with F_5 has nothing to do with real life, where, for example, 3+2=5, not 3+2=0. But suppose that physics in our world is determined by mathematics with a Galois field, where p is very large. Because operations in F_p are defined modulo p, then we can denote elements from F_p not only as $(0, 1, 2, \ldots p - 1)$, but also, for example, as $(-(p-1)/2, -(p-3)/2, \cdots -1, 0, 1, \cdots (p-3)/2, (p-1)/2)$. This set is called the minimum residue set. Then everything will be as usual until we add, subtract and multiply numbers that are much less than p in modulus, i.e., in this case, the existence of p will not be felt, and the difference from ordinary mathematics will be felt only when we are dealing with numbers not much smaller than p.

But the reader may say that the F_p example is also unrealistic, since here the division is not at all the same as usual. For example, 1/2 in F_p is a very large number $(p + 1)/2$, which seems to be contrary to common sense. This objection can be answered as follows. First, as I note in my works, there is no contradiction because in quantum theory, state spaces are projective. And secondly, as noted above, the standard division is also problematic and therefore the question arises whether the future quantum theory will be based not on a finite field, but on a finite ring, where there are only three actions - addition, subtraction and multiplication.

The second possibility seems very attractive even for aesthetic reasons. The history of physics says that it is desirable to introduce the smallest possible number of concepts and not to introduce concepts that do not have a fundamental meaning. In my early writings, I assumed that the ultimate quantum theory must be based on a finite field, but Metod Saniga wrote to me that the ring case is even more interesting.

Because for many years my life passed among physicists, then at first, I did not associate physics over finite mathematics with any philosophy, and thought that finite mathematics should be considered only from the point of view of applications to physics. My first idea of applying finite mathematics was as follows. Consider quantum electrodynamics with de Sitter symmetry and over finite mathematics. Then, in the basis of angular momenta, the irreducible representations for the electron, positron, and photon will be finite, since the angular momentum cannot exceed the characteristic of the finite field. This will lead to natural regularization instead of Pauli-Villars regularization and the theory will automatically contain no divergences.

However, later it became clear to me that such a naive idea does not work in connection with the following. In a theory over a finite field or ring, a particle and its antiparticle automatically belong to the same representation, and there are no representations for neutral particles. So in this approach, even a photon cannot be an elementary particle. Knowing the way of thinking of physicists, I think that most of them will immediately say that since the photon is not an elementary particle, then the theory is not physical.

Above, I described an example when in the standard theory (over complex numbers) with the de Sitter algebra $so(1,4)$ the concepts of particle and antiparticle change drastically. But in the case of the de Sitter algebra so(2,3) we still have representations with positive and negative energies, i.e., one can still talk about particles and antiparticles. But in the case of representations over a finite ring or field, the situation is similar to that obtained for $so(1,4)$, and here the very concept of an elementary particle changes drastically for any representations.

For example, since a particle and its antiparticle belong to the same irreducible representation, then transitions particle$\leftrightarrow$antiparticle are not forbidden, but the probability of such transitions is small if the characteristic of the field or ring is large. So, strictly speaking, the very concepts of particle and antiparticle are approximate, and the laws of conservation of electric charge, baryon and lepton quantum numbers are also approximate. These laws work well because at present the characteristic of the field or ring is very large. It is natural to assume that in the early stages of the world it was much smaller. Then transitions particle$\leftrightarrow$antiparticle were much more probable and this gives another natural explanation for the so-called baryon asymmetry of the world.

In such a theory, there is no problem of infinite vacuum energy, and the connection between spin and statistics has a natural explanation. Here the Dirac singleton can be a natural elementary particle. As Flato and Fronsdal showed, a massless particle (for example, a photon) can be built from two singletons. And one more interesting point, which casts doubt on the existing concept of an elementary particle. Even for de Sitter symmetry without a finite ring or field (and even more so with them), the mass of a particle is not a dimensional quantity, but a dimensionless one. If we accept for estimation that the radius of the world is about $10^{26}m$, then even the mass of the electron will be about 10^{39}, i.e., huge. It is hard to believe that a particle with such a mass is elementary.

All these properties of physics over finite mathematics are described in my works. But I think that sooner or later fundamental quantum physics will be over finite mathematics, not only because such physics will be better, but also because finite mathematics itself is more fundamental than standard continuous mathematics. Even from a purely mathematical point of view, continuous mathematics is a special case of finite mathematics in the formal limit, when the characteristic of a field or ring in finite mathematics goes to infinity.

In Sec. 2.5, I noted that standard mathematics has foundational problems, and, despite the attempts of many famous scientists, these problems have not been solved. Gödel's incompleteness theorems also tell us that standard mathematics is not self-consistent. But if you look at standard mathematics from the point of view that it is a special case of finite, then there are no problems. From this point of view, standard mathematics can only be considered as an apparatus that in many cases (but not all) gives a good approximate description, so there is no need to justify such mathematics, since in finite mathematics there are no problems with foundation.

The approach based on finite mathematics is also more natural from the point of view that any statements here are verifiable, at least in principle. Moreover, the principle here is that any statement is correct or not, if there is a way to check it. For example, we want to check if the statement 10+20=30 is correct or not. For example, we want to check it on a computer or accounts. Any computing device can only calculate modulo some number p, which depends on the amount of memory of this device. For example, if $p = 40$, then we will indeed get that 10+20=30, but if $p = 25$, then we will get that 10+20=5. From this it is clear that any mathematical operations (even $2 \cdot 2 = 4$) are verifiable only if they are modulo some number p. Standard mathematics is an idealized special case of finite, in the formal limit, when $p \to \infty$.

Although standard mathematics is part of our daily lives, what most people don't realize is that it has an implicit assumption that resources are unlimited. And there is no principle in standard mathematics that for any statement, its correctness can be verified. For example, it is impossible to check that $a+b=b+a$ for any natural numbers a and b.

That any statement must be verifiable is part of the Vienna School of Positivist Philosophy, of which Moritz Schlick was the informal leader. On the other hand, in the philosophy developed by Karl Popper there is "The Falsification Principle", and as Popper said, "science is more concerned with falsification of hypothesis than with the verification." Here the statement that always $a+b=b+a$ is considered conditionally true until such numbers a and b are found that $a+b \neq b+a$. It is clear that quantum theory is closer in spirit to the Vienna School, while classical theory is closer to Popper's philosophy. Therefore, it is not surprising that in the dispute between Einstein and Bohr about quantum theory, Popper was completely on Einstein's side.

Some of my readers have the impression that finite mathematics rejects, for example, π, e, Maxwell's equations, the Pythagorean theorem, and so on. In this regard, let me remind that, as already noted, a more general theory does not reject a less general one, but says that the latter is a good approximation only under certain conditions.

There are two levels of understanding π - how it is taught at school and how it is taught at the university. In school - this is the ratio of the circumference to the diameter. And what is a circle - it is a set of points located at a distance R from the center. And what is a point - a kind of speculative concept, there are no points in nature and there are no continuous curves either. If, for example, we draw a supposedly continuous curve on paper and look at it through a microscope, we will see that, in fact, the curve is strongly discontinuous since it consists of atoms, there are no points on it, etc. Therefore, the concepts of the diameter of a circle and its length are speculative. And why then Maxwell's equations, the Pythagorean theorem, divergence, differential equations, etc. work well? Or, for example, when we describe the water in the ocean with the equations of hydrodynamics, it works well. Because in the approximation when we neglect the size of atoms and represent matter as something continuous, then in this approximation there are infinitesimals, we can differentiate, etc.

Now about how we were taught at the University. All concepts of the type π, e etc. should not come from our geometric representations, but only from calculus. Here, ALL the functions that we have learned are DEFINED by their expansion in a Taylor series. For example, $exp(x)$ is defined by its Taylor series, $cos(x)$ and $sin(x)$ — by their Taylor series, and so on, and e is defined by the infinite Taylor series for $exp(1)$. From this it immediately follows that $exp(ix) = cos(x)+isin(x)$. And if we take the Taylor series for $arccos(x)$ or $arcsin(x)$, then $\pi = arccos(-1)$ or $\pi = 2arcsin(1)$, i.e., π is defined by its infinite series. The formula $exp(2i\pi) = cos(2\pi) + isin(2\pi) = 1$ is obtained only from manipulations with infinite Taylor series. Therefore, if you think that in PRINCIPLE you can count as many characters as you like for π and e, then you can count these characters until you turn blue. And if we nevertheless agree that, for example, the number of atoms in the universe is finite and it is impossible to build a computer with an infinite number of bits, then we have to admit that π and e are not so fundamental as usually believed. Quantum theory has completely changed our worldview. It cannot be said in it that some value "actually" exists, but cannot manifest itself in any way - if it cannot manifest itself, then it means that it does not exist.

So, when we pass to the limit $p \to \infty, \hbar \to 0$ and neglect the size of atoms, then the standard meaning of the differential equations, π, e etc. are restored.

4.4 Gravity as a kinematic consequence of the finiteness of the world

In non-relativistic classical mechanics, the law of universal gravitation is obtained if the potential energy of interaction of two particles with masses m_1 and m_2 is chosen as $-Gm_1m_2/r$, where r is the distance between the particles, and G is the gravitational constant. In general relativity, the law of universal gravitation is obtained in the special case when there are two non-relativistic particles. In quantum gravity, they try to explain gravity as a consequence of the exchange of virtual gravitons. This theory is not finished yet (and it is not clear if it will ever be finished), it is non-renormalizable and, at least in the existing approaches, it is not clear how to eliminate the divergences in it.

The standard dogma is that gravity is the fourth force to be combined with the strong, electromagnetic, and weak forces. Strong interaction - exchange of virtual gluons, electromagnetic - exchange of virtual photons, weak - exchange of virtual W and Z bosons, and gravitational - exchange of virtual gravitons. As described in 2.2, the observation of binary pulsars is believed to provide indirect evidence for the existence of gravitons, while the recent LIGO experiments provide direct evidences. However, as noted in this section, such claims are very problematic.

My approach to gravity is based on the following principles. First, as described in 2.6, operator algebra is more fundamental than space. Second, as described in the Sec. 4.1, de Sitter symmetry is more fundamental than Poincare symmetry. Finally, as described in Sec. 4.3, fundamental quantum theory must be based on finite mathematics.

Consider first a theory based on the usual de Sitter algebra, i.e., without involving finite mathematics. Let there be two free non-relativistic particles with masses m_1 and m_2. In the Poincare invariant theory, the mass of such a two-particle system is (in the system of units $c = 1$)

$$M = m_1 + m_2 + q^2/2m_{12}$$

where q is the relative momentum and $m_{12} = m_1 m_2/(m_1 + m_2)$ is the reduced mass. Therefore, the mass of a two-particle system depends only on the relative momentum, but not on the distance r between the particles, and cannot be less than $m_1 + m_2$. In particular, in this approach it is impossible to obtain the gravitational correction $-Gm_1m_2/r$ to the mass.

In anti-de Sitter symmetry, the mass of a two-particle system also cannot be less than $m_1 + m_2$, and the gravitational correction to the mass cannot be obtained either. But in the theory invariant under the de Sitter algebra $so(1, 4)$,

$$M = m_1 + m_2 + q^2/2m_{12} + V(r, q)$$

where $V(r, q)$ is some function that depends on the quantum state of the two-particle system. In particular, there is no law forbidding such states that $V(r, q) = -Gm_1m_2/r$. In this case, the constant G is not taken from outside but must be calculated. Therefore, the problem is to understand why such a relation holds for semiclassical states.

As I noted above, the belief that gravity is an exchange of gravitons arose from an analogy with particle theory. However, gravity is known only at the macroscopic level, and to think that the same mechanisms will work here as in particle theory is a far extrapolation. In addition, to think that at the macroscopic level the coordinate operator has the same form as in atomic physics and particle theory is also a far extrapolation. In my publications, I show that at the macroscopic level, the coordinate operator cannot be the same as in microscopic physics.

I propose another coordinate operator and then in the semiclassical limit and in the nonrelativistic approximation the mass of the two-particle system equals

$$M = m_1 + m_2 + q^2/2m_{12} - Cm_1m_2(1/\delta_1 + 1/\delta_2)/[(m_1 + m_2)r]$$

where C is some constant, and δ_1 and δ_2 are the widths of the momentum wave functions for particles 1 and 2. In ordinary theory (not over finite mathematics) there is no restriction on these widths and the last term can be very small. But in finite mathematics there is such a limitation and the width is inversely proportional to the mass. Therefore, the law of universal gravitation is obtained, where $G = constR/(m_0 ln(p))$, where m_0 is the mass of the nucleon, p is the characteristic of the field or ring in the finite matrix, and $const$ is of the order of unity. It cannot be calculated exactly because we do not know the wave function of a macroscopic body. If we take the existing value for G and take for an estimate that R is of the order of $10^{26}m$, then it turns out that $ln(p)$ is of the order of 10^{80}, i.e., p is a huge number of the order of $exp(10^{80})$. From this formula it turns out that $G \to 0$ in the formal limit $p \to \infty$, i.e., in a formal transition to ordinary mathematics, gravity disappears. So, in this approach, gravity is a consequence of the finiteness of the world. You can also consider corrections to Newton's law, etc. So, in my approach, gravity is not an interaction at all, but a purely kinematic manifestation of the fact that the world is finite. In particular, there are no gravitons in this approach.

Chapter 5

My attempts to publish papers on the cosmological constant and on physics based on finite mathematics

This chapter will substantiate the opinion expressed in chapter 3 that the main reason for the impasse in fundamental quantum theory is that many physicists and mathematicians associated with establishment (i.e., those who decide what to promote) do not adhere to high moral standards. Strictly speaking, I can't even say that this degradation has occurred. I don't know how high moral principles used to be. But we must proceed from the presumption of innocence, although articles on the history of physics describe cases that not everything was idyllic before.

I will begin my reminiscences with an attempt to publish my paper on de Sitter invariant theory in the journal Communications in Mathematical Physics. Haag was then the editor-in-chief of this journal. He is known in the physics community, but many have a rather negative attitude towards him. He and his group profess the philosophy that fundamental quantum physics should start from fundamental axioms and be built on rigorous mathematics. But for most of the establishment, such a philosophy is unacceptable. It is a known joke that the contribution of Haag and his group to physics is less than any pre-specified epsilon. Members of the group know this joke and sometimes quote it.

Perhaps Haag's most famous result is the theorem that the interaction representation can be rigorously justified only if the interaction is zero!!! But all the loudest results of quantum theory are based on the interaction representation. It would seem that if physicists do not like this result, then they should either refute it or say some words about why the interaction representation still works so well, at least in some cases. But even in QFT textbooks there is not a word about Haag's theorem, as if it does not exist. So, the authors of these textbooks have strange ideas about scientific ethics, to put it mildly.

But since I thought (and still think) that physics should be based on correct mathematics, I hoped that my paper would be considered on its merits. But I received a response from Haag, where he writes that he will not take the paper. Reason: not because something is wrong, but because the work is based on de Sitter, and there is no S-matrix in de Sitter. Indeed, in the approach of Haag, Poincare invariance and existence of the S-matrix are unshakable dogmas. But it seemed to me that since establishment treats Haag badly because he does not fit into its dogmas, then Haag himself should not think that only his dogmas are allowed. But alas, it turned out that this was not the case. After that, I lost all sympathy for Haag. And this paper was then published in Journal of Physics A, which was then a very decent journal (more on this below).

When I worked in Dubna, and my monthly salary (Dr. Sci. and Leading Researcher) was in the range of 50-100 dollars (depending on the ruble exchange rate), the only way to live more or less normally was if I was invited somewhere or included in the grant. Therefore, you had to do not what you want, but what someone wants.

I had opportunities to collaborate with someone only on the topic of my doctoral thesis, i.e., relativistic effects. During my work in Dubna, I published many papers on this topic in the so-called prestigious journals (Nuclear Physics, Physical Review, Annals of Physics, etc.). Some of these papers were only mine, and some were joint papers with Gianni Salme and Emanuele Pace, who invited me to Rome. At that time, I had no problems with publishing papers in the so-called prestigious journals. I noticed this pattern: the more standard the paper is, the easier it is to publish. Problems arose if something was non-standard. But here, too, these problems could, as a rule, be solved. Probably because they still did not have a fundamental deviation from what establishment considers acceptable. And maybe because the papers were sent from the institute in Dubna, which establishment knows.

But now I send papers that are unacceptable for establishment, and I send them from my programming company, which for establishment sounds something like "Horns and Hooves". Most likely, both factors play a role, but I think that the first factor plays a more important role. Perhaps, also a new factor also plays some role: many journals have either become completely open access or switched to a system where the author himself can choose how to publish an article: as open access or according to the usual system. In the first case, the author himself must pay, and usually the price is rather high: 2-3 thousand dollars. Such journals swear that the peer review process does not depend on which option the author chooses. But I have a feeling that when they see that the author has not chosen open access, they immediately try to find a reason to kick back without a review. Three years after my arrival in America, I had new results in quantum physics over the Galois field. At first, I was quite optimistic and thought that the so-called prestigious journals would gladly take them. But it turned out that they did not even want to consider it, although, for example, Nuclear Physics B also published purely mathematical works.

And I thought: what if I try to send it to Russian Journal of Nuclear Physics? This is actually an ITEP journal and before that I had a lot of papers there (probably about 20). All of them were on more or less standard topics, and I, of course, understood that for the ITEP establishment, this is a typical case about which they say that it is exotic, pathological, onanism, etc. But I hoped that a lot of time had passed and, perhaps, people from the establishment, having become older, were no longer so irreconcilable and, moreover, because they know me, then there is no question that the author is not clear from where.

The editor-in-chief of the journal was Alexey Borisovich Kaydalov, and the deputy editor was Leonid Avksent'evich. When I was a student, A.B. gave us lectures on strong interactions, and since he belonged to the upper layers of the ITEP establishment, it seemed to me that he was rather impregnable. But, when I was with him at several conferences, I realized that such an opinion was erroneous. He turned out to be very simple and benevolent, and he did not have the ITEP way of thinking at all, that only they have high science, and everything else ...

I wrote to A.B. and L.A., that for me the main thing is not whether the paper will be accepted or not, but that there should be a review. I was wondering what the ITEP luminaries would say, because to say in the corridor that something is a pathology or onanism is one thing, but to substantiate it on paper is another. In addition, I knew that some ITEP people supported my approach. For example, Mikhail Aronovich Olshanetsky and Jan Kogan recommended my paper to this journal in 1988. It was published, probably because there were no such strong statements as now.

A.B. and L.A. told me that they tried to find the friendliest reviewers, but still did not succeed. I.e., a clear hint was such that one of the greats was against it. But they said that Mikhail Aronovich is a member of the editorial board of Theoretical and Mathematical Physics, and he advises sending it there. I did so and the paper was published there without any problems. And I did not receive any review from ITEP.

When I went to conferences on the few-body problem, the most famous physicist at them was, perhaps, Fritz Coester. He was born in Berlin in 1921, but during the war he studied at the University of Zurich, where he received his PhD in 1944. In his memoirs, the incomplete text of which I found on the Internet, he describes that Heisenberg once came to their seminar, then they had dinner and walked somewhere together. He writes that according to his convictions, Heisenberg was then a Nazi, but I did not see confirmation of this statement (perhaps because I saw only an incomplete text).

At conferences, I discussed various problems with him and there was even a paper describing a round table at a conference in Trieste in 1995, where 6 people participated: F. Coester, V. A. Karmanov, F.M. Lev, R. Schiavilla, A. Stadler, J.A. Tjon. But the most impressive thing for me was this.

At that time, under the impression of my communication with ITEP physicists, I thought that QFT could be somehow substantiated mathematically. But I found that, due to the Schwinger terms, the 4-vector current in QED commutes incorrectly with the angular momentum operators. So, the 4-vector current is not actually a 4-vector! I sent papers about this first to Phys. Rev. Lett., and then to Phys. Rev. D, but Jackiw rejected them. He sent me a letter, where he wrote that the result is obvious: in QED, the current operator is for particles with spin 1/2, while for scalar particles, the current operator does not contain Schwinger terms and commutes correctly. I no longer remember all the details, but it would seem that everything is simple here: was the result that the current operator in QED commutes incorrectly with the angular momentum operators before that was published somewhere or not? If there was, then, it would seem, indicate the link and then the question of publication automatically disappears. But there was no explicit link in the letter. This paper of mine is in arXiv (see [7]).

And I told Coester that I have a problem that I can't mathematically substantiate QED. And he replies: it's not even worth trying because substantiation is impossible. I was dumbfounded and asked him several times if I understood correctly, because, for many, his words would be complete sedition. But he repeated that I understood correctly and there was no chance to substantiate QED mathematically. Then it made a very big impression on me because, after talking with ITEP physicists, for whom QFT is almost like a religion, I thought that maybe there is something in this.

When in 1994 I was at a conference in America, then after the conference Coester invited me to Argonne National Laboratory for a week, where he worked. In particular, we discussed my future paper, on the current operator in relativistic quantum mechanics (i.e., not in QED). This paper appeared in the Annals of Physics at 57 pages (see [8]) and by some circumstantial evidence it seems to me that Coester was the reviewer. Then, in our correspondence with Coester, I convinced him that the fundamental symmetry should be de Sitter symmetry, not Poincare symmetry, and, moreover, the symmetry should be over the Galois field. He even became interested in de Sitter, and I explained to him some of the differences between de Sitter and Poincare. But, as far as I understand, he was interested in de Sitter not as fundamental symmetry, but from the point of view of applications to few-nucleon systems. But he categorically rejected Galois.

Nevertheless, I decided to turn to him with such a request. After my papers on quantum theory over the Galois field were published in Journal of Mathematical Physics in 1989 and 1993, the editor-in-chief was replaced by R. Newton instead of L. Biedenharn. He rejected my papers under the pretext that they were not for this journal, but for a journal on elementary particles. And in journals on particles, I was rejected under the pretext that they are not physical, but mathematical. So, there was a vicious circle and I decided to try sending the paper to Foundations of Physics. Then the editor of the journal was Van der Merwe. The majority of physicists considered the journal unreputable, in which authors write only philosophy, i.e., chatter. Van der Merwe's approach was to ask the authors to choose their own reviewer. So I sent my paper there and asked Coester to be the reviewer in the hope that he would admit that if the paper is correct, then it can be published even if the philosophy of the paper is not to his liking.

Initially, Coester wrote to me that it was not reputable to publish in this journal because the journal is not serious. I told him that I saw his article in this journal. But he replied that it was not a serious article, but simply on the occasion of the anniversary of Rohrlich. However, I wrote a review anyway. But it was not clear from the response whether he approved the paper and whether he advised publishing it. I sent the reply that Van der Merwe forwarded to Coester. But after that Coester decided not to answer at all and Van der Merwe said that he could do nothing.

After some time, the Nobel laureate ‘t Hooft became the editor of the Foundations of Physics and a new description of editorial policy appeared. It makes a strong impression and therefore I will quote it almost in full:

"*Our views of the physical world are changing rapidly. Humanity's continuing search for coherent structures in physics, biology, and cosmology has frequently led to surprises as well as confusion. Discovering new phenomena is one thing, putting them into context with other pieces of knowledge, and inferring their fundamental consequences is quite something else. There are controversies, differences of opinion, and sometimes even religious feelings which come into play. These should be discussed openly. Philosophical issues that are of a general, nontechnical nature should be handled in the opinion pages of the news media, but when the discussed arguments become too technical for that, when peer review is needed to select the really valuable pieces of insight, only a distinguished scientific journal is the appropriate form. Foundations of Physics is an international journal devoted to the conceptual bases and fundamental theories of modern physics and cosmology, emphasizing the logical, methodological, and philosophical premises of modern physical theories and procedures. We welcome papers on the interpretation of quantum mechanics, quantum field theory, thermodynamics and statistical mechanics, special and general relativity as well as cosmology. Also, we think it is time for the experts on quantum gravity, quantum information, string theory, M-theory, and brane cosmology to ponder the foundations of these approaches. New insights are gained only by intense interactions with professionals all over the globe, and by solidly familiarizing oneself with their findings. Fortunately, there are many authors with a deep understanding of the topics they are discussing who are willing to take the opportunity to present their ideas in our journal, and their clever inventiveness continues to surprise us. Acceptance of a paper may not necessarily mean that all referees agree with everything, but rather that the issues put forward by the author were considered to be of sufficient interest to our readership, and the exposition was clear enough that our readers, whom we assume to be competent enough, can judge for themselves.*"

These words are breathtaking and, it would seem, a journal with such an editorial policy should not be based on prejudices and should be open to new ideas. So, I decided that this is exactly what I need and sent a paper there. When there was no answer for a long time and I asked why, the secretary replied that the editor-in-chief for my article is 't Hooft himself, and this means that he will either review it himself or ask someone.

When the reply was finally received, it contained two reviews, one from Reviewer 1 and the other entitled "EDITORIAL COMMENT:" i.e., that it was written by 't Hooft himself. But first, I will give a reviewer's opinion.

Reviewer 1: There is a lot of work on extensions of standard quantum field theory into other number fields (especially on p-adic numbers). The present paper studies various aspects of quantum field theory on Galois fields. I find the physics in the paper confusing, and I cannot recommend publication in Foundations of Physics. However, I believe that there is some merit in the discussion of representations of various groups over Galois fields. This is an interesting subject in its own right, and it might have applications to Physics. The author might like to present some of his work in that form (perhaps in mathematical physics journals).

So, the main argument is: he can't recommend because he "find the physics in the paper confusing". In what it is expressed - there are no explanations. So, his way of thinking is such that if he thinks so, then this is the highest truth and no explanation is needed i.e., he not only has no idea about scientific ethics, but also violates the journal's rules that different approaches can be considered. And then he even writes something positive that there are merits in discussing group representations over the Galois field. So, he doesn't even understand that there are no groups over a Galois field in the paper, but only algebras over Galois fields, and he doesn't understand the difference between them. And in conclusion - the usual kickback: a recommendation to send it to a journal on mathematical physics.

And in the editorial comment (which probably wrote 't Hooft) there are already some more specific statements:

EDITORIAL COMMENT: 1. It seems that the entire Hilbert space here is taken to be over the Galois field. The problem with that is that it is hard to distinguish "small" from "large" numbers in the Galois field, and this prohibits any probability interpretation of the wave function. A discussion of probabilities would be very important and is missing here.

2. The paper is really too long for a discussion of such an elementary idea. The objections are significant, while the fact that there are no infinite numbers does not carry much weight; in most theories we can handle that problem. If the cosmological constant vanishes identically this would only be suspicious: in the real world it does not seem to be exactly zero. We could reconsider a new submission about this idea if the paper could be made much more concise and to the point. The discussion of neutral particles could also be postponed until the more essential obstacles are put out of the way.

Point 1 says that once the spaces are over the Galois field, then there is a problem with the probabilistic interpretation and this should be discussed. The remark is absolutely correct. But in all my papers I discuss this and note that in spaces over the Galois field, the probabilistic interpretation can only be approximate. If he thinks that the discussion should be more detailed, then I would be very happy to include such a discussion.

Item 2 begins by stating that the paper is too long for such an elementary idea, i.e., the idea that in quantum theory spaces must be over the Galois field, he considers elementary. So, he clearly hints that this idea is more elementary than his great ideas. Of course, such a phrase does not mean respect for the author, but I am ready to live with it. But the most important thing: if the idea is elementary, then it should be immediately obvious whether it is correct or not, whether something worthwhile follows from it, etc. If nothing worthwhile follows from it, then why discuss it at all, and if it does, then it would seem that it should be welcomed. And then he writes that the goals of the paper are important, i.e., this would seem to contradict the assertion that the idea is elementary. And it doesn't even matter what he writes next, but he writes that the paper can be revised.

Of course, I immediately wrote to them that I would prepare a revised version of the paper, but I received a response from his deputy that the paper would no longer be considered. So, it is not clear whether the left hand knows what the right hand is doing or the phrase that the paper can be revised was written only to write something.

In connection with the above, I believe that 't Hooft does not comply with scientific ethics because it is clearly unethical to have such an editorial policy and at the same time not to allow the author to send an appeal if he does not agree with the review and the editorial opinion.

After that, I sent them two other papers and both times the story was the same: at first, meaningless reviews of reviewers were sent, and my objections were not even considered, because, as they wrote, they do not have the opportunity to consider author's appeals. So, again, what is written in the editorial policy is of no importance.

My second paper submitted to this journal was on the problem of the cosmological constant. Before that, I had a rather long correspondence with Volovik, with whom I studied at the same MIPT course. Correspondence did not change anything in that our views were completely different, and remained. But my paper in Foundations of Physics was sent to two reviewers for review, he was one of them (probably because it contained a reference to his work) and he wrote to me about it. His review is this:

The paper sounds scientifically and can be published after the clarifications of the fol-

lowing points are made. The author suggests that the de Sitter symmetry is fundamental and thus the cosmological constant problem does not exist. For the de Sitter symmetry to be fundamental the de Sitter Universe must be stable. However, at the moment the stability of the de Sitter vacuum is a debated topic, see [1, 2] and references therein. If Polyakov is right, and dS is unstable towards Minkowski, the dS symmetry cannot be fundamental. Moreover, the de Sitter symmetry can be spontaneously violated. The example of such symmetry violation is demonstrated in [3]: the de Sitter universe spontaneously decays to Minkowski one. So, to praise the dS symmetry is not enough, the author should address the problem of the stability. The other point which should be addressed is the claim of the author that there are no neutral particles in dS invariant theories. His consideration is based on massive Dirac fermions as fundamental elementary particles. However, we know that the original Standard Model fermions are Weyl fermions, with left and right particles belonging to different representations of the SU(2) group. Weyl fermions are massless and thus the division into particles and anti-particles made by the author is not applicable to Weyl fermions. Also, since the real Dirac particles are composite objects, being the mixture of fermions of different representations, they must lose their mass in de Sitter as all other composite particles. Instead of a single mass one has the spectrum of mass which includes the zero value. This means that the author's division into particles and anti-particles does not make sense even for Dirac particles.

References

[1] A. M. Polyakov Decay of vacuum energy, Nucl. Phys. B 834, 316-329 (2010); arXiv:0912.5503.

[2] G.E. Volovik, Particle decay in de Sitter spacetime via quantum tunneling, JETP Lett. 90, 1-4 (2009); arXiv:0905.4639.

[3] F.R. Klinkhamer and G.E. Volovik, Towards a solution of the cosmological constant problem, JETP Lett. 91, 259-265 (2009); arXiv:0907.4887.

Such reviews are typical, so I will discuss this review in some more detail. If someone who is not an expert in these matters looks at this review, they will probably decide that the review was written at a high scientific level and the arguments are very serious. But actually, the review is pointless.

First, it is immediately clear that he does not want the paper to be published. At the beginning of the review, the words are pronounced that the paper looks scientific and can be published if answers to objections are given. So, he does not directly write that he is against publication but pretends that he is honest. And at the end of the review, he says that the paper is meaningless. So there is no logic. But all objections are meaningless for this reason.

He writes that it is not enough to praise de Sitter because, according to Polyakov's work [1], there are problems with de Sitter symmetry, but this is a debated issue that is discussed in [2,3] and I also have to comment on this issue. But, as noted in Sec. 4.1, de Sitter symmetry is better than Poincare symmetry for a simple reason - it is more general and Poincare symmetry is just a special case of it. And we must begin not with empty space, but with algebra. But when physicists adhering QFT hear "de Sitter", they immediately think that one means QFT where de Sitter space is chosen as empty space.

The authors of [1-3] play this game. Physicists with this way of thinking prefer to work with QFT and accept the dogma that empty space must be flat and then there is a great activity with dark energy. He wants me to play this game too. But I don't even want to speak out because the approach starting with an empty de Sitter space does not make sense and, as noted in Sec. 4.1, symmetry at the algebra level has nothing to do with QFT with the de Sitter space.

The second point of his criticism is that there are no neutral particles in my approach, and, in his opinion, my interpretation of particles and antiparticles does not make sense. Why doesn't it have? Because, supposedly, it contradicts the Standard Model. But even if you do not go into this question in essence, you can ask: what, the Standard Model is the law of God? Even the name says it's just a model. It comes from twenty adjustable parameters and in many cases describes the experiment really well. The theory that in the beginning there were only massless

Weyl fermions is far from being substantiated. But still, you can't doubt the Standard Model. And again, he does not understand that, if we proceed from the de Sitter algebra, because it is more general than the Poincare algebra, then the conclusion about neutral particles and the division into particles and antiparticles is obtained automatically, and the states describing particles cannot be massless. But this does not contradict even the Standard Model, since zero mass in Poincare theory can be derived from small mass in de Sitter theory. A detailed discussion of this problem was in my paper in the Journal of Physics A [9], to which I refer, and which Volovik knew about since we discussed it. I will write about this paper below.

I will write about Polyakov's paper [10] below, but, regardless of what I think about this paper, Volovik's review contradicts the editorial policy of the journal, that different approaches have the right to exist. And this is a typical situation: reviewers write reviews without paying attention to editorial policy i.e., they think they know best. And no matter what nonsense in the review report is written, the author has no chance because his objections are simply ignored. In such a situation, as I noted in chapter 3, editorial policy has the same meaning as the Stalinist constitution of 1936, i.e., doesn't make any sense.

Let me note that my paper was called "Does the cosmological constant problem exist?" and probably hundreds of authors have claimed that what they have proposed is a solution to the problem of the cosmological constant. For example, the same Volovik wrote papers on this topic and, of course, he discusses this problem based on the prerequisites that he likes. Of course, such papers are published because they are in line with what the establishment does. But since I actually write that the establishment approach is meaningless, then all reviewers stand to death and do not let my articles pass.

Already in our first year study at MIPT there was an opinion about Volovik that he was strong in mathematics. It was said that he even knew Stolz's theorem, which was not included in the program, and in Fichtenholtz's book on calculus it was printed in small print, i.e., as optional. Probably, in those questions of mathematics that he needs, Volovik is strong even now. But the fact that he does not understand that the de Sitter algebra is better than the Poincare algebra, simply because it is more general, suggests that he is clearly not strong in Lie algebras and their representations. This is not a problem, because one can't know everything, and everyone knows something and doesn't know something. But the problem is that he allows you to work only in those approaches that are closer to him.

As will be noted below, approximately the same can be said about Polyakov. There is an opinion among physicists that he is strong in mathematics, and again, it is quite possible that this is so in the mathematics that he needs. But, as will be noted, he is engaged in theory with de Sitter, but does not know the basic representations of the de Sitter algebra. And he also allows you to work only in those approaches that are closer to him.

Here I will make such a digression. It would seem that one cannot deal with ordinary quantum theory if one does not know the representations of the Poincare algebra or the Poincare group. Therefore, it would seem that if someone is engaged in quantum theory with de Sitter invariance, then one must know the representations of the de Sitter algebra, which replace the representations of the Poincare algebra. But almost all those who are engaged in such a quantum theory do not know such representations, because, in view of their way of thinking, de Sitter invariant quantum theory is a theory on de Sitter space, and some even write that fields are more important than particles. So Polyakov is no exception here.

Now I will briefly describe the history of my paper in the Journal of Physics A in 2004 [9] that I mentioned above. It is shown here that, based on the de Sitter algebra, there really are no neutral particles, and the particle and its antiparticle belong to the same irreducible representation. Before that, I had several papers in this journal and the reviews of the reviewers were always at a high level. In this case, the first reviewer's response was entirely positive and noted that the result was very important, while the second reviewer's response was negative. That is why the paper was given to the adjudicator and he supported me. But then all my papers on the cosmological constant and on the theory over the Galois field submitted to this journal were rejected and no serious arguments were given. Why this happened - I have several hypotheses, but I will not give them.

I had many attempts to publish papers on physics over the Galois field and on the cosmological constant, and below I will describe some of them. One of the papers on physics over the Galois field was sent to Annals of Physics. The first review was, as usual, meaningless. For example, it was unacceptable for the reviewer that the paper did not specify which Lagrangian was used. So, the reviewer does not understand that there is no Lagrangian in the theory over finite mathematics, since there is no principle of least action because in a finite field the concepts more and less can only be approximate.

Naturally, I wrote an appeal. The editor of the journal at that time was the Nobel laureate Wilczek. He wrote to me to suggest a reviewer. Naturally, I would like a well-known physicist to be the reviewer, and I also understood that Wilczek would agree with my candidacy only if the physicist was known. And I thought about Polyakov. At that time, he was not yet a Milner Prize winner, but, of course, he was very famous. I tried to weigh all the pros and cons. Of course, I knew that Polyakov is considered a great scientist in QFT. I saw his lecture at the ITEP school and in it, as a conclusion, there was a phrase that quantum field theory is, probably, a science which can be fully constructed. Therefore, I thought that he would not consider me a competitor because QFT is inexhaustible and there will always be work for his children and grandchildren. In addition, he also studied at the Moscow Institute of Physics and Technology and was considered a star at ITEP, although he is only a year older than me. It was believed that the spirit of MIPT was such that there should be no dogmas in science and that different approaches have the right to exist. In addition, as I noted, many physicists had the opinion that Polyakov was strong in mathematics and therefore I hoped that he would not mind the Galois field approach.

I called him, described the situation and asked him to be a reviewer. He asked: "Will I understand?" I tell him that of course, because everything is simple and there is nothing to understand. But I do not insist that he be a reviewer and if, for some reason, he cannot, then there is no problem and I will find someone else. He said he would think. I waited for his decision for three months and called him. He says: I will either write a positive review or I will not write any review. I answer him that it is good, but time is running out and Wilczek is already asking if I have found a reviewer. And if he cannot or does not want to, then there is no problem. Again, he said he would think.

Another three months have passed and I receive feedback from Annals of Physics. At the beginning of the review, he writes that the approach is very interesting, and he even explains what the Galois field is. And then he writes that because the results are published in arXiv and there is no question of priority, then there is no need for immediate publication (I wonder if he also applies this criterion to his work?). He writes that I should show how my approach works in standard cases, for example in the QFT theory φ^4. Therefore, this review is similar in spirit to Volovik's review in Foundations of Physics. He does not directly write that he is against and pretends that he proceeds from scientific criteria. But he wants me to play on his field, i.e., in QFT, i.e., again, the dogma is accepted that QFT is the pinnacle of science and everything that is not QFT is not allowed. And he even sets me the task as if I were his graduate student.

It is clear that I again wrote an appeal and here gave an example. Let's say that Heisenberg or Schrödinger wrote a paper on quantum mechanics, and the reviewer says to them: I want you to show how quantum mechanics works in ordinary cases, for example, how it describes the motion of the Moon. So, the reviewer wants the motion of the Moon to be described by the Schrödinger equation. But this makes no sense, because the Moon is a classical object and in the classical limit, the Schrödinger equation turns into the classical Hamilton-Jacobi equation, which describes the motion of the Moon. So, it makes no sense to describe the motion of the Moon by the Schrödinger equation and, at the end, go to the classical limit, since it is much easier to go directly to the Hamilton-Jacobi equation. And this is a complete analogy with my case: since I have a transition to ordinary physics at $p \to \infty$, then it makes no sense to consider standard problems through Galois fields, since one can immediately pass to the $p \to \infty$ limit. But he was adamant, although I called him and tried to convince him.

I will take this opportunity to write a few words about his paper [10], which Volovik cited in his review (see above) and which was one of the reasons to reject my paper in Foundations of Physics (see above). In the abstract, he says that what he proposes "*may help to solve the cosmological constant problem*". At the beginning of the paper, he writes "*Dark energy, like the black body radiation 150 years ago, hides secrets of fundamental physics*". Already from these phrases it is clear that the way of thinking of him and Volovik is completely in conflict with what I write, that dark energy - this is nonsense and there is no the cosmological constant problem. And since now it is clear to me that (as follows from the above facts) neither he nor Volovik follow scientific ethics, they will never agree to publication of my papers on the cosmological constant. He further writes that in the case of de Sitter the spectrum is symmetric with respect to reflection (i.e., for each eigenvalue there is an eigenvalue with the opposite sign) and refers to Wigner's article, which is either published or not, and in a footnote he writes "*I thank Pierre Ramond for providing me with an unpublished (?) manuscript by Wigner and Fillips, containing this statement.*" That in de Sitter, the spectrum is symmetric is obvious to anyone who understands the representations of the de Sitter algebra, and this is written in all papers on these representations. If he understood this, then he would not have to refer to some little-known paper by Wigner and thank those who gave him this paper.

Then he develops some theory on the de Sitter space, which I don't even want to go into because, in view of what I wrote above, I don't consider it a serious science. But the paper concludes: "*Although there are many unanswered questions and future surprises, I believe that a small step made in this work is the step in the right direction.*" So, the conclusion is rather "modest" and the analogy with the famous phrase of Armstrong, who, having stepped on the surface of the Moon, said "*a small step of a man - a big step of mankind*" is clearly visible. But in fact, he says that nothing is clear here and it is not clear when it will be clear. And, according to Volovik's review, since I didn't fit in here and didn't say anything about it, then my paper cannot be published.

By the way, Polyakov wrote several memoirs, and it is not difficult to catch the idea in them that this is how great he is. For example, he writes that the Nobel laureate Wilson, who discovered the operator product expansion, wrote that if he had not discovered it, then others, such as Polyakov, would have done it. But when he was interviewed on the occasion of the $3 million Milner Prize, his modesty surprised me. He said he hoped his results would be included in 22nd century textbooks i.e., not the fourth or fifth millennium, but only the 22nd century.

In the story of the paper in Annals of Physics, of course, I cannot have any complaints about Wilczek: he acted very kindly, suggesting that I find a reviewer myself, and this is already my problem that I found Polyakov. So I sent one of my papers on the cosmological constant to Annals of Physics.

At first, I received the following answer from the journal: "*The editor finds your paper, referenced above, not appropriate for our journal. His comment is that the paper is too speculative. There is no formal report.*" There were no attempts to substantiate his opinion, but the editor simply decided so. So, the editor does not believe that a decent scientist should substantiate his official decision. Of course, I wrote an appeal:

Dear Professor Wilczek, I am surprised that my paper is characterized as speculative. The main results are obtained from the explicit mathematical construction of irreducible representations (IRs) of the de Sitter algebra. The results shed new light on fundamental problems of quantum theory. One might wonder why those results have not been known although many physicists are working on de Sitter QFT. I believe the explanation is that, for some reasons, physicists prefer to work with fields rather than with IRs of the dS algebra and probably they did not expect that the IRs could give so important information about fundamental notions of quantum theory. I believe it is rather strange that, although Wigner's results on IRs of the Poincare algebra are well known, the physical *interpretation of IRs of the dS algebra has not been discussed in a wide literature. To the best of my knowledge, this interpretation has been discussed only in a book by Mensky (1976) (printed only in Russian) and my paper in Journal of Physics (2004). I would appreciate if the editorial decision is reconsidered.*

After that I got this response:

Dear Dr. Lev:

Responding on behalf of the editor, Prof. Wilczek, I want to assure you that he read your request to reconsider your submission. However, he instructed me to let you know that he does not wish to do so.

I am sorry if you took the brief evaluation and quick decision as a lack of consideration on our part. ANNALS values the interest and contributions of authors around the globe.

Very truly yours,

(Ms.) Eve Sullivan Senior Editorial Assistant

for Frank Wilczek Editor-in-Chief

So, the great scientist, Nobel laureate Wilczek considers it beneath his dignity to somehow justify his decision as editor-in-chief. More precisely, the rationale is this: I wanted to and after that there is nothing to discuss. In connection with the stories about 't Hooft' and Wilczek, I was occupied with such a psychological question. It would seem that a person who received the Nobel Prize, went down in history, and secured a comfortable existence for the rest of his life, can afford such a luxury as following the rules of scientific ethics. In particular, to ensure that the requirements set out in the editorial policy of those journals where they are editors-in-chief are met. For example, they could write in editorial policy like this: I am a great scientist and Nobel laureate; therefore, I only accept articles that I like and will not explain the reasons why I accept some articles and reject others. At least that would be fair. But alas, from the above examples it is clear that they themselves violate what is written in their editorial policy.

Above, I quoted the editorial policy of Foundations of Physics, which, as I wrote, is breathtaking. It is known that 't Hooft does not accept the standard interpretation of quantum theory and this, of course, is his right. He accepts in his journal articles with titles like "Quantum discreteness is an illusion" and other articles whose authors do not accept quantum theory. In principle, any approaches have the right to exist, but it would still be interesting to know what the reviewers of such articles write and whether it has any significance. My experience with this journal is that all of my three papers submitted to this journal when 't Hooft was editor-in-chief were rejected, and on the other hand, the journal asked me to be a reviewer for three papers. In all three cases, I wrote a negative review. But, unlike my reviewers, I only write a negative review if I can clearly indicate a mathematical error and in no case will I write a negative review just because the author has a philosophy that I do not like. In two cases the papers were not accepted, but in the third the paper was accepted. Although I explicitly indicated which result was mathematically incorrect, the editor of the paper still wrote that the paper could be published, and the author did not even try to correct this result or somehow explain it. Therefore, I think that the admission criteria are far from being only scientific.

After this digression, I will continue the description of my attempts to publish my papers. As is clear from what has been said above, the main topics were the cosmological constant and quantum theory over finite mathematics. First, I will describe my misadventures with the cosmological constant. One of the papers was sent to Physical Review Letters. They always have a standard reply that the paper is not interesting for the journal and therefore they kick it off without a review. But according to their rules, in this case you can file an appeal and then a member of the editorial board must answer. It is clear that I filed an appeal and after that I received the following answer:

Report of the Divisional Associate Editor – LK13347/Lev

I have read the submitted manuscript "Do we need dark energy to explain the cosmological acceleration" by Lev (LK 13347). The paper consists of a rather elementary discussion of *the nature of constants in general relativity, and attempts to argue that a cosmological constant is somehow natural from the viewpoint of quantum theory.*

This paper has previously been rejected on the basis that it is not suited for PRL. The editors indicated that the paper "does not have the substantial research, major impact innovation, and broad interest needed for publication."

I completely agree. The arguments contained in this article are unlikely to convince anyone that the cosmological constant rests on solid physical ground. The only non-trivial point seems to be on the last page, where the author argues that perhaps the "dS algebra is more pertinent than the Poincare" algebra. But then, this does not help explain the smallness of the observed cosmological constant. And that's part of the whole problem.

I support the editorial rejection of this article.

Sincerely,

Robert Caldwell Division Associate Editor Physical Review Letters

So, at first, he says that there is nothing new in the paper and few people are interested in it. And at the end he says that the only thing that is non-trivial in the paper is that the last page says that the de Sitter algebra is more suitable than the Poincare algebra. But, in his opinion, this does not help solve the problem of why the cosmological constant is so small. These words indicate that he does not understand at all that, as explained above, the de Sitter algebra solves everything and there is no problem of smallness of Λ. He has a standard way of thinking that Λ comes from dark energy and therefore it is necessary to express it through G and explain why it is small. But, as noted above, the dark energy problem is purely artificial and Λ has nothing to do with G.

It is clear that I was trying to publish my papers on Λ in arXiv. Before describing this attempt, a few remarks about my relationship with arXiv. At first, the relationship was perfect. They accepted all my papers in those sections where I wanted and even advised in which section there might be more readers. But everything changed in 2009. I don't know what the main reason was, but they began to publish all my papers in the section "general physics" (gen-ph), although it seems obvious that papers on quantum theory over finite mathematics and Λ have nothing to do with general physics. Moreover, they did this even when my endorsers recommended another section.

The problem with gen-ph is this. If, for example, you have a paper in hep-th, and you think that it may be of interest to readers on gravity (gr-qg), then you can do a cross-listing. And you can't do cross-listing from gen-ph. So, the impression is that for them gen-ph is like a dustbin where they dump what they want to get rid of. And it is clear that if someone is interested in the same problems as me, then he/she will not go to gen-ph, i.e., the probability that my papers will be read in gen-ph is much less than, for example, in hep-th. It is clear that I asked them to publish the paper in the section that I considered more appropriate, but they refused under the pretext that their decision was based on the advice of some moderator. They have so-called moderation system, the description of which is: "arXiv is an openly accessible, moderated repository for scholarly papers in specific scientific disciplines. Material submitted to arXiv is expected to be of interest, relevance, and value to those disciplines. arXiv reserves the right to reject or reclassify any submission." Moreover, they say that the moderator is not a reviewer, and they are not obliged to explain why they decided this way and not otherwise.

Philip Gibbs believes (quite rightly) that such a system is not consistent with scientific principles. He created his website and named it vixra. This word is obtained from arxiv when read backwards. He is absolutely right that if you cannot make arguments against a paper, then you have no moral right to recommend that the paper should not be accepted. Therefore, any scientific paper can be accepted into vixra. So any author can be sure that his/her paper will see the light of day, and this is important not only from the point of view of whether physicists recognize it or not, but also from the point of view of copyright.

So, as I wrote, since 2009 arXiv posted all my papers in gen-ph. But the paper on Λ was not even accepted even there. As usual, there was a standard reply that the moderator thought the paper was not suitable for arXiv. It would seem that the topic Λ is one of the most burning topics in modern physics, so if this topic does not fit, then it is not clear what does. And it is clear that I wrote an appeal. The answer was amazing. They wrote that they would take it only if the paper was first published in some journal. This, of course, completely contradicts the idea of arXiv as an electronic preprint, where papers are posted before they appear in the journal.

Because my attempts to get the article published in some "prestigious" journal were unsuccessful, I submitted it to the Journal of Modern Physics. They have reviewers from everywhere, but the journal is published in China. The review was to the point, I changed the paper a little, they took it and I paid $600 for open access. When the paper came out, I wrote to arxiv that I had fulfilled their requirements and asked them to take the paper. But some moderator answered me that the paper had errors, Journal of Modern Physics is not a prestigious journal, so they won't accept the paper. Well, since the opinion of the moderator is not a review, then no objections are accepted anymore. So, in general, all my attempts to publish papers on Λ ended unsuccessfully, papers appeared only in vixra and Journal of Modern Physics. On the background of the fact that many papers are being written on dark energy, experiments are being prepared, conferences are being held, etc., I think that the reason is clear.

Another attempt to publish a paper on dark energy is Physics of the Dark Universe. This journal is considered very prestigious - it has an impact factor of 6.5. Alessandra Silvestri has been appointed editor in charge of my paper. The first review was not meaningful, and the paper was rejected. I replied that, according to the rules of the journal, there should be at least two reviewers. After that, she suggested that I redo the article, and there were already two reviewers for the new version. One of the reviews was completely positive, and the second was this:

I cannot recommend publication of this paper due to the following reasons:

Essentially, the results discussed in the paper are not new, being largely based on previously published work by the same author, e.g. Ref. [9]. The paper does not address the cosmological constant problem, neither it shows convincingly that "the problem does not arise", as claimed in the abstract. Therefore, this work does not lead to any significant advance of our understanding of dark energy, and for this reason I can hardly see how it would be of interest to the Journal's readership. Section 2 is a naive, incomplete and unnecessarily lengthy discussion of the subject of limiting theories, based on two examples: Newtonian mechanics as a limiting case of special relativity, and the classical limit of quantum mechanics. The related notions of physical dimensions and units of measurement are systematically confused throughout the paper (I recommend https://arxiv.org/abs/physics/0110060 for a clear discussion of the subject). The Author claims that deSitter and Anti-deSitter spacetimes are more "fundamental" than Minkowski spacetime, since the latter can be obtained as a particular case when the cosmological constant (or, equivalently, the curvature radius) goes to zero. However, this only shows that dS and AdS spacetime are more general than Minkowski. Neither the former are more symmetric than the latter, as claimed on page 6 (all of them are maximally symmetric spacetimes). It is simply incorrect to speak of a given spacetime geometry as being more fundamental than another; rather, the attribute "fundamental" should be used with reference to a dynamical theory having a broader regime of applicability compared to a particular limit. Moreover, neither the value nor the sign of the cosmological constant can be fixed following the arguments in the paper. Without a theory (as given by, e.g., an action principle), there is no reason to assume a particular spacetime geometry (e.g. deSitter) as being a valid description for the vacuum. Moreover, it is quite challenging to build a theory of gravity where the cosmological constant (or, equivalently, the deSitter radius) matches the observed value without introducing new tunable parameters: finding such a theory could in fact be regarded as a solution of the cosmological constant problem. Such a crucial aspect is not discussed at all in the paper, and the Author does not propose any theory to frame his discussion.

From this review it is immediately clear that the reviewer is a purely classical physicist, and he/she cannot understand what is written in the paper from the point of view of quantum theory. He thinks only in terms of spacetime geometry and considers that the problem is very important. In addition, contrary to scientific ethics, he makes negative statements without any justification. For example, he writes that the discussion of dimensions is naive and even recommends to me this famous article by three authors. But he does not write what is naivety, whether there are discrepancies with these authors, etc. And it is clear that he does not understand the paper of three authors. Here they express radically different views, and he does not write which point of view he prefers. But now it's clear that it doesn't even matter what he/she writes, but Silvestri found such a reason for rejecting the paper: out of three reviews, two were negative. Of course, I wrote an appeal in which, in particular, I noted that 1) it is quite possible that both negative reviews belong to the same reviewer; 2) in any case, there are two reviewers for this version of the article. And wrote why the review is meaningless:

Ref: $DARK_2019_25_R1$

Title: Cosmological Acceleration as a Consequence of Quantum de Sitter Symmetry, by F. Lev

Author's appeal on editorial decision

The decision is based on reports of Reviewer 1 and Reviewer 3. Reviewer 1 did not say that the paper should not be published. He/she said that it could not be published in the present form because in his/her opinion the paper contained nothing essentially new in comparison with my previous papers. In view of this remark, I considerably revised the paper and now I explicitly explain why the new paper is fundamentally new. So in fact the decision is based only on the report of Reviewer 3.

At the beginning of the report, Reviewer 3 says the same words as Reviewer 1 without any substantiation. Regardless of whether or not Reviewer 1 is the same person as Reviewer 3, for the current version of the paper there were two reviewers with fully opposite recommendations. In such cases in my practice the paper was usually sent to adjudicator. However, in the given case the preference was given to one of the reviewers. Reviewer 3 says that "The related notions of physical dimensions and units of measurement are systematically confused throughout the paper (I recommend https://arxiv.org/abs/physics/0110060 for a clear discussion of the subject)." However, nothing specific is said on what is "systematically confused" and so it is fully unclear whether Reviewer 3 understands what is written about physical dimensions. He/she says nothing on whether or not my paper contradicts this reference. This reference is known and I discuss it in my monograph project https://arxiv.org/abs/1104.4647. The three authors propose considerably different opinions on the problem, and Reviewer 3 says nothing on what opinion (if any) he/she prefers. One of the authors (M.J. Duff) states that the most fundamental physical theory should not contain arbitrary constants at all, and in Sec. 2 I also argue in favor of this statement.

The next part of the report also shows no sign that Reviewer 3 understands my results. First, he/she says that "Section 2 is a naive, incomplete and unnecessarily lengthy..." but nothing specific is said on what is naïve, incomplete etc. Reviewer 3 writes: "The Author claims that deSitter and Anti-deSitter spacetimes are more "fundamental" than Minkowski spacetime..." but there is no such a claim in the paper and the comparison of those spacetimes is not discussed at all. I don't know whether Reviewer 3 understands basic facts of quantum theory, whether he/she works in the framework of this theory or he/she works only in the framework of classical theory. As I noted in my previous emails, many physicists do not understand that spacetime is only a classical notion, and spacetime description is only a consequence of quantum theory in semiclassical approximation.

On quantum level symmetry is defined by the commutation relations in the symmetry algebra as explicitly explained in Sec. 2, and in the formulation of this symmetry nothing is said about spacetime. In the theory of Lie groups and algebras a well-established fact is that if symmetry B is obtained from symmetry A by contraction, then symmetry A is higher than symmetry B. In Sec. 2 I refer to famous Dyson's paper [7] where this fact is explained for groups, and I explain this fact for algebras. Since Poincare algebra can be obtained from dS or AdS algebra by contraction, this automatically implies that dS and AdS symmetries are more fundamental than Poincare symmetry, and this has nothing to do with the relation between de Sitter and Minkowski spaces. The notion of *contraction is a fundamental notion of the theory of Lie groups and algebras, and the report shows no sign that Reviewer 3 has a basic knowledge in this theory.*

Reviewer 3 says "It is simply incorrect to speak of a given spacetime geometry as being more fundamental than another; rather, the attribute "fundamental" should be used with reference to a dynamical theory having a broader regime of applicability compared to a particular limit.". Again, as noted above, I do not discuss spacetime at all because this is only a classical notion. In Sec. 2 I give Definition when theory A is more general than theory B, and this definition explicitly says that a more general theory has a broader regime of applicability compared to a particular limit. So a question arises whether Reviewer 3 read my Definition and whether he/she tried to understand it. Reviewer 3 says: "Moreover, neither the value nor the sign of the cosmological constant can be fixed following the arguments in the paper." As I explain in detail, the problem of the value of Λ *does not arise for the same reasons as the problems of the values of* c *and* $\hbar$ *do not arise. Indeed this statement contradicts the usual dogma that* Λ *should be somehow fixed. However, Reviewer 3 says nothing specific on why in his/her opinion my explanation is incorrect or unacceptable, and so his/her objection cannot be treated as a scientific argument. It is known that relativistic quantum theory itself does not need the values of* c *and* $\hbar$*, and in all textbooks on this theory the presentation is given in units* $c = \hbar = 1$*. The numerical values of* c *and* $\hbar$ *are needed only if one wants to express some quantities in* (kg, m, s)*. The notion of the system of units was proposed many years ago when quantum theory and relativity did not exist. The notion of* (kg, m, s) *is pure classical and physical quantities are expressed in these units only for convenience. The problem why the values of* c *and* $\hbar$ *in units* (kg, m, s) *are as are does not exist since the answer is: because people want to measure* c *and* $\hbar$ *in these units. Reviewer 3 writes: "Moreover, it is quite challenging to build a theory of gravity where the cosmological constant (or, equivalently, the deSitter radius) matches the observed value without introducing new tunable parameters...". As explained in Sec. 2, quantum dS or AdS theories themselves do not need the numerical value of* Λ *for the same reasons as relativistic quantum theory does not need the numerical values of* c *and* $\hbar$*. I also explain the known fact that even for classical dS and AdS theories themselves the numerical value of* R *is not needed. Since Reviewer 3 again raises this question, I will try to explain this obvious point again.*

Suppose for simplicity that our world is a surface of two-dimensional sphere. Then the coordinates on the sphere can be described by two dimensionless polar angles (φ, θ)*. For the description of geometry we do not need the radius of the sphere* R *and we can assume that* $R = 1$*. The quantity* R *in meters has the meaning of the radius of the sphere seen from the three-dimensional space where the sphere is embedded in. But we know nothing and do not need to know about this space and its coordinates. Those coordinates are of interest only when we want to attribute to* R *some value and consider a formal limit* $R \to \infty$*. In this limit a vicinity of the Northern pole of the sphere becomes the flat two-dimensional space.*

Analogously, for dS or AdS theories themselves the value of R *is not important; we can assume that* $R = 1$ *and describe geometry on dS or AdS space by using only dimensionless polar and hyperbolic angles. The value of* R *becomes important only when we consider transition from dS or AdS space to Minkowski one. So the desire to describe* R *in meters does not have a fundamental physical meaning. The question why* R *is as is does not arise since the answer is: because people want to measure* R *in meters.*

The only problem which is indeed important is whether dS quantum theory is more fundamental than AdS one or vice versa. I discuss this problem in my paper in J. Phys. A [9] and in my papers published in J.Math. Phys., Finite Fields and Applications, Phys. Rev. D and other journals where I argue that a quantum theory based on a finite ring or field is more fundamental than standard quantum theory based on complex numbers.

The cosmological constant problem is purely artificial. One first tries to build quantum gravity from Poincare invariance because it is associated with Minkowski background. Then he/she realizes that the expression for the vacuum energy-momentum tensor strongly diverges, and after the cutoff which is called reasonable he/she obtains that Λ *is of the order of* $1/G$ *as expected. However,* *as noted above, on quantum level Poincare symmetry is a special degenerate case of dS or AdS symmetry not because Minkowski space is less symmetric than dS or AdS space but because Poincare algebra can be obtained from dS or AdS algebra by contraction. With the same success one can discuss the speed of light problem or the Planck constant problem.*

Finally, let me note the following. Reviewer 3 claims that my paper is of no interest for the readers of Physics of Dark Universe and for this reason he/she does not want the readers to know about my results. I believe, however, that the readers are interested in knowing different approaches to the problems of their interest. My paper shows that a known problem can be tackled from a fully different approach. I believe that for the readers it would be extremely interesting to know that the result of General Relativity on cosmological acceleration obtained from dS space can be obtained from semiclassical approximation of dS quantum mechanics without using dS space at all (i.e., its metric, connection etc.). This result is obviously more general than the result of General Relativity because any classical result should be a consequence of quantum theory in semiclassical approximation. As I note in my explanations, while in [9] this result has been obtained after lengthy mathematical calculations, in the present paper I give a short description on three pages such that the reader will understand the necessary steps.

Let me also note that my paper is fully in the scope of Physics of the Dark Universe because the editorial policy contains "cosmic acceleration and its alternative explanations". At the same time, Reviewer 3 does not allow alternative explanations and accepts only those approaches which are in the spirit of his/her mentality.

The report cannot be treated as a scientific recommendation because: 1) it contains no sign that Reviewer 3 understands what is done in the paper; 2) scientific ethics implies that all negative statements in the report should be substantiated but all of them are made without any substantiation; 3) the report contains no specific statement on why anything in my paper is incorrect or unacceptable, my only "fault" is that my statements contradict known dogmas which have no physical justification. For those reasons I would appreciate if the editorial decision is reconsidered. I am also grateful to Reviewer 2 for the recommendation to publish the paper and for important remarks which will be taken into account in the next version of the paper.

In addition, I wrote her the following letter:

Dear Professor Silvestri,

Thank you for the info about your decision on my paper. Of course, I believe that the decision is not fair. Please find my appeal attached. I think that the main problem is not that Reviewer 3 understands nothing in my paper and obviously cannot refute my derivations. Everybody knows something and does not know something, and it is impossible to know everything. In my opinion, if a scientist is proposed to review a paper which he/she does not understand then he/she should either decline from being a reviewer or say that different approaches have a right to exist. However, Reviewer 3 believes that only papers done in the spirit of his/her mentality can be published and all other papers should be prohibited such that the readers even should not know about their existence. Reviewer 3 does not understand that it is disgraceful to make negative statements without any substantiation. As explained in the appeal, I believe that my results will be extremely interesting for the readers of Physics of the Dark Universe, and my paper is fully in the scope of the journal. However, if your final decision is that my paper cannot be published in the usual way, I would be grateful if you consider the following possibility. My paper is published but along with the paper you or any reviewer writes a paper or comments explaining why my approach is unacceptable. In particular, the report of Reviewer 3 can be published. I believe this will be extremely important for the readers because they will be given an opportunity to make a judgement and will understand pros and cons of different approaches. Maybe my understanding of Reviewer 3's intentions is not correct, and he/she will appreciate the opportunity to express his/her opinion. Thank you. Sincerely, Felix Lev.

but immediately received this response:

Dear Dr. Lev,

I understand your disappointment, every decision if of course questionable, but our decision is final. Kind regards,

So, she doesn't even want to play the game that she's supposedly honestly trying to figure out. She is the head of a group that writes supposedly highly scientific papers on dark energy. In these papers there is no quantum theory at all, everything is based on classical general relativity, the papers are published by Phys. Rev. and other journals, so that the appearance of great science is maintained.

All this took three months and now it is clear that from the very beginning she was only looking for an excuse to kick off. After that, I wrote to the editor-in-chief of the journal:

Dear Professor Tait, I regret that you decided not to respond to my seminar proposal. The proposal had nothing to do with the fact that my paper was rejected. I believe the results are fundamental and my hope was that physicists at UCI would be interested. In this situation I decided to describe my experience with your journal. For the first time in my practice the editor even did not try to make an appearance of fair treatment.

First the paper was rejected because Reviewer 1 wrote a short (and meaningless) review stating that the paper contains nothing new. According to the editorial policy, a paper should be reviewed by at least two reviewers, but this requirement was ignored. When I pointed out to this requirement the editor changed her opinion and proposed me to revise the paper.

After revision the editor found two reviewers. The report of Reviewer 2 was positive, and the report of Reviewer 3 was negative. Then the editor found the pretext for rejecting the paper that two of three reviews were negative. The pretext obviously is not reasonable for the following reasons. First, it is quite probable that Reviewer 1 is the same person as Reviewer 3. But regardless of whether or not this is the case, for the current version there were two reviewer reports, positive and negative. In that case the paper is usually sent to adjudicator or a board member writes a report. But in this case, in contrast to standard practice, the editor immediately rejected the paper without any additional reports.

The report of Reviewer 3 had no sign that he/she understands what is done in the paper. In addition, Reviewer 3 does not understand that it is disgraceful to make negative statements without any substantiation. I wrote an appeal but again, in contrast to the usual practice, the editor even did not want to consider the appeal and informed me that her decision was final. Ignoring author's appeal fully contradicts scientific ethics.

Let me say a few words about the dark energy problem. Usually, physicists working on this problem believe that since this a macroscopic problem then there is no need to involve quantum theory and the problem can be tackled exclusively in the framework of classical theory. And many physicists working on this problem are not even familiar with very basics of quantum theory. In particular, the report of Reviewer 3 shows no sign that he/she understands basic facts of quantum theory. He/she tried to reinterpret my statement in terms of classical physics, but he/she does not understand that quantum theory cannot be interpreted in terms of classical physics.

Meanwhile, as shown in my paper, it is obvious from quantum theory that the cosmological constant problem (or dark energy problem) does not exist. I tried to explain this obvious fact in my several papers. Some of them have been published (e.g. in Phys. Rev. D) but the papers devoted exclusively to this problem have been rejected even by arXiv. However, I believe that the arguments given in the last version of the paper are so convincing that now arXiv has accepted my paper https://arxiv.org/abs/1905.02788 . I would be grateful if you inform physicists about that paper.

Thank you. Sincerely, Felix Lev.

Editor-in-chief Timothy Tait wrote to me that he was no longer the editor-in-chief. At my request, he forwarded this letter to Stefano Profumo, who became editor-in-chief. It would seem that if the editor-in-chief sees that one of the editors is acting contrary to all the rules of scientific ethics, then he should somehow react. But he did not condescendstoop to answer me and somehow react.

The next attempt was Nuclear Physics B. And the answer came right away:

Dear Dr. Lev,

I have now carefully considered your manuscript and reached the conclusion that it falls outside the scope of Nuclear Physics B. Therefore, I regret to inform you that we are unable to publish your manuscript in Nuclear Physics B. For the kind of articles we publish please refer to http://www.sciencedirect.com/science/journal/05503213 Thank you for giving us the opportunity to consider your work.

Yours sincerely, Hubert Saleur

Editor, Nuclear Physics, Section B

My answer:

Dear Professor Saleur,

You rejected my paper with the motivation that it falls outside the scope of NPB. This motivation is not clear to me. You are the editor responsible for QFT and mathematical physics. The main result of the paper (obtained for the first time) is fundamental even for quantum theory itself i.e. even regardless of applications. It is fundamental not only for cosmology but even for particle physics, and the paper can be also treated as a mathematical physics paper. As noted even in the abstract, physicists usually understand that physics cannot (and should not) derive the values of c and $\hbar$ *but they usually believe that physics should derive the value of* Λ*. Physicists often believe that "fundamental"* Λ *is zero and so QFT can start from Poincare symmetry. They also believe that even if* Λ *is not zero then it is so small that de Sitter symmetry is not important in particle physics.*

As shown in the paper, Λ *is meaningful only in semiclassical approximation while on quantum level one should work with the parameter of contraction from dS or AdS algebras to the Poincare algebra R. The main result of the paper is that R is fundamental to the same extent as c and* $\hbar$*. Therefore de Sitter symmetry is not emergent but is more fundamental than Poincare symmetry. This has several fundamental consequences. I tried to make the paper as short as possible and for this reason I discussed mainly consequences for the dark energy problem because this problem attracts a lot of attention and has been discussed in particular in NPB (for example in A. M. Polyakov . B 834, 316 (2010)). At the same time, as shown, for example in [8], irreducible representations (IRs) of the dS algebra considerably differ from IRs of the Poincare algebra. In particular, in dS IRs a particle and its antiparticle belong to the same IR. Therefore the very notion of particle and its antiparticle is only approximate and even electric charge is not strongly conserved. One IR of the dS algebra splits into IRs of the Poincare algebra for a particle and its antiparticle in the limit* $R \to \infty$*.*

For me it is rather strange that famous Dyson's paper "Missed Opportunities" appeared in 1972 but physicists still believe that fundamental theories should be based on Poincare symmetry. I hope that my paper can change this situation.

I could agree that maybe it was desirable to discuss applications to QFT in greater extents, but I believe that it is obvious that the main result is fundamental even for QFT and particle theory. Also, NPB publishes many mathematical physics papers. Of course, if the paper is sent for review I will take into account referee recommendations. I hope that in view of the above remarks your decision may be reconsidered. Another possibility is that I revise the paper such that it contains the same main result but applications are discussed in greater extent. May I hope that in that case the paper will be sent for review? Is it possible that NPB will invite me to submit such a paper? Let me note that the paper is in arXiv: https://arxiv.org/abs/1905.02788

Thank you.

Sincerely, Felix Lev.

And everything is as usual: Saleur wrote some words, but he is not going to answer the author's objections; the author has no right to appeal.

But recently a miracle happened: after my many appeals, arXiv accepted my paper [11], recently this paper was also accepted by Physics of Particles and Nuclei Letters, and this journal is published by Springer. The paper appeared in [12]. And in my other paper [13] the issue of Λ

was also considered. Professor Odintsov, who is the editor-in-chief of Symmetry journal, was one of the reviewers. Then we had a short correspondence. As he wrote, in his works he explains Λ from more or less generally accepted approaches. I asked him why he accepted my work then. He replied that he believed that different approaches could be published. This is an example of high scientific integrity. Now I'll make a short summary.

The GR formula for cosmological acceleration is simply a consequence of quantum theory in the semiclassical approximation. To prove this statement, de Sitter space and any geometry (metric, connection, etc.) are not needed. We simply consider quantum mechanics of two particles in quantum theory, in which the symmetry is determined by the commutation relations of the de Sitter algebra. The result has nothing to do with whether there is dark energy or not, i.e., dark energy is not needed to prove it, and therefore there is no reason to believe that it exists at all. So, there is no problem of the cosmological constant or the problem of dark energy in principle.

The question of why Λ is so small is, in principle, out of the question for the following reasons. This quantity makes sense only in the semiclassical approximation, and if we want to express this quantity in terms of (kg, m, s), then the result depends on the nummerical values of the quantities $(c, \hbar, R)$, where R is the parameter of contruction from the de Sitter algebra to the Poincare algebra. As explained in Sec. 4.2, the question of why these values are such and not others does not arise in principle.

My result is more general than that of GR since any classical result must be a consequence of quantum theory in the semiclassical approximation. The result is also important for the following reasons. So far, all fundamental quantum theories proceed from background space (although, as noted above, this concept should not exist at all in quantum theory). Many physicists who work on quantum theories of gravity think that background space in quantum theory should be such that in the classical limit it goes into background space in general relativity. For example, Loop Quantum Gravity is based on such a philosophy. But the result on cosmological acceleration shows that the result of GR in the semiclassical approximation is obtained without any background space in quantum theory.

Since my works on cosmological acceleration have been published in my papers and in my book (see chapter 8), at first, I did not plan to continue writing papers about it. But then I thought this. My explanation of the cosmological expansion is very clear, but for some reason no one recognizes it. There are scientific and non-scientific reasons for this.

The non-scientific reasons are obvious. Since there is a lot of activity going on around dark energy - articles, conferences, experiments planned, etc., those who are into it will obviously not want to give it up and admit that it is nonsense. And the scientific reason is this. As I already wrote, even physicists involved in de Sitter invariant quantum theories do not know irreducible representations of the de Sitter algebra. Probably for them, my papers about this are not easy to read because they think that fields are more important than particles. Therefore, they probably have no incentive to understand my work. Therefore, I decided to write an actually popular article about why dark energy is nonsense, and my explanation is natural.

I sent a paper to several journals, but everywhere they kicked it off. Sometimes on purely formal grounds. For example, they have software that finds plagiarism. This software sees that the paper is using sentences that were previously used and concludes that the article is plagiarism. In this case, the system sees that words from my previous works are used and also concludes that plagiarism. I tried to explain to the editors that in this case it is natural because the paper is a popular presentation of my results. But it's like hitting a wall: they see the conclusion of their software and don't want to delve into it anymore.

But I sent the paper to Foundations of Physics, although my experience with this journal, described above, showed that the journal does not follow the principles of scientific ethics and does not follow the rules of its own editorial policy. The reason was this. Although Rovelli, who became editor-in-chief after 't Hooft, does not follow scientific ethics (this is described in detail below), but he is one of the few who wrote that dark energy is nonsense. And my paper begins with a link to a paper by Bianchi and Rovelli (2010) titled "Why all These Prejudices Against a Constant?". So I hoped that, at least for Rovelli, it would be clear what the paper was about and therefore the paper would be reviewed.

The answer, signed by Samuel Craig Fletcher, was this:

Dear Dr. Lev,

We have received your submission FOOP-D-22-00163 entitled "Discussion of cosmological acceleration and dark energy".

Before entering a submission to the reviewing process, we check whether it obeys criteria such as the following:

- Is the topic of research suitable for this journal?

- Does the paper contain original ideas and new results?

- Are the arguments and calculations accurate and correct?

- Is the exposition sufficiently well organized, and worded well?

- Does the overall quality agree with our very tough standards?

I regret to inform you that the editors had to conclude that this work is not suitable for publication in Foundations of Physics.

I would like to thank you very much for forwarding your manuscript to us for consideration.

That is, it is clear that no one read the paper or was unable to understand it. The answer is purely formal. There are five criteria that a paper must meet and no explanation why my paper does not meet these criteria. That is, no attempts to even pretend that scientific ethics are respected. It is clear that I wrote appeal:

Paper: FOOP-D-22-00163 "Discussion of cosmological acceleration and dark energy" Author: Felix Lev

Author's appeal on editorial decision

The problem of explaining cosmological acceleration (CA) is one of the key unsolved problems of modern physics. Almost all the literature on this subject assumes that CA is a manifestation of dark energy. Professor Rovelli is an expert on this problem, and in his paper with Bianchi titled "Why All These Prejudices Against a Constant?" the authors explain that such an explanation is not physical. In my works, I present new arguments in favor of this point of view and explain that CA is a natural consequence of quantum de Sitter symmetry. The purpose of my short letter to FOOP is to present arguments that will be understandable to a wide range of readers. Therefore, I hoped that my paper would be considered by the editors of the FOOP on the merits.

In the rejection letter, Dr. Fletcher first describes five criteria that a paper submitted to FOOP must meet:

1) Is the topic of research suitable for this journal?

2) Does the paper contain original ideas and new results?

3) Are the arguments and calculations accurate and correct?

4) Is the exposition sufficiently well organized, and worded well?

5) Does the overall quality agree with our very tough standards?

and then he writes: "I regret to inform you that the editors had to conclude that this work is not suitable for publication in Foundations of Physics."

The rejection letter does not explicitly say that my paper does not satisfy conditions 1)-5). However, since the paper is rejected, it is understood that it does not meet these conditions. Then the question arises, does it not satisfy all conditions 1)-5) or only some of them? Apparently, according to the meaning of the letter, one must understand that Dr. Fletcher thinks that all of them.

If Dr. Fletcher considers himself a scientist, does he understand that scientific ethics requires that any negative statement in an official rejection letter must be substantiated? The rejection letter does not contain any hint that someone from the editorial board was trying or was able to understand the meaning of my paper. One of the reasons why I sent my paper to FOOP was that *since Professor Rovelli is an expert on the subject then at least he can judge the paper. However, members of the editorial board responsible for my paper either did not read the paper carefully or were not able to understand it.*

I hope that if the editorial board wants FOOP to have a reputation as a journal that respects scientific ethics, then the decision on my paper will be reconsidered.

and in response received a letter from Rovelli himself. He writes that he immediately rejects my article because "unacceptable tone":

Dear Dr Felix Lev, your appeal has been forwarded to me. Given the unacceptable tone of your letter ("If Dr. Fletcher considers himself a scientist,",..., "if the editorial board wants FOOP to have a reputation as a journal that respects scientific ethics, ") I have decided not to follow up on it and confirm rejection definitively. Regards, Carlo Rovelli as FOP Chief Editor.

So Rovelli is probably very proud of himself that he rejected the paper because of my tone. And the fact that they treated me in a boorish way, the article was kept for more than two weeks, no one considered it, and they wrote a stupid rejection - this is not so important anymore. And he does not even have the intention to apologize that such an attitude towards the author is contrary to all principles of scientific decency. And my answer is not in the right tone. And for him the main thing is not whether the paper is important, what results there are, but the fact that my tone is unacceptable. This is one example that Rovelli does not follow scientific ethics. Other examples will be given below.

Next try: Letters in Mathematical Physics. They immediately answered:

Dear Dr Lev,

Your manuscript, MATH-D-22-00107 titled: "Discussion of cosmological acceleration and dark energy"

Author(s): Felix M. Lev

submitted for publication in Letters in Mathematical Physics on 07 Apr 2022 has been carefully considered by the Editors of LMP.

In their opinion, the content does not meet the high standards of our journal and we regret that we are not able to consider your manuscript for publication. Below, please find their comments for your perusal.

I would like to thank you very much for forwarding your manuscript to us for consideration and wish you every success in finding an alternative place of publication.

Comments to the author (if any): This manuscript does not appear to contain new significant mathematical physics of the type published in Letters in Mathematical Physics. I suggest transferring to Gen Rel Grav or similar.

Sincerely Yours,

Christopher Fewster Editor in Chief Letters in Mathematical Physics

That is, at first, they say that they allegedly carefully examined the paper, but then, without any explanation, they say that the paper does not meet the high criteria of the journal. And, of course, problems with scientific ethics do not bother them.

My next attempt was the General Relativity and Gravitation. From there, the following response came rather quickly:

Reviewer comments on your work have now been received. In view of the report and the recommendation of the Associate Editor who handled the paper I regret to inform you that your submission is not suitable for publication in GERG. The reviewer comments can be found at the end of this email or can be accessed by following the provided link.

Thank you for your interest in GERG.

Yours sincerely

Mairi Sakellariadou Editor-in-Chief General Relativity and Gravitation

Reviewer comments:

Associate Editor:

The submission is not appropriate for GRG.

Clearly, this is just a rebuttal. Although it says "The reviewer comments can be found at the end of this email", there are no reviewer comments. And the phrase of Associate Editor does not explain why the paper is not suitable for the journal. It is clear that I wrote an appeal:

...Such an attitude to the author fully contradicts scientific ethics because:

Although the email says that "The reviewer comments can be found at the end of this email", in fact there are no reviewer comments.

The phrase of the Associate Editor: "The submission is not appropriate for GRG." is given without any explanation and contradicts the editorial policy of GERG according to which "Theoretical and observational cosmology" and "Relativistic astrophysics" are in the scope of GERG.

My paper gives a solution to the problem of cosmological acceleration, and my approach is fundamentally new because the solution is given in the framework of quantum theory.

I would appreciate it if the editorial decision were reconsidered.

but received no reply.

The next attempt was to submit a paper to the European Physical Journal Plus. According to their rules, letters can only be submitted at the invitation of the editors. So, I sent the editors this Proposal:

Proposal for a letter to the Editor

The title of the letter is "Discussion of cosmological acceleration and dark energy". The current version of manuscript contains 8 printed pages. It be found in the HAL archive: https://hal.archives-ouvertes.fr/hal-03581039 .

The problem of cosmological acceleration (CA) is one of the hot topics of modern physics and cosmology. In the vast majority of works on this topic, the cosmological expansion is explained as a manifestation of dark energy, quintessence or similar mechanisms. For example, explaining the Nobel Prize for Peebles, some members of the Nobel committee said that he opened our eyes to the fact that we know only 5% of the universe because almost 70% is dark energy and 25% is dark matter.

The generally accepted approach in theoretical physics is such that when new experimental data appear, then, first of all, they should be explained on the basis of the available proven theory. Only if this fails, then some new exotic explanations must be invoked. However, in the case of CA, the opposite approach was taken: there were practically no works in which this phenomenon is explained on the basis of the available results, and in most works the effect is explained on the basis of dark energy and other exotics.

Probably, one of the historical reasons was that Einstein said that introducing Λ was the greatest blinder of his life. Even in textbooks written before 1998 a point of view was advocated 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".

However, several authors (see e.g., Refs. [1,2]) give clear arguments that the explanation of CA by dark energy is not physical. In my publications [2-6] I show that the problem of CA has a clear solution based on well-established results of quantum theory, and the explanation does not need dark energy or other exotic mechanisms the validity of which has not been proved. More details on my publications can be found in my ORCID: https://orcid.org/0000-0002-4476-3080

The generally accepted opinion is that since the problem of CA deals with large macroscopic bodies located at large distances from each other, there is no need to involve quantum theory to study this problem, and the problem must be considered within the framework of General Relativity and other classical theories. However, ideally, every result of classical theory should be obtained from quantum theory in semiclassical approximation.

Consideration of the CA problem from the point of view of quantum theory sheds essentially new light on this problem. For example, in classical theory the case $\Lambda = 0$ corresponds to the flat Minkowski space while the case $\Lambda \neq 0$ corresponds to the de Sitter (dS) space. As noted above, *the usual philosophy is that empty space should be flat and therefore the case $\Lambda = 0$ is preferable than $\Lambda \neq 0$. However, the concepts of background space-time and Λ are pure classical. On quantum level the problem is what symmetry group or algebra is preferable. As shown by Dyson in his famous paper "Missed Opportunities", the dS group is more general (fundamental) than the Poincare one because it is more symmetric, and the latter can be obtained from the former by contraction. In addition, since the dS group is semisimple, it has a maximum symmetry and cannot be obtained from other groups by contraction. This Dyson's result has nothing to do with the relation between dS and Minkowski spaces and with the value of Λ. Consequently, quantum theory based on dS symmetry is more general (fundamental) than quantum theory based on Poincare symmetry.*

It is difficult to imagine standard quantum theory without irreducible representations (IRs) of the Poincare algebra. Therefore, quantum theory based on dS symmetry should involve IRs of the dS algebra. However, my observation is that even physicists working on dS quantum theory are not familiar with such IRs. Some of them give a strange argument that such IRs are not needed because fields are more important than particles.

My results in [2-6] and other publications are based on large calculations. To understand them, the readers must be experts not only in quantum theory, but also in the theory of representations of Lie algebras in Hilbert spaces. Therefore, understanding my results can be a challenge for many physicists. Since the problem of CA and dark energy is very important, I decided to write a short note, which outlines only the ideas of my approach without calculations. I hope that after reading this note, many readers will have an interest in studying my approach because it gives a clear solution of the problem of cosmological acceleration and considerably differs from approaches of other authors.

References

[1] Bianchi, E., Rovelli, C.: Why all These Prejudices Against a Constant? arXiv:1002.3966v3 (2010).

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[3] Lev, F.M.: Finiteness of Physics and its Possible Consequences. J. Math. Phys. 34, 490-527 (1993).

[4] Lev, F.M.: Could Only Fermions Be Elementary? J. Phys. A37, 3287-3304 (2004).

[5] Lev, F.M.: de Sitter Symmetry and Quantum Theory. Phys. Rev. D85, 065003 (2012).

[6] Lev, F.M.: Cosmological Acceleration as a Consequence of Quantum de Sitter Symmetry. Physics of Particles and Nuclei Letters 17, 126-135 (2020).

What's wrong with this Proposal? But their answer showed that they didn't even think understanding:

"Dear author,

we have received and gone through your proposal for a letter to the editor with title "Discussion of cosmological acceleration and dark energy". After some internal discussion we regret to say that we do not consider this work for an invited letter to the editor. We would of course very happy if you could consider EPJP for a regular submission, either with this or with any other topic of your interest. Thanks very much for contacting us.

Truly yours
Gastón García
Editor in chief"

That is, the decision was made after "some internal discussion". How this discussion went, whether someone gave some arguments or just whispered something - this is not mentioned. And this is the answer of a scientific journal with a high reputation!

Now the answer from the Brazilian Journal of Physics:

"The Brazilian Journal of Physics (BJP) aims to disseminate original contributions from all areas of Physics, which, in addition of being scientifically sound, introduce new ideas, insights or processes which can be significant contributions to the knowledge of the area. Differences relative to existing knowledge must be sufficiently emphasised and justified, either on theoretical grounds or on clear physical application. Another very important criteria for acceptance is that the contribution should appeal to physicists of all backgrounds. After analysis, it was concluded that the present manuscript does not clearly satisfy these criteria, being more appropriate for submission to a specialised journal."

As usual, general words without any hint that someone was trying to understand the paper. But the strange phrase is that "the contribution should appeal to physicists of all backgrounds". So, a physicist with any, even the lowest level, should understand? But then any paper where there is something more complicated than 2 + 2 can be rejected. And in conclusion they write that this paper is for a more specialized journal. Their editorial policy says: *"Founded in 1971, this journal presents original and current research on all aspects of experimental, theoretical and computational physics from around the world. The scope includes all fields from the traditional fundamental and applied physics disciplines (atomic, condensed matter, molecular, nuclear, optical, particle and statistical physics), as well as relevant topics of an interdisciplinary nature, such as biophysics, nonlinear dynamics and complex systems , to name but a few."* That is, it seems that the meaning is such that the journal takes papers on any topic. But they declare that my paper that is only suitable for a more specialized journal.

I thought that if so, then there could be no journal more specialized than Astronomy&Astrophysics. But the answer from there is such that even with my great skepticism I could not assume that this could be:

Our Ref. : AA/2022/44085

Dear Prof. Lev,

Thank you very much for having submitted your manuscript entitled:

"Discussion of cosmological acceleration and dark energy"

to Astronomy and Astrophysics.

I regret to inform you that your manuscript cannot be considered for publication in Astronomy and Astrophysics because we do not publish articles that are not authored by members of astronomical research institutes.

Sincerely,

Joao Alves A&A Letter Editor in Chief

That is, if you are not a member of the astronomical research institutes, then you cannot publish papers in their journals, even if they are on their topic and contain outstanding results. But in their editorial, I did not find such a requirement, which obviously contradicts all reasonable scientific criteria. When you register on the journal's website, you fill out a form with your data and there is no such requirement. That is, people can waste time preparing an article for the A&A, but time will be lost if these people are not members of astronomical research institutes. Therefore, I wrote a letter that, if this is indeed the case, then can the journal consider the paper if one of the members of astronomical research institutes says that he/she endorses this paper. But I did not receive any answer, i.e., for Joao Alves, it was necessary to find some reason to kick back and immediately forget, but questions of scientific ethics do not bother him.

Another attempt is Physics Letters B. Here Philippe Brax is the member of the editorial board responsible for dark energy. He is a big person in dark energy, writes papers in which he proposes experiments on dark energy, participates in conferences, etc. Once I wrote to him about my work. I wrote that it is obvious from it that there is no dark energy, that different approaches have the right to exist, but the so-called prestigious journals don't even want to consider my work. Didn't receive any response. And Physics Letters B sent my paper for review. A month and a half later, this "thoughtful" answer came:

Dear Dr. Lev,

Reviewers' comments on your work have now been received. I regret to inform you that they are advising against publication, and I have decided your paper cannot be published in Physics Letters B.

For your guidance, reviewers' comments are available to you from the EM website. For your convenience reviews sent to us in plain text format are also appended below.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,

Philippe Brax Editor Physics Letters B

Reviewers' comments:

Reviewer 1: This paper tries to justify the fact that a non-vanishing cosmological constant is natural from a quantum point of view. The main result of the paper (7) is simply the well known fact that a cosmological constant acts as a harmonic potential on particles leading to a force linear in the distance and here proportional to $1/R$ which quantifies the value of the cosmological constant. This is well known and is for instance used in the Newtonian derivation of the Friedmann equation with a cosmological constant. Hence I cannot see any reason to publish this paper.

It is clear that I wrote an appeal:

"Ms. Ref. No.: PLB-D-22-01075 Title: Discussion of cosmological acceleration and dark energy Physics Letters B

Authors appeal on editorial decision

Dear Professor Brax,

Thank you for your email informing me about the editorial decision on my paper. The decision is based on the referee report. Even the first sentence of the report shows that the referee does not understand the main goal of the paper and does not understand the meaning of the cosmological constant problem widely discussed in the literature. The goal of the paper is not only "to justify the fact that a non-vanishing cosmological constant is natural from a quantum point of view" but, more importantly, to explain that the problem why Λ is as is does not arise. I note that, ideally, any result of classical physics should be derived in semiclassical approximation of quantum theory.

My result (7) is derived in semiclassical approximation of quantum de Sitter symmetry. I note that the result can also be derived in General Relativity (GR) with Λ. However, GR is only a pure classical (i.e., non-quantum) theory, here Λ is simply a phenomenological parameter taken from outside, and the theory cannot explain the known problem that the experimental value of Λ is 120 orders of magnitude less than the value expected from quantum field theory. The referee says nothing on whether my derivation is new and whether it is important that, as noted even in the abstract, it is based "only on universally recognized results of physics and does not involve models and/or assumptions the validity of which has not been unambiguously proved yet (e.g., dark energy and quintessence)".

In the referee's opinion, since Eq. (7) can also be derived from the Friedman equations with Λ then my result is of no interest. However, the Friedman equations also are pure classical, they follow from GR and here Λ also is simply a phenomenological parameter taken from outside.

As I noted in my cover letter, I believe that in physics, different approaches have a right to be considered. However, the report shows that, in the referee's opinion, only those results can be published which are based on approaches which the referee understands. The report contains no hint that the referee is an expert in quantum theory and can judge the results derived in this theory.

In summary, the report contains no hint that the referee tried to understand my results or is qualified to understand. In addition, it took more than a month for writing four trivial sentences. I would appreciate it if the editorial decision were reconsidered."

I explain that the review is meaningless and that the reviewer is completely unqualified: there is no hint in the review that he can judge anything in quantum theory. And got this response from Philippe Brax:

"Dear Dr. Lev,

Thank you for your correspondence. As the paper has been reviewed, PLB will only consider resubmission if the paper has been significantly improved/modified, i.e., a new paper, that this justifies a new reviewing process. As a physicist myself, I would suggest, although it is not my role strictly speaking as a PLB editor, that you may consider sending your improved manuscript to a journal whose style and contents would be more appropriate than PLB."

That is, he writes that since there has already been a negative review, then, no matter what nonsense is written in the review, the author can no longer challenge the editorial decision: only if the author significantly reworks the paper and sends a new version, then this will be considered as a new paper. That is, from his letter it appears that the editorial policy of such a very prestigious journal with a high rating is completely contrary to the principles of scientific ethics. But the editorial policy says:

Physics Letters B ensures the rapid publication of important new results in particle physics, nuclear physics and cosmology. Specialized editors are responsible for contributions in experimental nuclear physics, theoretical nuclear physics, experimental high-energy physics, theoretical high-energy physics, astrophysics, astroparticle physics and cosmology.

It follows that the journal as a whole does not have a universal editorial policy for all papers, and for each paper everything is decided by the member of the editorial board responsible for this paper. Therefore, what he writes is not editorial policy, but his own policy. From this it is clear to me that Brax is not a scientist who observes scientific ethics. In his letter, he even writes how good he is: he writes that although it is not part of his duties as an editor, he advises me to send the paper to some other journal that has a style more suitable than that of PLB. But how does it follow that PLB has the wrong style? As is clear from the above, this style is entirely determined by the editor responsible for the article. So, Brax himself decided that the style was inappropriate. He pretends to give me good advice (even though I know without him that I can send the paper to other journals). But it's obvious to me that his hint is clear: whatever paper I send to PLB, he won't let it through.

One may wonder why he is so against me. I think the following explanation is natural. If you look at his articles, it is immediately clear that he is a typical physicist who does business on QFT, in which you can fish for a long time in troubled waters. In particular, as I noted, he is in the forefront of dark energy apologists. In most of the papers that discuss dark energy from a QFT point of view, the authors consider different models with assumptions that have not yet been confirmed (and it is not clear whether they can be confirmed). These authors argue that their models will turn out to be correct, and in future experiments in which dark energy is discovered, these models will be confirmed.

But Brax and co-authors surpassed them. They wrote a paper [14] claiming that dark energy has already been discovered by the XENON1T collaboration. In this experiment, an excess of the expected recoil electrons was found: 285 events, 53 more than the expected 232 events, and it was said that the significance of this observation is 3.5σ which is treated as a reliable observation. Based on this result, the paper proposes a big science that dark energy particles are born on the Sun and these particles can be detected in their flow to the Earth. There are 248 references in the paper, mostly to deep works on QFT models. Clearly, it is not surprising that this paper, which has so much QFT on the burning topic of QFT in dark energy, was published in Physical Review D, which does not allow deviations from the establishment point of view.

But then there was an embarrassment: a more accurate experiment of the XENONnT collaboration [15] showed that in fact there was no excess of electrons in the XENON1T experiment. Therefore, the article [14] became meaningless. It would seem that it is not good to gloat, since mistakes are always possible. But it would seem that after that Brax should have written a note admitting that the "thoughtful" long article [14] is wrong, and this story should have been an argument for him that different approaches have the right to exist. In my papers, including that sent to PLB, I show that the problem of cosmological acceleration has a clear solution without any assumptions and without dark energy. But he did not accept my paper, and, probably, the reason is understandable: my paper shows that his great work on QFT in dark energy is meaningless.

My general impression of communication with scientists promoting dark energy is this. First, a purely scientific remark. As explained in my works, dark energy is nonsense because there is no problem with the explanation of the cosmological expansion: the de Sitter symmetry is more general (fundamental) than the Poincare symmetry, and the question of why this and not the other is not worth it. But these scientists come up with models, propose experiments, there is a lot of activity, and so on. From the point of view of scientific ethics, it would seem obvious that different approaches should be discussed in the scientific community and, if someone has a different approach, then scientists should be interested in discussing these approaches and establishing the truth. But none of these so-called scientists are going to discuss anything and they do everything not to allow publications that present other approaches. Therefore, I think that the only explanation for this situation is that they need all this vigorous activity to justify their positions, receive grants, hold conferences, etc. They understand that if it turns out that dark energy is nonsense, then they will lose all this. Therefore, the situation with dark energy is the same nonsense as with string theory, and this may continue for a very long time.

Now I turn to the description of my attempts to publish papers on physics over finite mathematics. I sent one of them to Advances in Theoretical and Mathematical Physics. At that time, Yao was the editor-in-chief and the editorial board included many known scientists, such as Witten. The editorial policy of the journal is described in one sentence: "Publishes papers on all areas in which theoretical physics and mathematics interact with each other". It would seem that this is just what I need. I don't know why, but the review came only after 11 months. This review begins with the words: *Quantum field theory over a finite field (what the author abbreviates as GFQT) is a very intriguing proposal, and deserves more attention from both physicists and mathematicians. Publishing it in venues like ATMP would be very appropriate advertising. I would recommend the paper (in some form at least) for publication in this journal. A review of sorts of this paper has already appeared in Int.J.Mod.Phys.B20(2006)1761, so it is good to have some arguments given in more detail.* He then discusses the paper in detail and gives suggestions on how he thinks it can be improved. And the review ends as follows: *In summary, I find this line of research very intriguing. This paper should be published, and I hope the author continues his work in the area. But I would feel more comfortable if some easier physical systems were studied.*

It would seem that there could be no better review. But the editor's response is:

Dear Dr. Lev, Your paper has been reviewed and the report is enclosed. While we understand your concerns that the review process took so long, and while we appreciate your patience, let me say that 11 months is not at all an unusual period for a mathematics journal. This is especially true for a paper, such as yours, that is outside the mainstream of current research and for which it is difficult to find reviewers. It is not easy to find someone who will devote a significant amount of time for a task that does not promote his own research. This is no one's fault, only the normal situation in journal editing. In the present case we were fortunate to find someone who was willing to go beyond the usual thoroughness expected of a reviewer, and we hope you will appreciate his devotion. The review reveals some positive and some negative aspects to your paper. The paper points in new directions, and we appreciate that. At the same time the referee sees significant weaknesses, which can be easily read in the report, and which I will not repeat. The editorial board decided that, on balance, this report does not provide evidence of a paper that comes up to our standards. We have also had informal reports in the same direction. Unfortunately, we have reached the final decision *that we will not accept this paper for ATMP. We thank you for submitting your paper to us and we wish you success in finding a suitable journal for your work.*

With best wishes, and on behalf of the editors,
Charles Doran Editor and Managing Editor
Advances in Theoretical and Mathematical Physics

So, he says that the reviewer notes the positive and negative sides, as well as weaknesses that are clearly visible from the review and which he does not repeat. In fact, what he calls weaknesses are the wishes of the reviewer, but in any case, it is obvious that the reviewer fully supports the publication, but for some reason the letter does not say this. The letter also states that "The paper points in new directions, and we appreciate that".

It would seem that even for the fact that the paper indicates new directions, the journal should gladly publish it. But still, the paper does not meet the high requirements of the journal. Then it is generally not clear what requirements the papers they take satisfy. Do they also indicate new directions or is it optional? Finally, he writes that "We have also had informal reports in the same direction." It is not clear here what informal reports are, whether they exist on paper or just someone whispered something to someone. I don't understand why he is telling me this. How can it be an argument for a mathematical journal that someone has said something, and the author is not told what was said?

Now I will describe the case when the paper was accepted in Physical Review D. The idea of the paper is to briefly describe my results on Λ and quantum theory over finite mathematics.

At first there were two negative reviews, as usual, I wrote an appeal and then the same reviewers wrote that they were against it anyway. It immediately became clear that the reviewers have the same way of thinking as Volovik and Polyakov: when they hear "de Sitter," they immediately decide that this is QFT on the de Sitter space. For example, one of the reviews begins as: "The paper proposes to use modified quantization algebras of de Sitter type, in order to have a consistent quantization of field theories in a de Sitter background", although from the very beginning I try to explain that I do not come from QFT, but from algebra. And in the second review, the reviewer writes that I should start from physics "instead of diluting the physical CC problem in the communication relations of dS algebra".

So, he believes that commutation relations are not physics, but de Sitter space is physics. It would seem that if he considers himself a quantum physicist, then operators are indispensable, but for him this is not physics. And this is the way of thinking of many who consider themselves quantum physicists. He then writes: "For all this I maintain the opinion that this article should not be published in PRD. I would encourage the author to continue working on this problem, but should improve substantially its starting point". So, he encourages me to keep working (and many thanks to him for this), but he thinks that I should change everything from the very beginning, i.e., in the sense that instead of the commutation relations I should start from the empty de Sitter space. And, of course, such physicists think that such a space has some meaning, although the arguments that I gave say otherwise. And, as usual for me, there were arguments that since such mathematics is used, then this paper is for a mathematical journal, and not a physical one. In my appeal, I wrote that I do not do mathematics, but apply it to gravity and elementary particles, but this argument, as usual, was not taken into account. And I also wrote that "In the present paper I discuss only systems of FREE elementary particles, so FOR THE CLASS OF PROBLEMS DISCUSSED IN THE PRESENT PAPER I DO NOT NEED QUANTUM FIELDS AND SPACE-TIME AT ALL" and, to highlight this thought, wrote it in capital letters. And then I wrote: "So I disagree with the referees that only those approaches to quantum theory should be allowed which are based on space-time from the beginning."

According to the rules of the journal, if the reviewers continue to reject, then the next appeal will be considered by a member of the editorial board. I asked that this member be Misha Shifman, whom I knew well from my studies at ITEP.

And he wrote that the idea of the paper was good, and the question in which journal to publish — mathematical or physical — is a matter of taste, and he recommends publication. I am very grateful to Misha for such a review. Interestingly, it wasn't until after the paper was published in Physical Review D [16] that arXiv agreed to move it from gen-ph to hep-th, and before that, all my requests were denied. It is also interesting that in this paper my results on Λ are, although when I tried to publish them separately, as described above, this was rejected.

I hoped that after my paper was published in Physical Review D, other journals would treat me more favorably. But almost nothing has changed. I will give just one example. The editorial board of Letters in Mathematical Physics (LMP) is made up of renowned physicists and mathematicians and their editorial policy says: "We are committed to both fast publication and careful refereeing". But when I sent them a paper, they replied: "Your manuscript has been carefully considered by the Editors of LMP. From their opinion, the content does not meet the high standards of our journal and we regret for not being able to consider your manuscript for publication". Above, I already wrote how my paper was rejected by the LMP. And now they reject it with the same text. That is, they have a standard text for hitting for all occasions. Although they allegedly read carefully, there are no explanations; they also do not understand (or pretend not to understand) that, according to accepted scientific ethics, official negative statements can only be made with justification. And, again, it turns out that what is written in the editorial policy has nothing to do with the real policy of the journal.

When I thought about what to do in my situation, I had such thoughts. Physicists are stupid not because they don't know something. They know a lot and you can't know everything. They do not know finite mathematics and this is also understandable. But the way of thinking of many physicists is such that if they see a paper with mathematics that they do not understand, then, either for internal justification, or for other reasons, they immediately conclude that these are some kinds of mathematical tricks that have nothing to do with physics. If such physicists lived 300 years ago, then, probably, they would also consider papers where there are derivatives to be mathematical tricks. Mathematical physicists, although more qualified in mathematics, still, as a rule, do not accept physics with finite mathematics. But there are mathematicians who deal with finite fields, and for them it should probably be interesting that such fields can be applied to physics. For example, the editorial policy of Finite Fields and Their Application says: "The journal also publishes papers in various applications including, but not limited to, algebraic coding theory, cryptology, combinatorial design theory, pseudorandom number generation, and linear recurring sequences. There are other areas of application to be included, but the important point is that finite fields play a nontrivial role in the theory, application, or algorithm." Judging by these phrases, they do not know that finite fields can be applied to physics, but the meaning of these phrases is such that they want to promote finite fields in different areas.

Therefore, I had the hope that my work would be of interest to them. In 2006 they took one of my papers, but now it is clear to me that the paper was not fundamental, and it happened by coincidence. But the next paper turned out to be a circus. Apparently, their train of thought was such that since the article is physical, it is necessary to send a physicist for a review. And this physicist wrote this review:

Based on earlier work, this paper investigates the possibility of replacing the field of complex numbers commonly used in quantum theory by a finite field. This implies the existence of a new "constant of nature": the prime number p, the finite field's characteristic. Aside from supposed cures to features of the usual theory, which the author considers undesirable (he is obviously unaware of much recent work), he claims that in a theory over a finite field the existence of antiparticles is automatic. He points out that in the usual Poincaré invariant quantum field theories over the field of complex numbers, CPT invariance, which guarantees the existence of antiparticles, is predicated, as is well known, on these theories' assumed locality. For some reason, he views the locality assumption as a shortcoming. Much work has been done by Alan Kostelecky and many others on how locality, Poincaré invariance, and CPT invariance could break down. String theory itself gives clues on how this may happen, but the existence of a large prime number as a new constant of nature is neither *necessary, nor compelling. I do not find this paper suitable for publication in Finite Fields and Their Applications.*

From this review it is immediately clear that the way of thinking of the reviewer is the same as that of Polyakov, Volovik, my reviewers in Physical Review D and others: only QFT is a great science, and everything else is allowed only as an addition to QFT. So, when he sees that finite fields are applied, he thinks it only makes sense to eliminate infinities in QFT.

As I wrote in Sec. 2.5, it would seem that the rules of the game in QFT are strange: at first, they use incorrect mathematics in which infinities arise, and then heroic efforts are needed to eliminate them. But QFT adherents see nothing strange in this and think that it should be so. In this case, he thinks that since Kostelecky and string theory were doing this, then the final fields are not needed. And he did not even try to understand what was done in the paper, and, most likely, he was not able to understand because with his way of thinking, final fields are not needed.

When such reviews are written for physical journals, this can still be understood somehow. But this review is written for a journal whose goal is to promote finite fields in different areas, and he writes that they are not needed. So, the reviewer does not understand how ridiculous his review is. But the journal also calmly accepts a review that completely contradicts its editorial policy and, on the basis of this review, rejects my work. As usual, I wrote an appeal. It is quite long and I won't quote it, but I wrote in it that "I believe it is paradoxical that a reviewer who does not know finite fields writes a report for FFA and recommends rejection because he does not like an approach based on finite fields. Probably he does not understand how ridiculous this situation is." And, as usual, my appeal was not taken into account.

So, it turned out that mathematicians also do not want to take my papers. Their way of thinking is such that since they do not know physics, then my papers can be accepted only if physicists approve, but physicists do not approve. So, it turns out to be a vicious circle.

But I had these thoughts. Since I was among physicists all the time, my way of thinking was such that finite mathematics should be considered from the point of view of application to physics. But if fundamental quantum physics can be constructed starting from finite mathematics, and the results of classical mathematics are obtained as a special case of finite mathematics in the formal $p \to \infty$ limit, then it turns out that classical continuous mathematics in itself is not a fundamental science. This mathematics originated at the turn of the 17th and 18th centuries and is still considered fundamental. As I noted in Sec. 2.5, the concept of infinitesimals contradicts modern quantum concepts, but still, probably due to historical reasons, even quantum theory is based on continuous mathematics. Of course, a lot has been done in classical mathematics, many sections of science and applications in it are substantiated, so it is hard to imagine what could be otherwise. One can, of course, ask how everything would have turned out if, for example, Galois had been born before Newton and Leibniz, but history does not know the subjunctive mood. In addition, many great minds (Kantor, Russel, Zermelo, Fraenkel, Hilbert and many others) tried to justify classical mathematics. Hilbert said that no one will expel us out of the paradise that Kantor created for us. Despite Gödel's theorems and other results, many mathematicians remain in this paradise. It seems that, for some reason, it is simply more convenient for them to stay there.

Based on the foregoing, I began to write in my works that finite mathematics is fundamental not only because fundamental quantum physics must be based on it, but also because classical continuous mathematics is its special case. It seemed to me that mathematicians should be interested in this. But mathematical journals immediately kicked me off under the pretext that it was only philosophy, and sometimes without any pretexts at all. For example, the Forum of Mathematics simply wrote: "Unfortunately, we cannot accept it for publication." without any explanation. The Israel Journal of Mathematics wrote: "Unfortunately your paper is out of the scope of the Israel Journal of Mathematics. Therefore, we cannot consider it for publication."

At least some meaningful answer came from Finite Fields and Their Applications:

Hi Professor Lev,

I circulated your note to the FFA Editorial Board for their input. The responses indicated that they did not feel that this paper is appropriate for FFA. A number of editors did however think your article was of some interest. One suggestion was for you to try submitting your article to the Notices of the A.M.S.

Thank you again for thinking of FFA.

Best wishes,

Gary Mullen

Editor-in-Chief

So, the editor-in-chief says that "A number of editors did however think your article was of some interest." and thanks for that. Of course, following their recommendation, I submitted the paper to the Notices of the A.M.S. and got this response:

Dear Felix,

Thanks for your recent submission to the Notices. Although your remarks are somewhat interesting, they seem to be rather vague and I'll-formed. I cannot publish them in the Notices.

I wish you good luck publishing your work elsewhere.

Sincerely,

Steven G. Krantz

Editor, Notices of the AMS

What is the meaning of his phrase: "Although your remarks are somewhat interesting, they seem to be rather vague and I'll-formed" - is unclear, because it is not explained. Therefore, this way of rejecting a paper is obviously contrary to scientific ethics.

I sent a paper to the philosophical journal "Journal of the American Philosophical Association". This journal was recently founded, and the founders wrote that they would accept only the most fundamental papers. The response of the journal showed that, just as for the editorial boards of journals in physics and mathematics, the understanding of the editors of a philosophical journal about scientific ethics is also, to put it mildly, specific.

The editor John Heil didn't send me the whole review, but a part of the review. There were comments here, but there was no conclusion about what the reviewer recommends: publish, make corrections, do not publish, etc. And the editor wrote that they decided not to take it. I replied that, of course, I understand what the editors decide, and the opinion of the reviewer has only deliberative value. Therefore, I will no longer seek publication, but at least send the entire review so that I know what the reviewer thinks. And John Heil replied: "Felix Lev: Thank you for the note. I'm afraid that I have no further comments to send." I think that such an attitude towards the author does not fit into any ethics. Let's say, as I wrote above, Advances in Theoretical and Mathematical Physics rejected my article, although the reviewer was completely in favor of the publication. But, in any case, they sent the entire review, and before that I had no cases when they sent only a part of the review, and they did not want to send the entire review, despite my request.

I also decided to try again with the Journal of Mathematical Physics, which took my two big papers on physics over finite mathematics in 1989 and 1993, when L. Biedenharn was the editor. Then Roger Newton became the editor, who kicked my paper off under the pretext that it was for a journal on elementary particles (and, as I noted above, such journals kicked me off under the pretext that they were mathematical). After him, Bruno Nachtergaele became the editor, who also found a way to kick off the article. But still, I had hope that this time they would understand that the article was important.

The reviewer's response was:

The author attempts to show that "finite" mathematics is the basis of "ultimate" quantum theory.

After an introduction containing the author's views of the purported role of the "infinitely large and small" in standard mathematics, the education of physicists, and a critique of negative numbers, the author turns to modular arithmetic as a new foundation of physics. Just as it was discovered that earth is not flat, the modulus p is to give a curvature to the universe. Taking the limit p to infinity is compared to a contraction in group theory. It appears inconsistent here that *this discussion mentions Lie groups, which are based on continuum notions of mathematics that the author calls "classical" and rejects as unsuitable for his purposes.*

The mix-up of "classical" and "modular" objects permeates the entire paper and makes it unclear what is intended or used where. For instance, if the author's mathematics is truly finitist, what is the logarithm he uses in the section about gravitation?

Section 3 contains a discussion of the vacuum field energy. It turns out to be finite if p is finite, just as it would in a quantum field theory with another regulator.

In Section 4, p is estimated as $exp^{10^{80}}$ *by an order-of-magnitude consideration.*

In the concluding section, Gödel is cited and classical mathematics is once more identified as a "degenerated case" of finite mathematics.

In summary, the author's attempt fails, and the paper is unsuitable for any journal with mathematical standards.

So, for the umpteenth time, my paper on the application of finite mathematics in physics is given to a reviewer who does not even know the very basics of finite mathematics. In my appeal, I analyzed the review in detail and showed it. But here another circus is that the paper was sent not to a physical journal, but to Journal of Mathematical Physics and its section 35 - Methods of Mathematical Physics. Therefore, I wrote to the editor that the paper should be judged based mainly on its MATHEMATICAL results, and for this the reviewer must have at least initial, knowledge in finite mathematics. And what does the editor of the Journal of MATHEMATICAL Physics say in response? He's writing:

Dear Dr. Lev,

We regret to inform you that your request to appeal the decision on the manuscript cited above has been declined. I reviewed the Associate Editor's recommendation and made additional inquiries. I am in agreement with the Associate Editor and conclude that Journal of Mathematical Physics is not a suitable journal to publish this paper.

Sincerely,

Bruno Nachtergaele, Editor

So, he writes that he once again looked at the recommendation that the Associate Editor gave and asked someone. And he is not going to give any answer on the merits. So, in fact, the answer of the editor of the MATHEMATICAL journal is such that it is not necessary to understand the MATHEMATICAL issues of the paper!

Another one of my attempts was with the European Physical Journal C. It has a section "Mathematical aspects of quantum field theories, and alternatives". There are two keywords in the title of the section: mathematical and alternatives. The first word gives hope that the paper will be considered by those who understand something in the mathematics that is in the paper. And the second word gives hope that the journal can consider something that is not QFT or string theory. And I sent a short paper there in the Letters section. But, as usual, the reviewer did not want to understand. His/her answer was this:

The article consist of two parts. In the first part the author argues about different aspects of mathematics. This part (section 1) clearly does not fit into the scientific area of EPJC.

In the second part the author tries to apply his theory, FQT (as motivated in the first section) to particle physics. Unfortunately, all applications rest on derivations done in Ref. [3] by the same author. This article, however, has not been published in an internationally renowned physics journal, and thus cannot serve as a sound basis for further exploration.

I suggest to the author first to publish [3] in one of the known journals such as JHEP or Phys. Rev. D. Subsequently, it could be used for further exploration.

The paper does not fulfill the high scientific standards of EPJC. I cannot recommend publication.

The main reason for the reviewer not to understand is this: all the calculations are only in the paper [3] in arXiv, and not in some known journal. Therefore, I must first publish a paper in a well-known journal. So, the logic is this: in order to publish a short paper as a letter, you must first publish a detailed paper. This logic does not climb into any gates, because common practice is the opposite. In addition, without making any attempt to understand, he concluded that the paper did not correspond to the high level of the journal.

Of course, I again wrote a detailed appeal and replied that the editorial policy of the journal says:

Letters must describe new and original work deserving rapid publication. Their aim is fast and concise communication of material of current interest:

1) an important theoretical, computational or experimental result

2) a valuable discussion of, or a short essay on, an open scientific issue

3) a valuable presentation of innovative and promising ideas and concepts

and my paper satisfies all these requirements.

But now I have been answered by Professor Heinemeyer, who was the member of the editorial board responsible for the paper. His answer is:

The serious problems with the paper unfortunately persist (and the author did not change his previous version to accommodate any of the criticism). The author simply argues that his work should be published, because the main title of the EPJC section contains the word "alternative". Still those alternatives must be well founded.

The author still refers mainly to [3], which is unpublished. But also his other articles only received self-citations, and thus his theory cannot be regarded at all as even vaguely accepted, neither by the mathematics, nor by the physics community.

Again, the author should first publish [3] in one of the well reputed physics journals. We cannot recommend the paper for publication in EPJC.

And again, the unwillingness to understand is justified by the same senseless arguments as in the review and the editors are not going to follow the rules that are proclaimed in their editorial policy.

Some friends told me that my attempts are doomed to failure because no one wants to delve into, they don't know me and they see that I'm not from a university, but from a company like Horns and Hoofs (this is the name of the company from the humorous novel "12 chairs" by Ilf and Petrov). They advised me to try to get to some conferences.

And such an opportunity appeared. I received an invitation to attend the Fq12 Finite Fields Conference held in Saratoga Springs, New York in July 2015. Why I received an invitation - I do not know. Maybe because I had one article in Finite Fields and Their Applications, and all authors were automatically invited. I agreed, sent an abstract of my report and the proposal for the report was accepted. It is clear that no one paid me the costs of the conference, and I paid for the flight and accommodation myself (about $2,000).

I was hoping that mathematicians working on finite fields would be interested to know that, contrary to popular belief, it is finite mathematics that is the most fundamental, and classical mathematics is its special case. There were many more people at my report than on average at other session reports. But then some people told me that bridges are built, planes fly, and differential equations work here. My arguments that such equations are also a special case of finite mathematics did not seem to be taken seriously. I was surprised that mathematicians working on finite mathematics still believe that classical continuous mathematics is fundamental. I got the impression that the horizons of these mathematicians are rather limited: they work on their own special problems in this area, they know how to publish, receive grants, etc., and they do not care about "high matters". Therefore, it is easier for them to think that this is only philosophy. However, as I wrote above, many physicists have the same way of thinking, but here QFT plays the role of the party line, and deviations are characterized by other words (for example, not philosophy, but exoticism, pathology, masturbation, etc.). I read somewhere that Rutherford forbade the employees of his laboratory to discuss the Universe because it is chatter that gets in the way. And in America, a very popular phrase is "Just do it," the meaning of which is also that you need to do something specific, and not chat. It can be understood that this rule is reasonable, for example, in some conveyor production.

However, many physicists and mathematicians believe that this principle applies to their science as well. As I wrote above, there was an unspoken rule at ITEP that if you are not a great scientist, then you should scribble your articles and especially not stick out.

My paper submitted for conference proceedings was rejected, although it was in full compliance with the editorial policy of Finite Fields and Their Applications. There was no review of the paper, but Gove Effinger, who was the Conference Chair, wrote:

I am sorry to inform you that the editorial board believes that your paper, though definitely thought provoking, is not appropriate for this volume. We hope that you can succeed in finding a venue for the paper which is aimed a bit more toward "philosophy of mathematics", which ours is not.

That is, although the paper is "definitely thought provoking", it still does not fit as philosophical. In fact, there was no philosophy in this paper, there were purely mathematical results of calculations in finite fields for particle physics and gravity. Sec. 1 was called Motivation and there were arguments why finite mathematics is the most fundamental. If you wish, you can say that these arguments are philosophy. I think that in fact the reason for the refusal was that they simply do not understand what has been done, they see that these are not the tasks they are used to and therefore it is easier for them to find a reason for refusal, saying that this is philosophy.

In the end, my little paper "Why Finite Mathematics Is the Most Fundamental and Ultimate Quantum Theory Will Be Based on Finite Mathematics" was nevertheless published in the journal Physics of Particles and Nuclei, which is being prepared in Dubna and published in Springer. The review was completely favorable. My observation is that although there are problems with science in Russia now, there are still many qualified physicists in Dubna who, moreover, unlike many physicists in the West, follow the rules of scientific ethics. I will write more about this.

And in conclusion of this chapter, I note that, after many attempts, the paper "Discussion of cosmological acceleration and dark energy" was published in the proceedings of the 25th conference "What Comes Beyond the Standard Models" (Bled, July 4–10, 2022) and after that the arXiv agreed to take it: [17]. Here is the abstract of this paper: *The title of this workshop is: "What comes beyond standard models?". Standard models are based on Poincare invariant quantum theory. However, as shown in the famous Dyson's paper "Missed Opportunities" and in my publications, such a theory is a special degenerate case of de Sitter invariant quantum theory. I argue that the phenomenon of cosmological acceleration has a natural explanation as a consequence of quantum de Sitter symmetry in semiclassical approximation. The explanation is based only on universally recognized results of physics and does not involve models and/or assumptions the validity of which has not been unambiguously proved yet (e.g., dark energy and quintessence). I also explain that the cosmological constant problem and the problem why the cosmological constant is as is do not arise.*

I hope that this abstract clearly states that there is no problem with the explanation of the cosmological acceleration since it is explained on the basis of existing science; therefore, dark energy is not needed and there is no problem with the cosmological constant. It would seem that since this paper is now in the arXiv, then all adherents of dark energy should read it and say whether the paper is correct or not. If it is wrong, then a paper should be written explaining why it is wrong and why dark energy is needed. And if it is correct, then they must admit that all their vigorous activity in dark energy does not make sense. But, most likely, there will be no reaction and the establishment will pretend that this paper was not noticed. They may have a reason to pretend that the paper was not noticed, since, as I wrote, arXiv persistently puts my articles in gen-ph. When I asked for the paper to be transmitted into gr-qc, I got their usual response: "After careful consideration, our moderators have denied your appeal. We understand this is a disappointing result, but please note this is the final decision and no further consideration will be given." As usual, what the careful consideration was is not clear.

When I told Maxim Khlopov about my problems, he advised me to submit this paper to a conference in Bled, where he is one of the organizers. Maxim said that at this conference the paper would be judged on its merits, and there will not be a problem whether or not the establishment will agree with this paper. I have known Maxim since our studies at MIPT, and I am very grateful to him for his advice.

Chapter 6

Paradox with the observation of photons from stars and attempts to publish papers on this problem

In the previous chapter, I described my attempts to publish papers on the cosmological constant and on physics based on finite mathematics. But there was another story associated with such a problem. A known effect of quantum mechanics is that the wave function of a free particle spreads: the size of the region of space where this function is significant increases with time all the time, and in the formal limit, when time goes to infinity, this size also goes to infinity. When quantum mechanics was created, this problem was considered by Schrödinger, de Broglie, Darwin and other scientists. It is described, for example, in Dirac's textbook on quantum mechanics. But then interest in the problem apparently disappeared and, for example, in the fundamental textbook on quantum mechanics by Landau and Lifshitz, there is not a word about spreading.

De Broglie believed that the existence of spreading indicates the incorrectness of standard quantum mechanics. He suggested describing a free particle not by the Schrödinger equation, but by using a wavelet that satisfies a nonlinear equation and is not spreading. Therefore, a natural question arises in which experiments the spreading is manifested. As Darwin showed in 1927, for macroscopic bodies, the time for which at least some significant spreading occurs is so huge that for them this effect can be neglected. This effect can be significant only for elementary particles.

I'm not sure that most physicists in this field have delved into this problem and evaluated the spreading in experiments with elementary particles. They probably think that in these experiments the time is so small that the spreading does not have time to manifest itself. My observation is that most quantum physicists either do not delve into the problem or think that the problem is insignificant. Therefore, for them, the spreading effect is not a reason to revise standard quantum mechanics.

When I worked on another problem, I had this simple thought. Photons from stars can fly to us even for billions of years. For them, spreading will certainly be significant. According to my observations, most people think that photons from stars fly towards us, roughly speaking, like bullets, i.e., almost on classical trajectories. Roughly speaking, if the Earth is at point A, and a star is at point B, then photons from this star fly towards us along a straight line connecting these points. But if the spreading is significant, then there are no classical trajectories, and then the question arises why it seems to us that photons fly along classical trajectories. In standard quantum mechanics, spreading is easily considered because the relationship between coordinates and momenta is: the wave function of a particle in the coordinate representation is obtained from the wave function of the particle in the momentum representation using the Fourier transform and, accordingly, the wave function of the particle in the momentum representation is obtained from the wave function of the particle in the momentum representation in the coordinate representation using the inverse Fourier transform (and this is a matter of terminology which Fourier transform is considered direct and which is inverse).

A question arises whether there is such a connection for a photon, which is not a non-relativistic particle, but always moves with speed c (assuming that the photon is a particle with zero mass). This issue has been discussed in the literature, and some authors (for example, Akhiezer and Berestetsky in their fundamental textbook on quantum electrodynamics) have argued that the photon has no wave function at all in the coordinate representation. The corresponding arguments are known, and, for example, they are discussed in my works. But the fact that some coordinate wave function of the photon must exist is obvious from simple considerations. For example, if a photon emitted by Sirius flies towards the Earth, then the theory should determine, at least approximately, where this photon is at a given time: still in the vicinity of Sirius, halfway, close to the Earth, etc. Even Pauli wrote that the coordinate of a photon cannot be measured better than its wavelength. But photons from stars have such small wavelengths that measuring coordinates with such accuracy is quite enough.

The coordinate wave function of a photon has been considered by many authors. Their results differ in spin terms and behavior at distances less than or of the order of a wavelength. But it makes sense to consider the motion of a photon from a star to us only in the semiclassical approximation. In this approximation, the contributions of the spin terms and the behavior at distances of the order of the wavelength are insignificant. If these contributions are not considered, then, for all authors, the coordinate and wave functions of the photon are related by the Fourier transform, as in standard quantum mechanics.

Therefore, it is not difficult to estimate the spreading of photons from stars. The result depends on assumptions about the reactions in which these photons are produced. But, even in the most optimistic scenarios, the wave functions of photons born on Sirius come to us with dimensions of tens of millions of kilometers or even more. Sirius is the brightest star in the sky, at a distance of "only" 8.6 light-years from us. And the wave functions of photons from other stars are much larger, which can even be on the order of light years or more.

So, if we accept the standard quantum theory, then it turns out that photons from stars do not move along classical trajectories towards us, and their wave functions are of enormous size. Does this contradict how we see stars or not? It would seem that the question is so obvious that the answer should have been known for a long time and this should be explained even in textbooks on general physics. But I asked different physicists and they have different opinions.

One of the explanations is this. Photons from stars fly to us not in the void, but in the interstellar medium. Therefore, photons cannot be considered free, because they can interact with particles from the environment. Suppose a photon is absorbed by some particle and re-emitted. Then its wave function will no longer have large dimensions; it will have dimensions of the order of the dimensions of the detector and the so-called wave function collapse will take place. Therefore, in some approximation, we can again assume that the photon approximately moves along the trajectory.

This explanation has a historical analogy. The famous story is that when Hubble discovered the expansion of galaxies at his Mount Wilson Observatory near Los Angeles, Einstein came to him and said the famous phrase that the introduction of Λ was the biggest blunder of his life. At that time, the conclusion about the expansion of the Universe was made, based on the Doppler effect, that the faster an object moves away from us, the stronger the redshift. Now the theory has become more complex because it involves GR and also considers cosmological and gravitational redshift. But some physicists believed that the explanation is not in the Doppler effect, but in the fact that the farther the object is from us, the more energy the photon loses from it since it interacts with a large number of particles. This approach is called tired light.

However, the tired light approach is currently rejected by the establishment. It is believed that the explanation is such that if the interactions of photons with the interstellar medium would be significant, then the images of stars would not be clear, but blurry. If this explanation is accepted, then we can assume that, in a good approximation, the photon flies towards us as a free particle, and the problem with the large size of the wave functions of photons from stars remains. My understanding of this problem has changed and at one time in some ways it was erroneous. More on this below. But I will describe this story in chronological order.

If the photon's wavefunction really spreads that much, then several problems immediately arise. For example, the explanation that we see pulsars is this. These are neutron stars that have a radius of about 10 km, a mass of the order of the sun, a strong magnetic field, rotate rapidly and are located at distances of thousands of light years from us. We see a signal from them only in the short time when it is directed at us. Popular literature compares this to the beam of a lighthouse. But if the wave function of a photon were so strongly blurred, then nothing similar could happen. The situation is similar to gamma-ray bursts, the sources of which can be even billions of light-years away from us.

These problems are obvious. But I thought (incorrectly) that the main problem was that if the photon's wave function spreads out so much, then we shouldn't see individual stars, just the background from all the stars. But now it seems to me that the paradox is even more unusual. This problem will be explained below. But, regardless of whether this problem exists or not, the following question arises: the rule that the coordinate and momentum representations are connected by the Fourier transform is a law that follows from some theoretical considerations, from experimental data, or from what?

Different authors give different arguments in favor of this rule. For example, Heisenberg considered a gedankenexperiment with a microscope, Dirac proposed the hypothesis that the commutator of the position and momentum operators should be proportional to the classical Poisson bracket with the coefficient $i\hbar$, Landau and Lifshitz write that with such a rule, the correct semiclassical approximation is obtained, etc. At best, these considerations are only arguments that the rule is reasonable. There is no discussion about whether there are other rules and which ones are better. Historically, the rule has been accepted from the very beginning of quantum theory and is so firmly rooted in the minds of quantum physicists that many do not even think that it could be otherwise. From this rule follow the famous uncertainty relations, which are discussed even in the popular literature. However, as noted above, the effect of spreading inevitably follows from this rule, and then problems arise.

I have shown that in fact the standard coordinate operator does not give a correct semiclassical description and proposed an operator that gives such a description. And then it turns out that in the direction perpendicular to the momentum, there is no spreading, and for a photon there is no spreading in the longitudinal direction. Therefore, for a photon there is no spreading at all, photons really move towards us approximately along classical trajectories and the problems noted above do not arise.

Naturally, I tried to publish these results. The approach of various journals and physicists with whom I tried to discuss these results was this. Nobody wanted to consider my coordinate operator at all. Everyone assumed that the standard relationship between the position and momentum operators was correct, but no one tried to argue in favor of this. All referees simply proceeded from the fact that this must be accepted and there can be no paradoxes. The argument of one of the reviewers in Physical Review D was that we can see the stars and therefore there is no problem. And the other reviewer didn't even understand the meaning of spreading.

Now I understand that there was something reasonable in the response of the editorial board member. He gave the problem considered by Mott and Heisenberg. Let's assume that there is a source emitting α-particles in spherically symmetric states. But when such α-particles enter the cloud chamber, they leave a rectilinear trace there, which gives the impression that before entering the chamber, the particles moved along rectilinear trajectories. However, I considered the spherically symmetric case in the paper and wrote that there is no problem, and there is a problem only for photons that were formed in the states of wave packets. Perhaps this editorial board member meant that a similar argument applies to package states, but it was not mentioned.

I thought that, probably, they don't want to consider the paper because they don't know me and they see that the author is from some incomprehensible office. Therefore, I decided to send it to Uspekhi Fizicheskikh Nauk, which is considered the most prestigious physics journal in Russia. V.L. Ginzburg was its editor until his death, and therefore I had the hope that the journal retained its level.

The first reviewer wrote that I have a high level, but there is no problem. The reviewer writes: "The author himself speaks about the discussion paper. I quote: "my calculation in the standard theory shows ... that we should not see individual stars at all .... Thank God, we see them." That is, the argument is almost the same as that of the reviewer in Physical Review D, but even stronger because God is also involved. I wrote a polite answer explaining why the reviewer's arguments are wrong.

But the paper was given to another reviewer, who did not even want to comment on my answer, but wrote a review that was not only illiterate, but also boorish. The reviewer writes: "This work is written at a non-professional level. The results, based on blunders, are buried in a large number of well-known facts from textbooks on quantum mechanics." It would seem that if he claims gross errors that I somehow buried, then the reviewer's task is to unearth them and show clearly where the errors are, for example, to say that in such and such a place the author writes 2 + 2 = 5, but it should be 2+2=4. But there are no such statements.

He thought he had found a refutation of my main argument: "The 'paradox' set out in section 7 is based on a blunder. Namely, the author declared the state (32) in which the occupation numbers of all states are equal to 1 or 0 to be 'classical'. This contradicts the standard conditions for large occupation numbers in the semiclassical state. State (32) is not described by the classical solution of Maxwell's equations, which is the resolution of the "paradox". By the way, the 'coherent' states mentioned in Section 8 are precisely the classical ones (again, contrary to the author's assertion)".

From this phrase it is immediately clear that the reviewer does not understand the foundations of quantum theory, since in it, only operators, not states, can be described by Maxwell's equations. The misunderstanding of quantum theory is also evidenced by the proposal on large occupation numbers and the last sentence. I will dwell on these issues in more detail.

The usual phrases are such that the classical theory is applicable when there are many photons, and when there are few of them, then quantum theory must be applied. This is even written in some textbooks, for example, in the book "Optical coherence and quantum optics" by Mandel and Wolf. But everything is not so simple. Of course, the classical theory can only be applied in cases where there are many photons, but this condition is not enough. There is one more parameter - the number of possible states. If this number is much larger than the number of photons, then the average occupation numbers are much less than one, the exchange interaction is insignificant, and the Boltzmann statistics, which are classical, apply. However, if the number of possible states is of the order of the number of photons or less, then the average occupation numbers are no longer much less than unity, the exchange interaction becomes significant, and Fermi statistics for fermions or Bose statistics for bosons are applicable. Because exchange interaction is a purely quantum effect that has no classical analogue, then the question of which case is classical and which is quantum is not a question of terminology, but a question in essence. All this is explained in standard textbooks on statistical physics, for example, in the textbook by Landau and Lifshitz. In particular, coherent states in lasers are states where the number of photons is much greater than the number of states. Therefore, here, although there are many photons, this is a purely quantum case, and not a classical one.

Now I see that despite the reviewer's misunderstanding of the foundations of quantum theory, there is something positive in the review. The reviewer writes: "... At the same time, it is absolutely unimportant whether the wave function of a photon has the form of a narrow divergent beam or the form of a spherical wave. After refraction in the lens of a telescope or in the lens of the eye, a wave with a fixed direction of the wave vector gathers into a point - the image of a star."

At that time, I did not take these words seriously because the rudeness of the reviewer and his lack of understanding of the foundations of quantum theory immediately set me up for the fact that he could not say anything smart. But when later I began to discuss this issue with other physicists, some of them said approximately the same words. I asked them about the same thing. My standard quantum mechanical calculation shows that due to spreading, photons from a given star will come to us with different momenta, and not just those that are directed from the star to the Earth. And what you say is somehow confirmed by the calculations? What, classical or quantum? Everyone answered approximately the same thing: this follows from simple physical intuition. So, it was clear that our brains are arranged differently: apparently, I have no physical intuition and I trust only calculations, but they believe that this is obvious even without calculations.

Finally, Tolya Kamchatnov, with whom we studied together in the 527th group of the Moscow Institute of Physics and Technology, began to give new arguments why he was right, and not me. We exchanged about 50 letters. Finally, I figured out how to formulate a problem so that the result can be confirmed by a quantum mechanical calculation. And it turned out that he and others who said similar words were right, and I was wrong.

Therefore, in those papers that I sent to Physical Review D and Uspekhi Fizicheskikh Nauk, one of my important statements was wrong. If the reviewers wrote clearly about this, then I would be grateful to them, and it is clear that I would not have any complaints. But when the argument is that, thank God, we see stars, or that my quantum state does not satisfy Maxwell's equations, then after that it is already difficult to take reviews seriously.

So, if we accept the worldview of Tolya Kamchatnov and other people who said similar things, then we get the following picture. Photons from stars do not come to us along classical trajectories because the wave functions of photons are strongly blurred and can even have dimensions of the order of light years. But with a probability close to one, photons from a given star will be received only with momenta directed from the star to us. So, this is a generalization of the Mott-Heisenberg problem to the case when the wave function of a photon is not necessarily symmetric, and this can be confirmed by calculation.

I asked Tolya and others, what about the observation of pulsars and gamma-ray bursts then? But they said that this is a separate story that needs to be dealt with. And to the question of whether the rule follows that the coordinate and momentum wave functions are related by the Fourier transform, from some theoretical considerations or from experiment, the answer was such that in the story with spreading, the theory does not contradict experiment, and this is an argument in its favor. But even if Tolya and these people are right, it is not clear why this issue is not described in any way in textbooks, even in textbooks on general physics. As I noted above, many physicists have not thought about this at all and think that photons from stars go to us along approximately classical trajectories.

And yet, thinking about this problem, I concluded that the situation when the wave functions of photons from stars have enormous dimensions does not correspond to what we observe. To explain this as simply as possible, consider the case where the wave function of a photon emitted by a star is spherically symmetrical. Then a simple calculation shows that the wave function of a photon is a sphere, which at each time t has a radius ct and some very small thickness a, which does not change with time. Over time, the radius of this sphere becomes larger and larger, and if the distance from the star to the Earth is L, then on the way to us, the sphere passes through all the stars and planets that are less than L from the star, even those stars and planets that are from the star in the opposite direction to the Earth.

The question arises: why such a photon was registered on Earth and was not registered on stars or planets through which the photon's wave function passed? The answer may be that the process of registering a photon is purely probabilistic: we were just lucky that the photon decided to make us happy and allowed itself to be registered with an eye or a telescope. But then another question arises. If on the way to us a photon passed stars and planets without being registered there, then with approximately the same probability it can pass the Earth and be registered on the opposite side of the Earth. And then we would see the stars through the Earth.

Moreover, consider such an experiment. Suppose we look at a star, and then we place a small screen in front of the eye and the star. Then experience says that we cannot see the star through the small screen. But since the wave function of a photon has passed through so many stars and planets in its path, then, with about the same probability, it will pass through the screen, and the photon will be registered by the eye behind the screen, so that we can see the star through the screen.

This situation does not seem too unusual in light of what we know about neutrinos. We know that neutrinos can not only easily pass through the Earth without interacting with it, but even neutrinos born in the center of the Sun reach the Earth without any problems. The main neutrino detectors are located deep underground and, for example, in the OPERA and ICARUS experiments, neutrinos produced at CERN traveled to the laboratory at Gran Sasso in Italy about 730km across the Earth. The explanation is that because at low energies, the weak interaction is really weak, then the probability of neutrinos interacting with atoms or molecules inside the Sun and the Earth is very small. A photon interacts with such atoms or molecules not weakly, but electromagnetically. At low energies, the electromagnetic interaction is much stronger than the weak one. But, on the other hand, because the wave function of a photon is enormous (light years or more), then the probability of interaction of such a photon with each given atom or molecule is much less than for a neutrino from the Sun.

It would seem that the problem is obvious, but some physicists with whom I discussed it said that there is no problem here either. In their opinion, the situation here is analogous to diffraction in classical electrodynamics.

Indeed, consider the case when a wide wave hits an object whose dimensions are much smaller than the wave width. Then, after passing through the object, the part of the wave that was far from the object will not change, in that part of the wave that was inside the object, a hole will appear, and the only problem is what will happen to that part of the wave that is close to the boundary of the object. The classical theory says that when the wave moves away from the object at a distance called the Rayleigh radius, the hole will tighten, and everything will look like a situation as if the object would not exist. Therefore, if the analogy with the classics worked, then there would be no problems: immediately after passing through the object, the wave function of the photon would be equal to zero immediately behind the object, and there could be no situation when the photon is registered on the far side of the Earth or behind the screen.

However, such an analogy with the classics cannot be for several reasons. First, a qualitative explanation of classical diffraction is evident from the fact that a classical electromagnetic wave consists of many photons. Indeed, suppose, for simplicity, that these photons are (almost) pointlike. Then with those photons that do not fall on the object, nothing happens at all, and those photons that fall on the object are absorbed by this object. But in our problem, we are dealing with only one photon, whose wave function is not (almost) a point but has enormous dimensions.

Secondly, the wave function of an elementary particle cannot be interpreted as a classical wave. The term "wave function" arose at the beginning of quantum theory for explaining quantum phenomena in classical language, but such an explanation does not exist. For example, consider an electron whose electric charge is e. Then the quantity $e|\psi(\mathbf{r})|^2$ cannot be considered the electron charge density at the point $\mathbf{r}$ (at least in the classical sense) because the electron charge is indivisible. And if dV is the volume element at the point $\mathbf{r}$, then $|\psi(\mathbf{r})|^2$ shows not what part of the electron is in this volume element, but only with what probability the electron as a whole can be found in this volume element. I think, for example, the term "state vector" is more appropriate, but historically the term "wavefunction" is used, even though a wave is a classical state in which there are many particles.

One of the physicists defending the analogy with classical diffraction made this argument. At the moment when the photon just flew up to the object, the wave function of the photon can be represented as $\psi = \psi' + \psi''$ where ψ' is the part of the wave function inside the object, and ψ'' is its part outside the object. Let be Ψ the wave function of the object. Then the initial wave function of the object + photon system is equal to $\Psi\psi = \Psi\psi' + \Psi\psi''$. The result of the interaction of a photon with an object is described by the action of the S-matrix on the wave function of the initial state: $S\Psi\psi = S\Psi\psi' + S\Psi\psi''$. Since part ψ'' of the wave function does not interact with the object, then $S\Psi\psi'' = \Psi\psi''$. On the other hand, part ψ' of the photon's wave function will be necessarily absorbed by the object, and as a result, the object will go into some excited state Ψ_1. So, after the wave function of the photon passes through the object, the wave function of the object + photon system will be $\Psi_1 + \Psi\psi''$. Therefore, if the probability that the photon is not absorbed by the object is triggered, then the wave function of the photon after passing through the object will be ψ'', and there can be no situation when a photon will be registered in the geometric shadow of an object.

These arguments are interesting, but, as will be noted below, I think they are wrong. I proposed to this physicist to write a joint paper in which he would defend these arguments, and I would give counterarguments, but he refused and asked not to be named. Then I asked his permission to present these arguments in my paper, indicating that the arguments were proposed by him. He agreed, but asked that his name not be used.

I think that these arguments cannot be correct for the following reasons. First, since in quantum theory the coordinates necessarily have some kind of uncertainty, the decomposition $\psi = \psi' + \psi''$ cannot be uniquely determined. But suppose that it can be determined in some approximation. Then the result $\Psi_1 + \Psi\psi''$ shows that the photon always interacts with the object because there is no situation when the wave function of a photon after passing through the object would remain the same as before passing through the object. But only the ψ' part of the photon wave function interacts with the object, and the ψ'' part does not interact at all.

Let $\rho' = ||\psi'||^2$ be the norm of the ψ' state squared, and $\rho'' = ||\psi''||^2$ be the norm of the ψ'' state squared. In the situation we are considering, $\rho' \ll \rho''$, and if the photon wave function is normalized to one, then $\rho' + \rho'' = 1$. The critical moment in this problem is this: if $\rho' \neq 0$, then this does not mean that ρ' part of the photon is inside the object. As noted above, an elementary particle has no parts. Unlike the classical case, ρ' does not mean that the ρ' part of the photon is inside the object, but only that the probability ρ' to find the photon as a whole inside the object is $\neq 0$. Since the probability that the photon interacts with the object $\leq \rho'$, this probability is very small, and with the probability $\geq (1 - \rho') = \rho''$ the photon does not interact with the object at all. Therefore, the final state of the object+photon system can be written not as above, but as $\Psi_1 + c_1\Psi\psi$, where $|c_1|^2$ is the probability that the photon does not interact with the object. Since this probability is very close to unity, then most likely, after passing through the object, the photon will have the same wave function as before approaching the object. Therefore, the problem that a photon can be registered behind an object remains.

Such a conclusion is also obvious from quantum electrodynamics. Here, the interaction of a photon with particles is described by Feynman diagrams, the vertices of which contain only one photon - incoming or outgoing. In this language, any process of photon interaction is described so that when an incoming photon is absorbed by a particle, then there is no photon at all in the intermediate virtual state; it can either be completely absorbed or reborn from the intermediate state anew as a whole. There cannot be a situation where a part ψ' of the photon wave function interacts, but the ψ'' part does not interact.

So, for one photon, the analogy with classical diffraction does not take place because the photon is an elementary particle and has no parts. As a consequence, if we accept that the coordinate and wave functions are related by the Fourier transform, then the paradoxes described above cannot be avoided. But, if we build the coordinate operator as suggested in my work [18], then there is no spreading, the photon wave function does not have cosmic dimensions, the photon moves almost like a point particle along a classical trajectory and there are no paradoxes.

It is often stated in the literature that the interference of an elementary particle on slits (for example, the so-called double slit experiment) is a strong confirmation of quantum theory, and even Feynman's words are quoted that almost all of quantum mechanics can be understood by the example of a double slit experiment. But here everything is not so simple. To explain the experiment, words about particle-wave duality are often spoken. This term, like the wave function, is also an example of the fact that when creating quantum theory, they tried to explain quantum phenomena in the classical language and, although many years have passed since then, the term is still used. It is said that Young's experiment confirms the wave nature of light, and the photoelectric effect corpuscular. It is also said that the double slit experiment is explained by the diffraction of photons. But, as discussed in detail above, for a single photon there is no analogy with classical diffraction. From the point of view of quantum theory, a photon is a particle whose state vector has a probabilistic interpretation. In contrast to the paradox with light from stars discussed above, in the double slit experiment there is no situation where the width of the photon wave function is much greater than the size of the object. Therefore, here the interaction of a photon with an object is not a small effect, and a simple qualitative explanation does not work.

Naturally, I wrote a paper about the starlight paradox and wanted to publish it. But journals were usually rejected outright under various meaningless pretexts. One of my attempts was Annals of Physics, and one of the motives was that Wilczek was no longer the editor of this journal and I hoped that the paper would be considered on its merits. This journal had the option to send the paper to one of the editors. I looked at the list of editors and saw that one of them was Victor Gurarie, who graduated from the Moscow Institute of Physics and Technology and defended his PhD under the guidance of Polyakov. I had hope that at least the first fact is positive. I had the impression that the spirit of the Moscow Institute of Physics and Technology is such that scientific work should be evaluated only by scientific criteria. My stories with Polyakov and Volovik showed that, at least for some physicists, such a principle is inapplicable, but still there was hope. However, Gurarie didn't even dignify me with an answer. So I sent the paper in the usual way. I was informed that the paper was under review and I had been waiting for it for three months. And the answer came that surprised even me. The answer is:

Ms. AOP 71457

Title: Fundamental Quantal Paradox And Its Resolution

Corresponding Author: Dr. Felix M. Lev

I regret to inform you that the reviewer of your manuscript, referenced above, advised against publication, and we must therefore reject it. The reviewer's comments are included below.

Thank you for giving us the opportunity of considering your work.

Yours sincerely,

Shahid

Journal Manager

for the Editors, Annals of Physics

Reviewer's comments:

So, it says that the paper was rejected due to the Reviewer's comments, but there are no comments. Usually, when editors reject a paper, they pretend that everything was within the bounds of decency, but here outright rudeness is not even camouflaged, but in fact it simply says that you should went away and we don't even want to discuss it.

I answered like this:

Dear Shahid,

In your email you refer to Reviewer's comments, but no comments are given. So, after three months the paper was rejected without any explanation.

Please confirm whether this is the case.

Sincerely, Felix Lev.

This answer was in a small hope that decent people should somehow explain (and even apologize). But received no answer. Therefore, I decided to write to the editor-in-chief Brian

Greene. He is considered a known string theorist and is widely known in scientific and pseudo-scientific circles. He works at Columbia University and has been the chairman of the World Science Festival since its inception in 2008. He gives many popular lectures on string theory, gravitational waves, the century of general relativity, parallel universes, extraterrestrial civilizations, etc. One of his books is called "The Elegant Universe" and everywhere he speaks of the greatness of science. It would seem that such a person should adhere to strict scientific principles and the rudeness that the journal in which he is the editor-in-chief demonstrated (if we think that he did not participate in this) should be unacceptable for him. So I wrote him a letter. In it I described the situation and at the end I wrote:

... So, during the consideration of my paper (if any consideration took place) all my inquiries were ignored and in the letter of March 16th Mr. Shahid simply lied about reviewer's comments. In response to my letter, a decent man should apologize for the erroneous info. However, my letter was ignored again and so he lied demonstratively without bothering himself with moral problems.

In summary, my paper was in the AOP office for three months and was rejected without any explanation. In addition, the editorial policy of AOP does not reserve for authors a right to appeal the editorial decision. I believe that such an attitude to the author is fully loutish and contradicts all the norms of scientific ethics. As far as physics is concerned, I don't know whether any physicist looked at my paper. Probably he did and instructed Mr. Shahid to respond in such a way. So this physicist demonstrated the absence of ethical norms because if he has no arguments then he should not have any judgement. In addition, for me it is obvious that this physicist does not understand very basics of quantum theory because I describe a problem in such a way that it should be understandable for any quantum physicist. I have no doubt that my paper is fundamental for understanding quantum theory and fully satisfies all the requirements described in the AOP editorial policy. I was not told that the paper was not in the scope of AOP. So, I believe that I have a right to request a reviewer report.

Thank you. Sincerely, Felix Lev

But this letter was also ignored, and therefore it seems obvious to me that in fact the lofty attitude to science that Brian Greene promotes is just words.

The story with this problem again ended happily when I sent a paper to the Dubna journal. And again here, in contrast to the so-called Western prestigious journals, the review was based on scientific criteria only. At first, the reviewer made some comments, and in the revised version of the paper I answered them. Then the reviewer wrote that, apparently, he thought about this problem less than I did, but still, he does not think that the paradox takes place, although he cannot rigorously refute my statements. He again made some remarks and wrote that, despite his opinion, he did not mind if the paper was published in the form in which it is. For many Western reviewers, this level of scientific integrity is unattainable. Despite this opinion, I decided to revise the paper again, and then it was accepted without comment and published in [19].

Chapter 7

On the problem of time and attempts to publish works on this problem

The problem of time in quantum theory is as follows. According to the postulates of quantum theory, each physical quantity corresponds to an operator. At an early stage of quantum theory, it was believed that the time operator existed, and even now some people accepts it, despite Pauli's assertion that there can be no time operator for several reasons. Although, as I wrote above, there are problems with the coordinate operator, you can still enter it in some approximations. And the time operator does not make sense in any approximations. So here, in contrast to the theory of relativity, there is no situation that space and time are just different parts of the four-dimensional Minkowski space: in quantum mechanics, time is usually treated simply as some kind of external parameter t and the evolution of the system is determined by the operator $exp(-iHt)$, where H is the energy operator. Some authors write that not only in quantum theory, but even in classical theory, time is not a primary concept, and a fundamental theory can be built without time at all. There are many works on this problem.

I propose the hypothesis that the real parameter of evolution is not the classical time t, but the parameter p in finite quantum theory, and we perceive classical time because p changes, i.e., not p does not change with time, but classical time is a consequence of p changing.

If some hypothesis is proposed, it does not mean that there is no science in it. There are well-known hypotheses that have played a big role in science, such as Fermat's hypothesis, Riemann's hypothesis and others. The scientific value of a hypothesis is determined by what arguments there are in favor of this hypothesis and, of course, there should be no evidence that the hypothesis is wrong. In my case, the argument is that there are scenarios where the classical equations of motion for cosmological repulsion and gravity are a consequence of this hypothesis. It is difficult to prove the hypothesis because it is not yet clear which wave functions describe the particles in order for such a scenario to work. But the hypothesis is completely new and there is no reason to immediately say that the hypothesis is unrealistic. The hypothesis is described in detail in [20], but before that I tried to publish it in various journals.

The first natural thought is that the work is entirely in the spirit of the editorial policy of the Foundations of Physics (see above). But since my experience with this journal was very negative (see above), then I hesitated. In the end, I decided to send it there for such reasons. The first and foremost was that the editor-in-chief was replaced in the journal - now it is no longer 't Hooft, but Rovelli. In his papers and even his book, he discussed the idea that time is not a fundamental concept. In addition, he says and writes that the fundamental quantum theory must be discrete, i.e., speaks good words. Of course, the question arises whether there is something behind these words that is not only good wishes.

In the scientific world, Rovelli is known as one of the authors of quantum loop gravity (LQG). One of the main goals of this theory was to show that GR is the classical limit of LQG. If this could be shown, then this would, of course, be a great achievement, but, despite many years of attempts by many physicists, it has not yet been proved. At one time, there were big disputes between string theorists on the one hand and LQG on the other. Supporters of LQG said that since string theory begins with a flat space-time, one of the main principles of general relativity, the principle of background independence, is violated. This principle says that the theory should not be based on the preference of some space-time background and should be invariant when moving from one space-time background (generally speaking, curved) to another.

In Sec. 2.6, I wrote that, at the quantum level, the very concept of background space-time is meaningless, so, in my opinion, this argument is also meaningless. But any mainstream quantum theory comes from some kind of space-time background. In LQG, space-time background is the fundamental concept on which everything is based; it is discrete, but we would like it to pass into some space of GR in the classical limit. And some LQG supporters said that the string players grabbed almost everything (which was close to the truth) and did not follow Einstein's precepts about background independence.

True, it should be noted that LQG supporters were also not deprived of everything. For example, they dominated the Perimeter Institute of Theoretical Physics (PI). For example, when I was foolish to enter the FQXI essay contests, various people noted that those who were supported by PI mostly won there. However, some physicists thought the LQG proponents were poor because the string mafia clamps them down.

And, in particular, they said about Rovelli that he is open minded and admits that different approaches have the right to exist. To be honest, I tried to understand what he writes about LQG, but when they immediately start with the axioms about spin networks, I immediately turn off. Nevertheless, I thought that what if he was decent and decided to send him a paper. But everything turned out to be almost like with ‘t Hooft', and what is the meaning of "almost" I will describe below.

In the beginning it was like 't Hooft'. According to the rules of the journal, if the paper is suitable for the journal, then the review must be given within three months. As with 't Hooft', the paper was not immediately rejected, and I waited almost three months for a review. And, as with 't Hooft', the review was pointless and completely against scientific ethics. It was:

COMMENTS TO THE AUTHOR:

Reviewer 1: In the manuscript, the authors review some ideas exposed previously. The main approach consists in assuming that classical mathematics (involving such notions as infinitely small/large, continuity etc.) is a degenerate special case of finite one, and ultimate quantum theory will be based on finite mathematics.

The main results of the paper are described in Sec. 8. Here, it is shown that there exist scenarios when classical equations of motion can be obtained from quantum theory without using any classical notions such as coordinates, time, position operator, standard semiclassical approximation etc.

While the goal seems interesting, the manuscript is very hard to read and its goals and consistency are not clear. Some affirmations are polemical or even wrong.

Given the previous authors contributions, and given the poor quality of the manuscript, I don't recommend publication.

The first two paragraphs simply repeat the words from my paper. The reviewer does not express his attitude to them at all, for example, to the fact that I consider finite mathematics to be fundamental and to the fact that the classical equations of motion may not be related to standard classical concepts. In the third paragraph, he writes that although the goals seem interesting, the paper is very difficult to read and the goals and self-consistency are unclear. This proposal is already controversial because if the goals seem interesting, then why are they unclear. And it would seem that if the paper is difficult to read, then tell me exactly where it is badly written, but it is not clear whether it is badly written, or it is difficult for him to read because he is stupid. For example, if a paper on higher mathematics is given to a first-grader to read, then he, too, can say that it is difficult for him to read. And the phrase that some statements are polemical or even incorrect again completely contradicts scientific ethics because nothing is specifically said about what is polemical and what is wrong. Well, in conclusion, it is said that the quality of the paper is poor and again without any explanation.

I can't understand the psychology of reviewers who write such reviews. First, there is no hint in the review that he even tried to understand something. Does he understand that by writing a negative review of a paper in which he understands nothing, he is acting dishonestly? After all, if he cannot or does not want to understand, then why agree to be a reviewer? Or does he find some excuse for himself? For example, such that he sees some words in a paper, decides that this is not science, and then it is not necessary to follow the rules of scientific ethics. Or does he consider himself such a great scientist that if something seems to him, then it is not necessary to substantiate it? Or, if he believes that for some lofty reason he should write a negative review without any attempt to understand, then why drag out three months? And finally, how can a decent editor accept reviews that are not only completely contrary to scientific ethics, but also completely contrary to the editorial policy of the journal where he is the editor?

It is clear that I again wrote an appeal, but this time something happened that was not under 't Hooft': this appeal was answered. Rovelli himself wrote that he took my appeal seriously and sent a new review:

This paper extends a line of ideas that the author has been developing. The author uses the representations of the DeSitter algebra to describe the quantum physics of some systems in terms of finite-dimensional state spaces. The novel idea is to identify time with a cut-off parameter that determines physical finiteness. The paper contains a long initial discussion part where the author discusses and criticises a number of common assumptions of physical theories. The discussion is not very clear, it is confusing, rambling, and lacks sharpness. It fails to say clearly what are the hypotheses of the model and what exactly the model proposed is supposed to describe. As far as this referee understand, the model does not include the field-theoretical description of the gravitational degrees of freedom, and therefore its relevance for fundamental physics is unclear. The idea that physics is finite is an old idea, but the model proposed does not seem sufficient to tie it to the current understanding of the world. The arguments supporting the claim that the cut-off parameter that determines physical finiteness can be identified with time are weak, hidden in the technicalities and this referee finds them unconvincing. As a whole, the paper lacks the clarity needed for a publication on Foundations of Physics. The author requests that the paper be judged solely on the consistency of the math and not the form and the content of the ideas, but a journal like Foundation of Physics cannot publish anything only because it is mathematically consistent, because a paper can be mathematically correct but contains ideas that are too weak to be relevant for a discussion about the Foundations of Physics. Therefore an evaluation of a paper cannot avoid an evaluation of the relevance of the ideas.

For various reasons, I think that Rovelli himself wrote this review. It pretends that there was an attempt to understand and, supposedly, to give a fair review. But basically, everything is the same as before.

Initially, he writes that it is a new idea to associate time with p, i.e., this he admits. But since he rejects the paper, then he needs to explain why the paper is bad.

At the beginning, he says that the paper contains a long initial discussion where commonly accepted assumptions are discussed and criticized. It would seem that there is no sedition in this for a paper in Foundations of Physics, especially since the journal itself writes in its editorial policy that it welcomes new approaches. But he goes on to write that the discussion is vague, incoherent, and lacks poignancy. And again, without any explanation, what is unclear, incoherent, etc. He says that there is no clear statement of the problem, and it is not clear what the approach should describe, and again without any explanation. This can be said about any paper.

He writes that, as he understands, the model does not contain field-theoretic gravitational degrees of freedom and, therefore, the connection with fundamental physics is unclear. I don't know if he understood that gravity is a very important element of the paper, but not within the QFT dogma (for him, gravity is only the field-theoretical description of the gravitational degrees of freedom), and therefore, according to his ideas, nothing fundamental. This is completely contrary to the words in the editorial policy that different approaches are welcome. But even if there is no gravity: the paper is not devoted to gravity, and it is not in the vast majority of papers in this journal. Therefore, the argument that there is no gravity is given only to say something else negative.

Further, he writes that the idea of finiteness is old, but the model does not connect it with reality, the arguments are weak, hidden in technique, they do not convince the reviewer, and in general there is no clarity. And again, no examples and no explanations. And the fact that he treats p as "cut-off parameter that determines physical finiteness" shows that he doesn't understand finite mathematics at all. To say that p is a cutoff parameter is about the same as saying that the theory of relativity is classical mechanics, but the velocities are cut off by the parameter c.

In conclusion, he writes that I supposedly asked that the paper be judged only on the basis of whether the mathematics is correct or not, and for Foundations of Physics, the correctness of mathematics is still insufficient. But this is a lie, and his phrase arose from this. In my cover letter, I described that three of my papers in Foundations of Physics were rejected and at the same time I wrote three negative reviews for the journal. But my reviews were negative because I explicitly pointed out where the papers were mathematically incorrect, the conclusions were based on incorrect mathematics, and I cannot write a negative review just because I do not share the author's philosophy.

In general, the review is meaningless. A lot of words are said to give the impression that the paper was judged from the point of view of high science, but there is nothing concrete and no hint that there was any attempt to figure it out. As I noted above, I think that Rovelli himself wrote the review, but even if not him, he obviously read it and saw that nothing concrete, contrary to scientific ethics, negative statements are made without justification and the review is contrary to the editorial policy.

I wrote that his phrases about discreteness sound good, but it is not clear whether behind these phrases there is something other than good wishes. In LQG space-time is discrete, it is implemented in the form of spin network, and the desire is to obtain space-time from GR in the classical limit. As I noted above, the very concept of space-time in quantum theory has no meaning, and in chapter 5 I noted that from the fact that the results of general relativity are obtained in the semiclassical limit, it does not at all follow that in quantum theory there should be a background space. And after he rejected my paper, his paper [21] appeared in arXiv, in which he talks about finiteness and discreteness. Here's an excerpt from that paper:

Now, the volume $Vol(R)$ of a region R of phase space has dimensions $Length^2 \cdot Mass/Time$ for each degree of freedom. This combination of dimensions, $Length^2 \cdot Mass/Time$, is called 'action' and is the dimension of the Planck constant. Therefore what the Planck constant fixes is the size of a (tiny) region in the space of the possible values that the variables of any system can take.

Now: the major physical characterisation of quantum theory is that the volume of the region R where the system happens to be cannot be smaller that $2\pi\hbar$:

$Vol(R) \geq 2\pi\hbar$ *(2)*

per each degree of freedom. This is the most general and most important physical fact at the core of quantum theory. This implies that the number of possible values that any variable distinguishing points within the region R of phase space and which can be determined without altering the fact that the system is in the region R itself, is at most

$N <= Vol(R)/2\pi\hbar$ *(3)*

which is a finite number. That is, this variable can take discrete values only. If it wasn't so, the value of the variable could distinguish arbitrary small regions of phase space, contradicting (2). In particular: any variable separating finite regions of phase space is necessarily discrete.

In this excerpt, the words "discrete" and "finite" are pronounced. First, he says that the phase space element has the same dimension as $\hbar$. This is known, although, as I wrote above, it is rather strange to express $\hbar$ in terms of classical dimensions. And then he says that the number of states satisfies condition (3). In quantum mechanics, there is a known (and described in textbooks) Bohr-Sommerfeld rule that when a semiclassical particle makes a finite motion, then the number of states N is finite and is given by the same formula (3), but instead of N one should write $N + 1/2$, instead of $\leq$ just write $=$ and, although N is large, $1/2$ still needs to be taken into account. So, in the semiclassical case, what he writes is simply the Bohr-Sommerfeld rule, but he does not mention it, and therefore it may be a mystery to the reader where this rule comes from. Apparently, he claims to be something more because he does not say that he considers only the semiclassical approximation. But the phase volume makes sense only in the semiclassical approximation, and it says nothing about how formula (3) is derived.

But in any case, this rule is obtained in standard quantum mechanics, where coordinates and momenta are continuous. In quantum mechanics, a discrete spectrum often arises (hence the name "quantum") because, as is known, some operators in Hilbert space (for example, angular momentum or energy) can have such a spectrum. But this does not mean that the theory itself is discrete since it is based on standard mathematics. So, at least in this case, the words about discreteness do not make much sense, and again it is not clear whether there is something more fundamental in his words about discreteness.

So, it became clear to me that even though 't Hooft was gone, the journal remained essentially the same. Most likely, this was intended. 't Hooft and Rovelli seem to be friends and even wrote joint papers (on the philosophy of quantum theory). So, I started thinking about which other journal to send. Decided to first try in Phys. Rev. A. I know well that all Phys. Rev. journals fight to the death so as not to allow something that is not in the mainstream, but there is one positive thing that I wrote about: if reviewers reject, then you can write an appeal, which a member of the editorial board is obliged to consider, and he is obliged to report his last name. And even if he rejects it, then formally you can still write an appeal to the Editor in Chief of the American Physical Society. In the case of my paper in Phys. Rev. D. in 2012 it worked because, although there were four negative reviews in total, but Misha Shifman, a member of the editorial board, said that it was possible to publish.

In editorial policy of Phys. Rev. A. it is written that the journal has a section "Fundamental Concepts", one of the possible topics is "foundations of quantum mechanics" and one of the possible sections of physics is "Quantum Theory". So my paper fully complies with their rules and therefore, it would seem, they should consider the paper on its merits. But, as usual, the first review (if you can call it that) was negative with the following text:

We have examined your manuscript. We conclude that the manuscript is not suited for publication in The Physical Review.

We make no judgment on the correctness or technical aspects of your work. However, from our understanding of the paper's physics results, context, and motivation, we conclude that your paper does not have the importance and broad interest needed for publication in our journals, as it seems too speculative.

This judgment results in part from our reading of the abstract, introduction, and conclusions, which are crucial for our readership. In view of our assessment, we are not sending your manuscript out for review. We regret that we must suggest that you submit the manuscript to a more appropriate journal.

Yours sincerely,
Marek Zukowski
Associate Editor

Physical Review A

This is the standard text prepared for those cases when they do not want to understand the paper and want to kick it off right away. Therefore, everything that is written has nothing to do with my paper. It is clear that this text is completely contrary to scientific ethics because negative statements are made without any explanation. It is difficult for me to understand how a decent physicist can subscribe to such a text. In this case, Marek Zukowski who signed up. For example, he writes papers on Bell's theorem (I won't write here what I think about this theorem). From this text it is completely unclear whether he understands the problem of time in quantum theory, whether he has at least an approximate idea of the very foundations of finite mathematics, or like many physicists, his way of thinking is such that if he does not understand something, he immediately decides that it has nothing to do with physics.

The question arises why one of the leading physics journals in the world does not hesitate to send authors texts where the most basic rules of scientific ethics are violated. In editorial policy they write: "If in the judgment of the editors a paper is clearly unsuitable for Physical Review A, it will be rejected without external review; authors of such papers have the same right to appeal as do other authors". So, they considered my paper "clearly unsuitable". But in this case, they must also explain why. This text seems to pretend to be an explanation: they say that they (supposedly) read the annotation, introduction and conclusion. If so, why not say what is unacceptable in them? But there are no hints in the text showing that at least some part of the paper was read.

You can say this: if they deal with each paper, then they simply will not have enough time for this because there are a lot of papers. But then why bullshit readers that, as they write: "By Scientists, For Scientists". Like all of the journals in the Physical Review family, PRA is shaped by researchers to serve the research community. This commitment ensures that its mission and standards prioritize the needs of researchers and authors... ". And anyway, why then write such a long editorial policy and pretend that they decide within the framework of some rules? Then you just need to write that the editors decide, they are not obliged to explain, and that's it.

It is clear that I wrote an appeal, and they sent me a reply, which was written by a member of the editorial board, S. Pascazio. This answer is a typical example of trying to write some scientific words to pretend that the answer is reasonable. I am giving this answer in full.

The manuscript written by Dr. Felix M. Lev deals with an approach to quantum mechanics and quantum field theory based on a finite field or ring. Focus is on the role of time (and space), space-time symmetries (from Poincare to dS and AdS) and IRs thereof, the semiclassical approximation and (some) classical results, obtained without making explicit reference to the standard semiclassical limit. Let me add that I have some basic knowledge in finite mathematics (enough knowledge to appreciate the content of Sec. 5). I wonder whether Dr. Lev is correct when he asserts that this knowledge is not shared by the majority of physicists. He is certainly a bit superficial when he writes that "referees in physics community [very often] do not have even very basic knowledge in the problem discussed in the refereed paper." This is certainly not true for Physical Review A.

But let me come to the point. I quote from the APS webpage: Physical Review A publishes important developments in the rapidly evolving areas of atomic, molecular, and optical (AMO) physics, quantum information, and related fundamental concepts. (I wrote "related" in italics.) There is one section on "Fundamental concepts" that covers what used to be considered (in the period that followed the discovery of Bell's theorem) foundational issues in the interpretations of quantum mechanics. Such an area of research has recently evolved into a very active field of investigation, dealing with entanglement, other quantum correlations, dissipative quantum systems and evolutions, quantum maps, quantum applications and also quantum technologies (this list is far from being exhaustive). In his accompanying letters, Dr. Lev wrote that "Your journal has a section "Fundamental Concepts" and my paper satisfies all the requirements for that section." In my opinion there is a misunderstanding. This expression has a different connotation. I quote again the webpage of Physical Review A: PRA covers atomic, molecular, and optical physics, foundations of quantum mechanics, and quantum information, including:

Fundamental concepts
Quantum information
Atomic and molecular structure and dynamics
Atomic and molecular collisions and interactions
Atomic and molecular processes in external fields, including interactions with strong fields and short pulses
Matter waves and collective properties of cold atoms and molecules
Quantum optics, physics of lasers, nonlinear optics, and classical optics

The ideas investigated in this manuscript are of interest, but do not appear to be in line with the scope of Physical Review A, for the reasons I have detailed. I believe that Physical Review A is not the right arena to discuss the issues brought up by Dr. Lev. I therefore suggest that this manuscript be submitted to a different journal, where appropriate refereeing can be provided.

In conclusion, I uphold the rejection of the Associate Editor Marek Zukowski and do not recommend publication in Physical Review A.

It took him 40 days to create this epistolary work. The answer is quite long, but it only has two topics.

First, he says that I am wrong, stating that most physicists do not know the basics of finite mathematics, and this is obviously wrong for PRA reviewers. In particular, as he writes, "I have some basic knowledge in finite mathematics (enough knowledge to appreciate the content of Sec. 5)". If so, great, and why not show that he really knows something? Why, in this case, does he not write a review, especially since (like many reviewers who reject my papers) he writes that "The ideas investigated in this manuscript are of interest"? But a further long part of the answer is devoted to arguing that the article is off topic on PRA.

It would seem that the paper is on the topic is quite obvious: as I wrote, the journal has a section "Fundamental Concepts", one of the possible topics is "foundations of quantum mechanics" and one of the possible sections of physics is "Quantum Theory". But in his long arguments, he wants to show that the paper is still off topic. He quotes a lot from editorial policy, but about "Fundamental Concepts" and "foundations of quantum mechanics" he says, after Bell's theorem, this meant "entanglement, other quantum correlations, dissipative quantum systems and evolutions, quantum maps, quantum applications and also quantum technologies (this list is far from being exhaustive)". So, not all questions in "foundations of quantum mechanics", but only those that are on his list, which is only in his head, and, as he writes, not complete. Therefore, the question arises: when a physicist sends a paper on "foundations of quantum mechanics", how does he know what is included and what is not? In particular, it follows from Pascazio's logic that the problem of time does not enter here. Obviously, the purpose of the answer is to find a reason to kick the paper without any review. Does he understand that he is acting dishonestly? He probably understands, but he decided that for some reason it was better for him to do so.

After this answer, I had one last chance to challenge the editorial decision: turn to the Editor in Chief of the APS. The editorial policy states that all scientific issues are the responsibility of the editorial board, the Editor in Chief does not deal with scientific issues, and when addressing him, the main question should be: "did the paper receive a fair hearing?". In my appeal, I wrote that, obviously, the consideration was not fair: although it is obvious that my paper is on the topic of the journal, but for two months they came up with reasons to kick the paper without any review. Obviously, this is exactly the question that the Editor in Chief must decide.

In his response, Editor in Chief Michael Thoennessen writes: "The Editor in Chief must assure that the procedure of our journals have been followed responsibly and fairly in arriving at that decision... The original decision by the editor was subsequently supported by Editorial Board Member Professor Pascazio who wrote a detailed report. Thus I conclude that your paper received a fair review".

So, his logic is this: if the editor refused and then a member of the editorial board confirmed this, then everything is fair. But then the question arises: why then The Editor in Chief is needed? Why is it written in the editorial policy that you can contact him, although he still does not want to decide anything and trusts the editors? Does he understand that by such an answer he shows that he does not want to fulfill his duties and is acting dishonorably?

My next attempt at publishing was to submit it to the Journal of Physics Communications. This journal has just started, it is owned by the Institute of Physics, and the editorial policy is breathtaking. In particular, it says that "The journal does not make a subjective assessment on the potential future significance of a paper, instead providing a rapid platform for communicating research that meets high standards of scientific rigor and contributes to the development of knowledge in physics. All physics-related research is in scope, including interdisciplinary and multidisciplinary studies. All types of results can be published, provided they contribute to advancing knowledge in their field, including negative results, null results and replication studies." It was also written that "All articles are subjected to rigorous single-blind peer review by at least two referees" and that "the author has the right to appeal against a rejection". But I did not receive any two reviews, but received a response with the following text:

BOARD MEMBER'S REPORT:

I think the paper is not suitable for JPCO. In my view the paper would be more suitable for a philosophy of science journal than a physics journal. Of course it is OK to propose new points of view and to challenge accepted knowledge, however the proposal that quantum theory should be based on discrete mathematics seems rather speculative to me. I do not think that this paper will have the right level of scientific rigour and/or will be interested in the mathematical physics community.

I wrote an appeal, and then an addition to it. I wrote in them that the decision of the editorial board completely contradicts the official policy of the editorial office itself. Firstly, I did not receive two reviews, the arguments of this member of the editorial board are given without any explanation, his opinion is simply a "subjective assessment", which they swear they do not accept, and the phrase "the proposal that quantum theory should be based on discrete mathematics seems rather speculative to me" is simply ridiculous. "Quantum" has the meaning "discrete", and therefore this sentence can be rephrased as: "the proposal that discrete theory should be based on discrete mathematics seems rather speculative to me".

So, this member of the editorial board does not understand the foundations of quantum theory and scientific ethics, that no negative statements should be made without justification. When the journal was organized and advertised, they wrote that because editorial policy is non-standard, they explain it to reviewers. But here it turns out that even a member of the editorial board is acting in complete contradiction to this policy.

They wrote to me that the appeals were sent to the Editorial Office and "wait for a response." I waited a long time, and then wrote to them that they completely violated their own rules, and I am retracting the article. In response, I received a letter, in which, in particular, it is written:

We are sorry to hear that you are so unhappy with the review process. We do understand your frustration, particularly as it does currently give the impression on our homepage that all articles will receive two referee reports. I thank you for bringing this to our attention. This information is unfortunately wrong, and I have requested as a matter of urgency that it is changed to be a true reflection of our peer review policy: "This journal operates a single-blind peer review policy. All submissions are preliminarily assessed for editorial suitability by an in-house editorial team, and in some cases also by the Editorial Board, ahead of formal peer review. Articles that are considered inappropriate for the journal at this initial stage will be rejected without further review.... Your article received a preliminary report from the editorial board, and was rejected based on their recommendation. Your appeal had been sent back to the editorial board for further consideration, but we have not yet heard back from them. We have therefore withdrawn your article from consideration as you requested, and you are free to submit it elsewhere".

What's unusual about this response is that they now say that the editorial policy clause that one gets two reviews is incorrect and thank me for pointing that clause out. Now they removed it and wrote that at first the editorial board decides whether the paper on the topic or not (that is, in this case, almost all the unusualness of their policy is lost). And that my paper was rejected based on the report of a member of the editorial board. But they do not write whether this decision should somehow be justified. And they also write that my appeal was sent to the editorial board, but so far there is no decision. Because a lot of time has passed, it seems that the appeal was not going to be considered, again in complete contradiction to the editorial rules.

In all three cases, when I sent a paper about the problem of time, I asked in a cover letter to consider the paper in accordance with the editorial rules, because my experience shows that, for various reasons, editorial offices do not comply with the requirements of their own policies. It would seem even strange to ask the editors about this, because this should be taken for granted. But all these requests were ignored and, in all cases, the editorial rules were violated.

Finally, I was surprised by this. As noted above, in contrast to my experience with the so-called prestigious Western journals, the reviews that I received from the Dubna journal "Physics of elementary particles and the atomic nucleus" and letters to this journal were at a high level and the purpose of the reviews was to help the author improve the paper. Therefore, I decided to send a paper about the problem of time to this journal. And I was very surprised by the review written in Russian. I give my detailed answer (also originally written in Russian) because it also cites the review:

Paper: "A Conjecture on the Nature of Time". Author: F. Lev.

Author's response to review

I am indebted to the referee for setting out his objections in detail. My approach can be treated differently, but I hope that the reviewer at least recognizes that the approach is non-standard. I understand that because of this, readers may have problems with understanding. I tried to explain my approach in detail, but at least I failed to convince the reviewer. I fully admit that the explanation may not always be clear. Therefore, I am always grateful for any criticism (including incorrect ones) because it helps to understand where the explanation needs to be improved. In connection with the comments of the reviewer, the paper has been significantly revised. Below I will try to answer in detail all the objections of the reviewer.

1. Why finite mathematics is more fundamental than continuous. The reviewer writes: "The division of mathematics into fundamental (finite) and non-fundamental (continuous) is completely arbitrary. First of all, the author does not define fundamental and non-fundamental theories, but considers at the formal level the possibility of transition from finite mathematics to continuous one. It is obvious that with the same success on the same level of rigor one can consider transition from continuous mathematics to finite one."

I tried to explain this question in detail in the paper [15], which the reviewer read. But, since he considers the explanation unconvincing, I will try to explain this point in detail so that it is completely clear. First, I will give three examples known from physics.

Consider the special theory of relativity (STR) and classical mechanics. The phrase that the first theory is more fundamental than the second can be understood as follows. STR has a finite parameter c, and classical mechanics can be considered as the formal limit of STR at $c \to \infty$ because any effect of classical mechanics can be obtained from STR in such a formal limit. But when we have already passed to the limit and obtained classical mechanics, we can no longer go back to SRT, and classical mechanics cannot describe all the effects of SRT. It can describe with good accuracy only phenomena where $v/c \ll 1$.

Consider a de Sitter (dS) invariant theory and a Poincare invariant theory, i.e., STR. The phrase that the first is more general than the second can be understood in such a way that the first has a finite parameter R (which can be called the radius of the world) and the second theory can be obtained from the first one in the formal $R \to \infty$ limit. Therefore, any effect of the second theory can be obtained from the first one in such a formal limit. But, when we have already passed to the limit and obtained STR, then we can no longer go back to the dS theory, and STR cannot describe all the effects of the dS theory.

In his famous paper “Missed Opportunities”, Dyson writes that SRT is better than classical mechanics, and dS theory is better than SRT not only from physical, but also from purely mathematical considerations. The Poincare group is more symmetrical than the Galileo group and the transition from the first to the second at $c \to \infty$ is also formally described by the contraction procedure. However, the de Sitter group is semisimple and therefore has the maximum possible symmetry. Therefore, this group cannot be obtained from any more symmetrical group by contraction.

Let’s compare quantum and classical theories. The first is understood as more general than the second, for example, because the second can be treated as a formal limit of the first when $\hbar \to 0$. Any effect of classical theory can be obtained from quantum theory in such a formal limit. But, when we have already passed to the limit, we can no longer go back to quantum theory.

In all the three considered examples, there is a common pattern. We consider two theories, so that the first contains some finite parameter, and the second is obtained from the first in the formal limit, when this parameter goes to zero or infinity. Then the first theory is treated as more fundamental than the second. When we have already passed to the limit, we can no longer go back to the first theory.

There is a term $c\hbar G$ cube of physical theories in the literature. The meaning is that the most general theory is the quantum relativistic theory of gravity, which contains all three parameters, less general theories contain fewer parameters, and the least general theory is classical, non-quantum, and without gravity contains neither c nor $\hbar$ nor G. In my papers, I write that, firstly, based on the above, it is better to talk about $c\hbar R$ cube, and not $c\hbar G$ cube. And secondly, from this terminology one might get the impression that, on the contrary, the classical non-quantum theory without gravity is the most general, because it contains no parameters at all. In fact, such a theory contains three parameters - (kg, m, s), and in the most general dS invariant quantum theory there are no parameters and dimensions at all. They arise only because we want to express $c\hbar R$ in terms of classical dimensions. These parameters are needed only for a formal transition from a more general theory to a less general one. This issue is discussed in my papers and in this paper too.

Finally, let us discuss the question of the connection between finite and standard mathematics. Standard mathematics starts with the natural series and, as follows from Gödel’s theorems, any theory containing the natural series automatically has undecidable problems with substantiation. The reviewer writes: ”The indistinct reference to Gödel’s theorems is irrelevant to this question, since these theorems also concern the natural series of numbers, which is used by the author as a starting building material ... However, in the reviewed article, speaking of Gödel’s theorems, the author simply threw out the mention of natural series of numbers...”.

Usually, when I have an ambiguity, I try not to immediately conclude that someone does not understand something and try to understand that maybe I do not understand something. But in this case I cannot find an explanation other than the reviewer does not understand. Finite mathematics cannot contain the natural series ”as the initial building material”, even because the natural series is infinite. The reviewer writes that, speaking of Gödel’s theorems on page 9, I deliberately left out the mention of the natural series. But I have only talked about theorems in connection with standard mathematics. In finite mathematics, there are no natural series and Gödel’s theorems. Finite mathematics begins not with natural numbers, but with a set of numbers $R_p = (0, 1, 2, \ldots p-1)$ which in the literature is called the ring of residues modulo p, and this is noted in the paper on page 8. In this set, addition, subtraction and multiplication are defined as usual, but modulo p. In number theory, the phrase that something is taken modulo p means that only the remainder of dividing that number by p is taken. For example, let’s take a set of numbers (0,1,2,3,4), i.e., $p = 5$. Then 3+1=4 as usual, but 3+2=0 and 3+3=1. This set is closed under these three operations since we will always get a number from this set. And if p is simple, then this collection becomes not only a ring, but also a field, since division can be defined. For example, 1/2=3, 1/4=4, etc. We can say that all this is exotic and (or) pathological, which has nothing to do with life, because 3+2 is always 5, not zero.

The answer to this objection is this. Assume, for simplicity, that p is odd. Because operations in our set are defined modulo p, then R_p can also be represented as $R_p = (-(p-1)/2, -(p-3)/2 \cdots -1, 0, 1, \cdots (p-3)/2, (p-1)/2)$. Then, if p is very large, then for numbers that are $\ll p$ in absolute value, addition, subtraction and multiplication will be the same as usual, i.e., in this case we don't notice p. The difference from the usual case will be only for numbers whose absolute value is comparable to p. One can raise an objection that all this is still unphysical because $1/2$ is equal to a large number $(p+1)/2$. But since states in quantum theory are projective, then this objection does not refute anything (as discussed in detail in my papers). Moreover, the question arises (see the new version of the paper) whether the usual division is a fundamental operation.

As noted in my works (for example, in [15], which the reviewer read), this set can be visually represented as points on a circle. This follows from the fact that if we take any element $a \in R_p$ and add 1 all the time, then in p steps we will exhaust all R_p, by analogy with the fact that when we move along a circle in one direction, then we will return to the starting point. At the same time, the ring of integers Z can be represented by integer points on an infinite line. As p increases, for more and more of our set, addition, subtraction, and multiplication become normal. This is analogous to the fact that when we are on a curved surface, we do not notice the curvature until the distances are $\ll$ of the radius of curvature. The formal limit $p \to \infty$ clearly means that we make Z from R_p, i.e., as if we break the circle and make a straight line out of it.

The historical analogy is clear here. For many years people thought the Earth was flat, but then they realized that it was round. While we are dealing with distances $\ll$ of the radius of curvature, we do not notice the curvature. Similarly, so far, the vast majority of people still think that a set of numbers is a straight line. This is because at present the number p is very large and when we are dealing with the numbers $\ll p$, we do not notice the "curvature".

When we break the circle, we lose symmetry because a circle is a more symmetrical figure than an infinite straight line. This follows from the fact that if we take $a \in Z$ and add 1 all the time, then we will not exhaust all Z. To do this, you need to add +1 and -1 to a, and an infinite number of times. And, when we broke the circle and got a set of integers, now with them we can induce a lot of science, introduce rational, real numbers, etc. As I explain in my works (see also the new version of the paper), rational and real numbers are artificial, and they are not needed for quantum theory. Nobody argues that the standard analysis technique is useful in many applications, but this technique is often a good approximation because p is very large. And just because standard mathematics often describes data well even in quantum theory, this does not mean that it will always be enough to apply standard mathematics. For example, classical mechanics describes a lot of data with high precision, but stops working when v/c is not small.

So, standard mathematics can be treated as a formal limit of finite one at $p \to \infty$. Finite mathematics is more general (or fundamental) because it can reproduce all the results of standard mathematics if p is large enough. And vice versa, in contrast to what the reviewer writes, when we have already passed to the $p \to \infty$ limit, we cannot go back. Standard mathematics cannot reproduce all the results of finite one because there are no operations modulo p in standard mathematics. The situation is completely analogous to that described above for the three cases: a less general theory is obtained from a more general one when some parameter, which is finite in a more general theory, formally goes to zero or infinity. I explain this in my works, including [15], which the referee read. But since this did not convince him, then in the new version I explain this in more detail, especially since ECHAYA is a review journal.

The referee writes that in [15] p is a constant such that finite mathematics deals with a finite number of objects p, while in the present paper "...p is a characteristic of a finite field or ring". Which characteristic is the author does not specify. In textbooks on finite mathematics, the number p is called the characteristic of a finite field or ring, so all operations are performed modulo p. This is what I wrote in my previous posts. In this paper, the meaning of p is explained, and at the end of section 2 it is said that in number theory p is a standard notation for the characteristic of a field or ring, so it is also explained in what sense p is characteristic. But still, in connection with the reviewer's remark, now in the new version of the paper, as soon as I enter p, I immediately write that it is called a characteristic.

So, both in my previous works and in this work, p is the same value. Referee's words that p is a constant, such that finite mathematics deals with a finite number of p objects is, in general, wrong. For example, the quadratic extension of the ring R_p is a finite analogue of the complex numbers, and here the number of objects equals p^2. And the logic of the referee is contradictory: in one place he says that I use the natural series (i.e., an infinite set) as "initial building material" and threw out the mention of the natural series, and in another that finite mathematics deals with a finite number of objects. From these considerations it follows that in any physical theory based on finite mathematics there is necessarily a finite parameter p, which is a characteristic of a finite field or ring. One could say that standard theory is better because it doesn't have that option. But this is analogous to the fact that in the examples of physical theories considered above, to say that the non-relativistic classical theory without gravity is the best because it doesn't have any parameters.

The question arises whether there are any considerations for the choice of p. The reviewer writes that in this work "During the evolution of the Universe, the parameter p must be given different values. According to what law this should be done, the author does not indicate. How this is consistent with the statement that p is a fundamental constant (see paragraph (a)), the author does not explain.

The question of the choice of p is very complicated and, since FQT is still far from a finished theory, it does not have an unambiguous answer to this question. When I started working on FQT, I assumed that this theory should be based on a finite field. In this case, p must necessarily be simple, and, for example, [6] discusses various options. But, after studying the works of M. Saniga and corresponding with him, now I think that this is not necessary and, since division is not a fundamental operation, then the theory over the ring is more attractive. In this case, p need not be simple.

In the initial works on FQT, I did not consider evolution, treated p as a fundamental parameter, but did not write that p is a constant that is the same at all stages of the evolution of the Universe. For example, in modern theories, the quantities $c\hbar G$ are considered fundamental constants, but there is no law that in the process of the evolution of the Universe they are always the same, especially if G changes over time. In this paper, I consider the hypothesis that what we perceive as classical time is a consequence of the fact that p is different at different stages of the evolution of the Universe.

So, I'm not saying that p changes over time since in this hypothesis, the very existence of time is a consequence of the fact that p changes. The main purpose of the work is to present arguments in favor of this hypothesis. I note that many well-known facts in science were originally formulated as hypotheses and some of them have not been proven so far, for example, the Riemann hypothesis about the zeta function. But there is a lot of controversy around this hypothesis, so it is wrong to say that Riemann should not publish his hypothesis.

If my hypothesis is correct and at each step of evolution what we perceive as time changes by the same amount each time, then, as follows from formula (77), the value of Δlnp each time changes by approximately the same size. But this issue requires further research. I hope the bottom line of this long discussion is clear: the final math is more fundamental than the standard. If the reviewer has objections and still believes that the question is not clear, then I will be grateful for any constructive criticism.

The referee is right that in [15] FQT is treated as a theory that has yet to be built. However, I do not understand why he decided that in this paper FQT is treated as a finished theory. There are no such statements in the paper. On page 9 it says: "In Refs. [13, 9] and other publications we have proposed an approach called FQT (Finite Quantum Theory) when Lie algebras and representation spaces are over a finite field or ring with characteristic p". So, FQT is characterized as an approach, not as an established theory.

2. On vacuum energy.

The referee writes: "The author is deliberately silent about the fact that the "prediction" of the final quantum theory (FQT) about zero vacuum energy ... contradicts the well-known Casimir effect in modern physics. In the peer-reviewed paper, the author does not mention this FQT "prediction" ... and does not explain it." I will describe the meaning of my result on zero vacuum energy. Let's take some standard QFT, for example QED. It is assumed here that the vacuum energy must be zero, but after quantization, an infinite expression is obtained for it. To avoid this, the creation-annihilation operators are said to be written in normal form. This does not follow from the postulates of the theory; we just want to have zero vacuum energy. In [15], which is mentioned by the referee, I refer to my detailed calculation, for example, in [3]. This calculation is as follows. First, as written in [15], I consider the case when the particles are not neutral, i.e., do not coincide with their antiparticles. I take an expression for the vacuum energy, which in the standard theory is infinite, but it is believed that the energy must be zero. I calculate the same sum, but over a finite field and get that for particles with spin 1/2 the sum equals zero. Further in [15], I write: "Our conclusion is that while in standard theory the vacuum energy is infinite, in FQT it is not only finite (in finite mathematics it cannot be infinite) but is exactly zero if s=1 (i.e., s =1/2 in the usual units)".

So, the result is purely technical: one simply calculates the sum, which is infinite in the standard theory, and shows that in FQT for non-neutral particles with spin 1/2 it is equal to zero. I will not discuss how fundamental the result is, but, in any case, purely formally, there is no contradiction with the Casimir effect.

However, I would like to point out the following. First, there is a discussion in the literature whether the Casimir effect is consistent with the assumption in quantum gravity that the vacuum energy must be zero. In addition, the Casimir effect is not about the vacuum energy of the entire system, but about the vacuum energy of only the electromagnetic field in the presence of other bodies. So, it is not even entirely clear whether the term "vacuum energy" can be physically correct in this case since the mean value of the energy operator of the electromagnetic field is not equal to zero not in a vacuum, but when there are other bodies.

So the referee's statement that I deliberately concealed my result in order not to show the contradiction between FQT and the Casimir effect is unfounded. There is no contradiction, and this result is not relevant to this work.

3. Problem of time in quantum theory.

First, about a purely technical issue that the referee discusses. He says that there is no formula (77) in section 8 at all. This is not so because on page 33, the fourth line from the bottom says: Since time is a dimensionful parameter, we define time such that its variation is given by $\Delta t = R\Delta lnp/lnp$. This is just formula (77), but in the units of $c = 1$, which are accepted in this section. Further, the referee writes: "...the quantities n and p on the right side of formula (77) are integers, so Δt can only take discrete values, which obviously does not allow treating t as a classical time. Thus, the author's assumption about the nature of (classical) time (formula (77)) is completely untenable."

Apparently, there is a typo here since in (77) there is no value n at all, but the meaning of the referee's statement seems clear. Again, as I wrote above, if I have an ambiguity, then I try not to immediately conclude that someone else does not understand. But in this case, again, I cannot find any other explanation than the fact that the reviewer's idea of classical time is (to put it mildly) rather strange.

In standard theories, t is assumed to be continuous since these theories use continuous mathematics. But the concept of infinitesimals was introduced by Newton and Leibniz when people did not know that there are atoms and elementary particles and thought that any substance can be divided into any arbitrarily large number of arbitrarily small parts. But now we know that this is impossible and there is no continuity in nature, so it now seems generally accepted that continuity is only a mathematical abstraction. As far as I understand, most physicists recognize this, and here the discussions are only about the accuracy with which quantities, which are considered continuous in continuous theories, are well approximated by such theories.

In particular, with regard to time, this is a purely classical concept. There is some discussion in the literature about the minimum time that can be measured. They write about the nanosecond and even the femtosecond. Those who are serious about inflation models even talk about $10^{-35}s$, and those who are serious about Planck units talk about Planck time $10^{-43}s$. But, it is completely unclear whether the times $10^{-100}s$ or $10^{-1000}s$ make sense. If the time has changed by Δt, and the coordinate r by Δr, then for small Δt and Δr, the value of $\Delta r/\Delta t$ can be very close to the value dr/dt given by some differentiable theory. But no physical experiment can measure Δt and Δr with arbitrarily high accuracy.

Therefore, the reviewer's requirement that any time model must be strictly continuous looks (to put it mildly) very strange. In my FQT approach, no time can be continuous because FQT only uses finite mathematics. However, if the values of Δt are much smaller than the standard classical times, then the values of $\Delta r/\Delta t$ can be very close to those given by the standard classical theories for dr/dt. In particular, the quantities given by formula (77) can be so small that the expressions $\Delta r/\Delta t$ can be formally replaced with good accuracy by dr/dt that satisfy standard differential equations. All this is explained in detail in the paper, but for some reason, the referee is adamant: since strictly continuous time did not turn out, he declares without hesitation that "the author's assumption about the nature of (classical) time (formula (77)) is completely untenable".

In the final part of the report, the referee writes: "The paper lacks physically interesting results on the topic declared by the author (Assumption about the nature of time). The author's arguments are superficial and vaguely formulated, the presentation is clearly biased. ... This work does not provide anything meaningful for clarifying the problem of time and only clearly demonstrates the helplessness of finite mathematics in such an important issue for physics."

Of course, the referee has the right to have his own opinion about my results, but this opinion must be substantiated in the report. In the paper I explain in detail why the problem of time in quantum theory is very important and what is the meaning of my results. The referee also writes that the problem of time is important. But in his report, there is no hint of how he treats this problem. For example, does he consider it a problem that there is no time operator in quantum theory, does he think that time is not a primary concept, but should somehow be derived from quantum theory, or that it is simply necessary to postulate the existence of continuous time t.

It follows from the above that the only reason why the reviewer rejects my results is because, in my approach, time cannot be strictly continuous. If a pure mathematician, who does not know physics at all, said so, then this would be at least somehow understandable. But when a physicist says this, then, as I wrote, it looks (to put it mildly) very strange. In addition, the principles of scientific ethics suggest that any negative statement must be substantiated. The referee writes that my presentation is superficial and tendentious but does not explain what exactly is superficiality and tendentiousness. If he thinks this follows from his critiques, then, as I explain above, none of his critiques are correct.

4. Reviewer's comments on my motivation

In addition to critical remarks about my approach and results, the referee also expresses his opinion about my motivation. He quotes a phrase from the article: "The founders of (quantum) theory were highly educated physicists, but they used only classical mathematics, and even now mathematical education in physics departments does not include discrete and finite mathematics". It would seem that since he quotes this phrase, it is natural to expect him to express his opinion about whether the phrase is correct or not. But he does not express his opinion, but draws a conclusion about the motivation of this phrase: "Thus, the author is simply lobbying for another reform of mathematical education, primarily in Russia". This phrase poses a problem for me, do I need to somehow justify myself and explain that I didn't mean it at all. Since the referee raised this issue, I will try to explain the motivation for this phrase.

Of course, in response to a scientific review, the justification for this phrase may look strange. But nevertheless, I will write at the beginning that I have no interest in any kind of lobbying. I never had a permanent position in education, and sometimes I lectured only in addition to my main work. For example, in 1988 I gave a course of lectures at JINR within the framework of the program "Lectures for young scientists at JINR". In this sentence, I am simply pointing out a fact, but I am not saying anything about how much of the mathematical education in physics departments should be devoted to standard mathematics, and how much to non-standard ones.

I do think that sooner or later fundamental quantum theory will be based on finite mathematics. In my works and even in response to this review, I present various arguments in favor of my point of view. However, from the comments of the reviewer it follows that his way of thinking is this: if someone does not follow the traditional approach to quantum theory (based on continuous mathematics), then it is not science that is behind this, but some other interests. So, it effectively taboos any attempt to deviate from his ideas about quantum theory.

There is no doubt that standard quantum theory has achieved great success in solving various problems. But, at the same time, it is known that the theory is still far from being complete and there are big problems in it. For example, in theory there are infinities. In renormalizable theories, they can be formally eliminated (if one does not pay close attention to mathematical rigor). But it is usually believed that the quantum theory of gravity is a non-renormalizable QFT, and there one cannot get rid of infinities even in the second approximation of perturbation theory. And even in renormalizable theories, the properties of the perturbation series are completely incomprehensible, for example, whether it converges, whether it is asymptotic, and so on. So, if the interaction constant is not small, then nothing can be calculated either.

Some physicists believe that all these problems are not serious, and those who consider these problems serious think that QFT or string theory need to be improved somewhere and then these problems will be solved. True, for example, Weinberg believed that "new theory is centuries away". But still, many people think that everything will be done in ordinary continuous mathematics, although, from what has been said above, it seems obvious that such mathematics cannot be fundamental at the quantum level.

Some famous physicists such as Schwinger, Wigner, Nambu, Gross and others have considered the possibility that the future fundamental quantum theory will be based on finite mathematics. In my papers (including this one) I provide links to the works of other authors, where this possibility was also considered. So, I'm not the first in this. One of the motivations is that in this case there cannot be infinities in principle. If you follow the referee, then we must conclude that all these scientists were also guided not by scientific, but by some kind of lobbying interests.

Based on the foregoing, I believe that the referee's statement about the motivation for this phrase is actually a statement about my scientific dishonesty, which is unacceptable for a scientific review.

As noted above, the reviewer unreasonably believes that I interpret my vacuum energy result as a "prediction" of FQT for all occasions. But no matter how he understands my result, his statement that I deliberately concealed this result is also an accusation of scientific dishonesty. And in fact the referee's statement that I did not mention the natural series and Gödel's theorem for the case of finite mathematics is also such an accusation, and it does not even matter that in this case the referee does not understand that the natural series and Gödel's theorem have nothing to do with finite mathematics. Errors in understanding can occur in many cases, but this can only be interpreted as scientific dishonesty if it is absolutely clear that there are no other explanations. But the way of thinking of the referee is such that he does not even admit that he himself may not understand something or make mistakes, and he does not have a moral problem in the statement that the author shows scientific dishonesty.

My general comments about the referee report are as follows. I understand that it is difficult to review a work in which you are not an expert. Since discrete and finite mathematics are not really taught in physics departments, most physicists are not even familiar with the very basics of such mathematics. This is not a drawback because everyone knows something and does not know something, and it is impossible to know everything. But some physicists have such a way of thinking that if they see a work that they do not understand or that does not correspond to their ideas, they immediately conclude that it is exotic and/or pathology, which has nothing to do with physics.

Until now, I have received reviews from ECHAYA and Letters to ECHAYA, in which the comments were at a high scientific level and the purpose of these comments was to help the author improve the paper. In response to these comments, the papers were usually revised, sometimes more than once. One of the referees wrote that although he personally does not believe in my result, but because he cannot refute it, he does not object to publication. This is an example of high scientific integrity. However, the situation with this referee report is completely opposite.

The referee gives feedback on the paper based on finite mathematics. He does not even know the basics of this mathematics, which immediately follows from his phrases that I use the natural series as a building material for such mathematics and that Gödel's theorems are applicable to finite mathematics. As I noted above, ignorance of finite mathematics is not a drawback. However, without even a basic knowledge of such mathematics, the referee draws a conclusion about my results without even understanding them. As shown above, none of his criticisms are correct. But the main thing is not even that they are all wrong (everyone can make mistakes), but that the referee does not admit that he may not understand something and makes his conclusions in a peremptory tone. Also, in addition to making judgments about scientific results, the referee makes unsubstantiated claims about my motivation. What seems to him to be my mistakes, he explains not by scientific reasons, but by the fact that, allegedly, I deliberately hide the shortcomings of my approach and lobby for something, i.e., actually accuses me of scientific dishonesty. Therefore, I consider the conclusions of the referee unfounded, and I ask the editors to reconsider these conclusions.

P.S. As I noted above, any criticism, even incorrect, is useful to me since helps to understand where readers may have problems. Taking into account the comments of the referee, the paper is significantly expanded and, in particular, it explains in detail the fundamental (and even the most fundamental) fact that ordinary continuous mathematics is a degenerate special case of finite mathematics. Because ECHAYA is a review journal, I think that the presentation of this fact in the paper is justified.

What is the main point of referee report? First, the fact that the referee does not know the very foundations of finite mathematics, but confidently rejects it. And his phrase that since I wrote that the creators of quantum theory were qualified physicists, but did not use finite mathematics, because even now it is not taught in physics departments, then this is an attempt to reform education, primarily in Russia, is simply ridiculous.

It would seem that everything is explained in detail in my answer to the referee report. But the editors sent a new report, which I can't reproduce because it's in a Russian pdf file that I couldn't copy. Therefore, I can only give my answer to this report:

I previously answered the first referee report in detail. However, the second report is even more negative. For example, it contains the phrases "Absurdity!", "Absurdity again" and other non-diplomatic expressions. In addition, the Referee evaluates my general level very low: for example, he writes "for the first time I meet such a physicist" and suggests that I have forgotten the mathematics that I studied in my student years. The review ends with the words: "Further discussion of this topic with the author does not make sense". This means that the Referee declared his second report to be the ultimate truth. In response to the first referee report, I noted that the Referee does not even allow the thought that he may be wrong or misunderstand something. Of course, with this approach of the Referee, it does not make much sense to discuss anything. However, I must respond to those comments that directly relate to the paper. For example, I will not discuss the problem of vacuum energy, because already noted that it is not directly related to the paper (and besides, as usual, the Referee did not even try to understand the meaning of my remarks on this issue). Some comments of the Referee were taken into account, and some were not taken into account. First, I will note those comments that have been taken into account.

- The referee writes that "There is no strict rule in the mathematical community about what to include in the concept of finite mathematics". It seemed to me that this question is clear because there are even textbooks called "Finite Fields" or "Galois Rings". Now some of these textbooks are included in reference [1], and in the text I explain this term and how I understand physics based on finite mathematics, which I call FQT.

- The referee writes that I do not admit my mistakes: in previous works I called p a constant, and now it is a time-related parameter. Now I note that the terminology in physics is not quite clear. For example, the quantities $(c, \hbar, G)$ are called fundamental constants, but there is no proof that they are the same throughout the entire history of the Universe (the question of c is special because one simply chooses a system of units in which the constancy of c is postulated). In previous papers, I did not discuss the problem of time, but in [3] I noted that p can be related to time.

- In the previous version of the paper, I discussed two fundamental problems: a) Standard quantum theory is a special degenerate case of FQT in the formal limit $p \to \infty$.

  b) Even classical mathematics itself is a special degenerate case of finite mathematics in the formal limit $p \to \infty$.

  In a letter to the editor, I wrote that these problems are even the most fundamental ones: they change the standard paradigm about what physics and what mathematics are the most fundamental. I think that the proof of a) and b) was given at the level accepted in theoretical physics. But the Referee believes that "The proof proposed by the author is made at the popular science level, practically 'on the fingers' and is based on physical analogies". Prior to this, the Referee writes that "a professional approach to this issue should be based on rigorous mathematics, for example, in the style of Bourbaki's famous books".

  I recently wrote a paper (posted it on the Internet and sent it to a mathematical journal) in which a rigorous mathematical proof of statements a) and b) is given. At the same time, one of the necessary conditions for the feasibility of b) is the feasibility of a). Considering this, the words of the Referee and the fact that ECHAYA is a review journal, I have included this proof in a new version of the paper.

  Now about the comments of the Referee, which were not taken into account.

- On Gödel's theorems. In response to the first review, I tried to explain why the Referee's statements about Gödel's theorems are wrong. The theorems say that problems with the foundation of classical mathematics arise because this mathematics uses the entire infinite natural series. There are no infinite sets in finite mathematics, and Gödel's theorems do not apply here. I wrote that finite mathematics begins with a set of numbers $(0, 1, ...p-1)$, but I did not call these numbers natural, so as not to give the impression that finite mathematics begins with the entire natural series. Of course, the numbers in this set can be called natural, but they can just as well be called real or complex. However, the Referee has argued that I am using the natural series as a building block, and therefore Gödel's theorems apply to finite mathematics too. Now the Referee quotes the second paragraph from the top on page 9 and says: "Does it not follow that these theorems also apply to the author's approach, since he uses natural numbers when constructing the ring R_p?" and concludes: "Obviously, the author is completely confused with Gödel's theorems. So, the Referee's logic is still such that since the numbers $(0, 1, ...p-1)$ are natural, then I use the natural series and therefore Gödel's theorems apply to finite mathematics as well. Therefore it was the Referee who got confused: he still doesn't understand that Gödel's theorems only apply to theories that use the entire infinite natural series, and in finite mathematics there can be no infinite sets by definition.

- On finite mathematics in teaching. I wrote that the founders of quantum theory did not use finite mathematics, and even now discrete and finite mathematics are not included in the standard mathematical education in physics departments. The Referee saw in this phrase a secret meaning, that allegedly, I call for the reform of mathematical education, especially in Russia. In response to the first referee report, I tried to justify myself that there was no secret meaning in this phrase and that it was simply a statement of facts. However, in the second report, the Referee does not accept my excuses and writes: "And if this phrase is just a statement of fact, that is, without a semantic load, then why did the author not delete it in response to the referee's remark. Thus, the motivation indicated by me is the only logical explanation for this phrase of the author". Of course, if the phrase does not carry a semantic load, then it has no meaning. But it just carries a very large semantic load. As I note in the paper, the concepts of infinitesimal/large, continuity, etc. were introduced by Newton and Leibniz over 300 years ago. Then people did not know about atoms and elementary particles and thought that any substance can be divided into an arbitrarily large number of arbitrarily small parts. But now we know that this is not so: when we reach the level of atoms and elementary particles, then further division loses its meaning. There are no infinitesimals and continuity in nature. Therefore, this phrase explains a very strange phenomenon: although everyone already knows that there are elementary particles, nature is discrete and there are no infinitesimals in it, but even after 90+ years of quantum theory it is based on continuous mathematics, and the vast majority of physicists still think that the fundamental problems of quantum theory must be solved within the framework of continuous mathematics.

To support his interpretation, the Referee reminds me of what I taught as a student. He writes: "Are linear algebra, group theory, group representation theory, computational methods, programming, etc. not the basis of finite mathematics and the most finite mathematics? All this is taught in physics departments. Didn't the author study these subjects during his student years? Or he just forgot it and that's the only reason he stands up for the reform of mathematical education?"

Calculation methods and programming are not mathematics. So, let's look at linear algebra, group theory, and group representations, which I hope I haven't forgotten. In standard linear algebra, the spaces are finite-dimensional, but the coordinates of the vectors can be any real or complex numbers, i.e., they belong to infinite sets. Therefore, standard linear algebra, by definition, does not apply to finite mathematics. In the theory of crystals, finite groups are considered, but the groups that are studied in quantum theory (the rotation group, the Lorentz group, the Poincare group, etc.) use infinitely many real numbers, i.e., these groups are infinite sets. The representations of these groups are considered in linear spaces, in which the coordinates of the vectors can also be any real or complex numbers. So, these subjects are not included in finite mathematics. In the latter, linear spaces can be over a finite ring or field. But these concepts are not taught in physics departments. Moreover, I talked with many physicists from IHEP, ITEP and JINR and did not notice that any of them, except for M.A. Olshanetsky (by the way, the editors can ask him for his opinion on my article) used linear spaces over finite rings or fields.

- On the problem of time. In response to the first referee report, I explained in detail that in classical theories, time is considered continuous, and in quantum theory there is not even a time operator. But, as noted above, there is no continuity in nature, continuity is an idealization, and no quantity that is supposed to be continuous can be measured with absolute accuracy. In particular, the fact that time is strictly continuous is also an idealization. Therefore, discrete models can give a good experimental description of time. The referee did not raise any objections, and therefore it is not clear whether he understood my statements. But all the same, he writes that this is a logical mess, which I send to ECHAYA. Judging by the words of the Referee, he believes that since time is a classical concept, then, by definition, only those approaches in which time is continuous are allowed. The referee does not give any physical arguments in favor of this dogma but declares unacceptable what does not fit into this dogma.

- The referee writes that since p is dimensionless, and I compare physical quantities with it, this is absurd. But I explain in detail that in the most fundamental physical theories, all quantities are dimensionless, and the dimensional parameters $(c, \hbar, R)$ are needed only to transfer more general theories to less general ones. The referee did not raise any objections to this.

- In conclusion, what is in the Referee's way of thinking is unacceptable for me, regardless of what he knows or does not know about specific problems.

  I really believe that statements a) and b) are fundamental and important not only for physics, but also for the foundation of mathematics. In a previous letter to the editor, I tried to explain why my attempts to convince mathematicians have so far been generally unsuccessful. The referee is right that in the mathematical community "there is no generally accepted division of mathematics into fundamental and non-fundamental". My experience with mathematicians also shows that usually the horizons of a mathematician are limited to what he does. In particular, "finite" mathematicians believe that they have their own problems, "continuous" mathematicians have their own, and these problems do not intersect. When I tried to convince "finite" mathematicians that finite mathematics is more fundamental than continuous mathematics, they said that planes fly, bridges are built, and all this is based on differential equations. My attempts to explain to them that these equations come from classical physics and therefore are only approximate, were not successful because they do not understand the difference between classical and quantum physics. But in the physics community it is well known which theories are more and which are less fundamental. However, the Referee used my explanations against me. He writes that since my results have not yet aroused interest in the scientific community, there is no need to "hype" them.

  When Schrödinger and Heisenberg created quantum mechanics, almost no one understood it (and many physicists still do not understand it). If the question of publishing the works of Schrödinger and Heisenberg depended on people with Referee's way of thinking, then these works would never have been published. In my case, the situation is very simple. I claim to be giving a rigorous mathematical proof of statements a) and b). In particular, in the proof I give a definition of which theory is fundamental and which is its special case. There is probably no doubt that statements a) and b) change the standard paradigm about what physics and what mathematics are fundamental. Therefore, the scientific approach should not be as many people are interested in it, but as follows: my proof is correct or incorrect. If someone can refute my proof, then I would be very grateful and would immediately withdraw my paper. The referee believes that in my papers on FQT there are no physical results and that in them finite mathematics has shown its helplessness in physics. Of course, he has the right to have such a personal opinion. But now, if he cannot refute a) and b), then to express such an opinion officially is at least unethical.

  One can talk about the great successes of the standard quantum theory (and I agree with this) or that there are problems in this theory (and I also agree with this), but if statements a) and b) are true, then the future quantum theory cannot be based on continuous mathematics. The referee writes that my proof should be discussed first of all with professional mathematicians, that for physicists it is only of academic interest, and therefore the submission of the paper to ECHA is "inappropriate". Of course, I will try to convince mathematicians too, but, as I wrote, the problem here is that many of them have the same way of thinking as the Referee only with inversion physics$\leftrightarrow$mathematics: they see that the motivation comes from physics which they don't know, so they don't try to get into the mathematical proof of a) and b). At the same time, it is rather strange that the Referee thinks that all physicists have the same way of thinking as him, that mathematical proofs are just something academic for them, and that only applications are important to them.

  I hope that among physicists there are those who can apply some theory, not only believing

> that it is correct, but also if they themselves are convinced of this. Historically, as a rule, new physical theories arose when mathematics was involved, which had not been used in physics before. For example, before quantum theory, Hilbert spaces were not used in physics. Probably, the Referee has no doubts that Dirac is a great physicist. Here are his words, which, from the point of view of the Referee, are complete sedition: "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 also note that ECHAYA publishes many papers in which purely mathematical problems of quantum theory are considered, i.e., it is assumed that there are physicists for whom mathematics is not only of academic interest.

What I liked about the approach of this referee: he does not play diplomacy, cuts the truth (as Russians say, "rezhet pravdu-matku" as he understands it) and is not afraid that it may turn out that he does not understand something. Therefore, it is easy to respond to his referee reports because it's clear what he's saying. For example, Western reviewers are usually much more cunning. When they realize that they are incompetent, they try to go around the corners so that this does not manifest itself, so they pronounce some general words and you have to think how to answer so that it is clear that the reviewer has no idea.

After this answer I got a second reviewer's review:

Review of the article by Felix M. Lev

"Finite mathematics, finite quantum theory and a conjecture on the nature of time"

The author considers an extremely non-standard approach to quantum theory (QT) based on finite ring or field with characteristic $p \gg 1$ (the finite quantum theory (FQT). Author, in particular, shows that the conventional QT is a limiting case of FQT as $p \gg 1$. In the QFT approach, the characteristic p is a fundamental evolving [!] parameter which defines how the classical equations of motions arise as a consequence of changing of p; moreover, p may be (this is an author's conjecture) the "precursor" of notion of time itself ("...the existence of classical time is a consequence of the fact [!?] that p changes"). Well, although our physiology (and/or psychology?) does not provide a chance to understand what is evolution of Universe (or its part) out of time, the reader may believe that the author understands it and then tries to follow the formal (finite) mathematics. Nonetheless, if p changes, there must be even more fundamental cause governing this "fact"... but let's stop the metaphysics. Obviously, the very unconventional concepts formulated in the paper under review (as well as in the previous publications by the author (Refs. [1-3]) are highly disputable, but they are nontrivial and thus interesting. So these concepts must be presented to the community at least as a subject of criticism, controversy... or silence. A handicap of the paper (from my personal point of view) is its volume together with too lengthy explanations of comparatively simple and known things and too lapidary discussion of the specific axiomatics and (even more important) implications and (potentially) falsifying effects of the FQT.

1 The article looks like a novel about Cabbages and Kings (in other words, about everything known to the author). I guess that many items could be ejected in order to simplify understanding of the main ideas and results and to classify the ins and outs of the theory; this is not a demand but just a suggestion. In fact I have a lot of questions and even objections against the author's categorical statements, but I would not like to force a further increase in the length of the text. In conclusion, I think that the writeup under review is of interest for the community and thus is suitable for publication.

And yes, "Viennese School's philosophy" still predominates in physics, if we are able to separate postulates and consequences. This philosophy simply suggests to compare the consequences (and not the postulates) with the relevant empirical facts, but it does not demand to test the axioms of mathematics.

I am grateful to the referee for this review, after which the revised version of the article was accepted and published in [20].

Chapter 8

Attempt to publish a monograph in Springer

8.1 Monograph proposal

I decided to present my approach to quantum theory and my results in a monograph. My proposal for a monograph sent to Springer is:

Dear Dr. Lahee,

Please consider my monograph proposal. The monograph will be based on my paper https://arxiv.org/abs/1104.4647 which contains 259 pages. Probably the final version will be longer but not considerably. The title of the monograph is: Finite Quantum Theory and Applications to Gravity and Particle Theory and the abstract is:

We argue that the main reason of crisis in quantum theory is that nature, which is fundamentally discrete and even finite, is described by continuous mathematics. Moreover, the ultimate physical theory cannot be based on continuous mathematics because it has its own foundational problems which cannot be resolved (as follows, in particular, from Gődel's incompleteness theorems). In the first part of the work, we discuss inconsistencies in standard quantum theory and reformulate the theory such that it can be naturally generalized to a formulation based on finite mathematics. It is shown that: a) as a consequence of inconsistent definition of standard position operator, predictions of the theory contradict the data on observations of stars; b) the cosmological acceleration and gravity can be treated simply as kinematical manifestations of de Sitter symmetry on quantum level (i.e., for describing those phenomena the concepts of dark energy, space-time background and gravitational interaction are not needed). In the second part we consider a quantum theory based on finite mathematics with a large characteristic p. In this approach the de Sitter gravitational constant depends on p and disappears in the formal limit $p \to \infty$ i.e., gravity is a consequence of finiteness of nature. The application to particle theory gives that: a) the electric charge and the baryon and lepton quantum numbers can be only approximately conserved (i.e., the notion of a particle and its antiparticle is only approximate); b) particles which in standard theory are treated as neutral (i.e., coinciding with their antiparticles) cannot be elementary. We consider a possibility that only Dirac singletons can be true elementary particles. Finally, we discuss a conjecture that classical time t manifests itself as a consequence of the fact that p changes, i.e., p and not t is the true evolution parameter.

The monograph will be based on my results published in:

[1] F.M. Lev, Some Group-theoretical Aspects of SO(1,4)-Invariant Theory. J. Phys., A21, 599-615 (1988).

[2] F. Lev, Representations of the de Sitter Algebra Over a Finite Field and Their Possible Physical Interpretation. Yad. Fiz., 48, 903-912 (1988).

[3] F. Lev, Modular Representations as a Possible Basis of Finite Physics. J. Math. Phys., 30, 1985-1998 (1989).

[4] F. Lev, Finiteness of Physics and its Possible Consequences. J. Math. Phys., 34, 490-527 (1993).

[5] F. Lev, Exact Construction of the Electromagnetic Current Operator in Relativistic Quantum Mechanics. Ann. Phys. 237, 355-419 (1995).

[6] F. M. Lev, The Problem of Interactions in de Sitter Invariant Theories. J. Phys., A32, 1225-1239 (1999).

[7] F. Lev, Massless Elementary Particles in a Quantum Theory over a Galois Field. Theor. Math. Phys., 138, 208-225 (2004). The journal is published by Springer.

[8] F.M. Lev, Could Only Fermions Be Elementary? J. Phys., A37, 3287-3304 (2004).

[9] F. Lev, Why is Quantum Theory Based on Complex Numbers? Finite Fields and Their Applications, 12, 336-356 (2006).

[10] F.M. Lev, Quantum Theory and Galois Fields, International J. Mod. Phys. B20, 1761-1777 (2006).

[11] F.M. Lev, Positive Cosmological Constant and Quantum Theory. Symmetry 2(4), 1401-1436 (2010).

[12] F.M. Lev, Introduction to a Quantum Theory over a Galois Field. Symmetry 2(4), 1810-1845 (2010).

[13] F.M. Lev, Is Gravity an Interaction? Physics Essays, 23, 355-362 (2010).

[14] F. Lev, Do We Need Dark Energy to Explain the Cosmological Acceleration? J. Mod. Phys. 9A, 1185-1189 (2012).

[15] F. Lev, de Sitter Symmetry and Quantum Theory. Phys. Rev. D85, 065003 (2012).

[16] F.M. Lev, A New Look at the Position Operator in Quantum Theory. Physics of Particles and Nuclei, 46, 24-59 (2015). The journal is published by Springer.

[17] F.M. Lev, Why Finite Mathematics Is The Most Fundamental and Ultimate Quantum Theory Will Be Based on Finite Mathematics. Physics of Elementary Particles and Atomic Nuclei Letters, 14, 77-82 (2017). The journal is published by Springer.

[18] F. M. Lev, Fundamental Quantal Paradox and its Resolution. Physics of Elementary Particles and Atomic Nuclei Letters, 14, 444-452 (2017). The journal is published by Springer.

and possibly in other journals.

I graduated from the Moscow Institute for Physics and Technology, got a PhD from the Institute of Theoretical and Experimental Physics in Moscow and a Dr. Sci. degree from the Institute for High Energy Physics (also known as the Serpukhov Accelerator). In Russia there are two doctoral degrees; Dr. Sci. degree is probably an analog of Habilitationsschrift in Germany. In

Russia I worked at the Joint Institute for Nuclear Research (Dubna, Moscow region) and now I work at a software company in Los Angeles, USA.

I have many papers published in known journals (Ann. Phys., Few Body Systems, J. Math. Phys., J. Phys. A, Nucl. Phys. C, Phys. Rev. C and D, Phys. Rev. Letters and others). The majority of those papers are done in the framework of more or less mainstream approaches. On the other hand, the proposed monograph will be done in the fully new approach which I am working on for many years. In this approach quantum theory is based on finite mathematics.

I think that the main problems in convincing physicists that ultimate quantum theory will be based on finite mathematics are not scientific but subjective. First of all, the majority of physicists do not have even a very basic knowledge in finite mathematics. This is not a drawback because everybody knows something and does not know something, and it is impossible to know everything. However, many physicists have a mentality that only their vision of physics is correct, they do not accept that different approaches should be published and if they do not understand something or something is not in the spirit of their dogmas then this is pathology or exotics which has nothing to do with physics.

Probably this situation has happened in view of several reasons. For example, the successes of QED at the end of the 40th were very impressive and it is of course impressive that the theory gives correct eight digits for the electron and muon magnetic moments and five digits for the Lamb shift. From mathematical point of view, QED has several inconsistencies the reasons of which are clear. The above famous results are obtained by subtracting infinities from each other. However, in view of these and other results the mentality of the majority of physicists is that agreement with the data is much more important than mathematical consistency and many of those physicists believe that all fundamental problems of quantum theory can be solved in the framework of QFT or string theory (which has similar mathematical inconsistencies).

The meaning of "quantum" is discrete and historically the name "quantum theory" has arisen because it was realized that some physical quantities have discrete spectrum. The founders of quantum theory were highly educated physicists, but they used only standard continuous mathematics, and even now discrete and finite mathematics is not a part of standard mathematical education at physics departments. Several famous physicists (e.g., Schwinger, Wigner, Nambu, Gross and others) discussed a possibility that ultimate quantum theory will be based on finite mathematics. One of the reasons is that in this case infinities cannot exist in principle. However, standard quantum theory is based on continuous mathematics. Efforts of many physicists to resolve fundamental difficulties of this theory (e.g., existence of infinities) have not been successful so far. Continuous mathematics describes many data with high accuracy, but this does not necessarily imply that ultimate quantum theory will be based on continuous mathematics. For example, classical mechanics describes many data with high accuracy, but fails when v/c is not small. Continuous mathematics is not natural in quantum theory. For example, the notions of infinitely small and infinitely large have arisen when people did not know about atoms and elementary particles and believed that any object can be divided by any number of parts. Ultimate quantum theory cannot be based on continuous mathematics because the latter has its own foundational problems (as follows, for example, from Gődel's incompleteness theorems).

Moreover, as explained, for example, in Ref. [17], continuous mathematics itself is a special degenerated case of finite mathematics: the latter becomes the former in the formal limit when the characteristic of the ring or field in finite mathematics goes to infinity. The fact that continuous mathematics describes many data with high accuracy is a consequence of the fact that at the present stage of the Universe the characteristic is very large. There is no doubt that the technique of continuous mathematics is useful in many practical calculations with high accuracy. However, from the above facts it is clear that the problem of substantiation of this mathematics (which was discussed by many famous mathematicians, which has not been solved so far and which probably cannot be solved (e.g., in view of Gődel's incompleteness theorems)) is not fundamental because continuous mathematics itself, being a special degenerated case of finite mathematics, is not *fundamental.*

It is also seems obvious that discrete spectrum is more general than continuous one: the latter can be treated as a formal degenerated special case of the former in a special case when the distances between the levels of the discrete spectrum become (infinitely) small. In physics there are known examples in favor of this point of view. For example, the angular momentum has a pure discrete spectrum which becomes the continuous one in the formal limit $\hbar \to 0$. *Another example is the following. It is known that Poincare symmetry is a special degenerated case of de Sitter symmetry. The procedure when the latter becomes the former is called contraction and is performed as follows. Instead of some four de Sitter angular momenta* M_{dS} *we introduce standard Poincare four-momentum* P *such that* $P = M_{dS}/R$ *where* R *is a formal parameter which can be called the radius of the world. The spectrum of the operators* M_{dS} *is discrete, the distances between the spectrum eigenvalues are of the order of* $\hbar$ *and therefore at this stage the Poincare four-momentum* P *has the discrete spectrum such that the distances between the spectrum eigenvalues are of the order of* $\hbar/R$. *In the formal limit* $R \to \infty$ *the commutation relations for the de Sitter algebras become the commutation relations for the Poincare algebra and instead of the discrete spectrum for the operators* M_{dS} *we have the continuous spectrum for the operators* P.

I fully agree with Dirac who wrote:

"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 these words such that at the quantum level the usual physical intuition does not work and we can rely only on mathematics. The majority of physicists do not accept this approach and believe that physical meaning (which often is understood simply as common sense) is more important than mathematics. In discussions with me some of them said that the characteristic p in my approach is simply a cutoff parameter. This is an example when finite mathematics is treated in view of continuous mathematics while finite mathematics considerable differs from continuous one. For example, special relativity cannot be treated simply as classical mechanics with the cutoff c for velocities.

As shown in my works, the approach when quantum theory is based on finite mathematics sheds a fully new light on fundamental problems of gravity, particle theory and even mathematics itself. I would be very grateful if Springer accepts my monograph proposal.

8.2 Reviewers Answers

Reviewer 1

What I do not really see is the fundamentally new aspect. It seems that any finite approximation to the standard continuum theory of gravity, quantum mechanics or quantum field theory more or less gives what the author proposes. But then, any such finite approximation is implemented (though not at a group theoretical level) when making numerical calculations of quantum mechanical (or other) problems on a computer. The criticisms of the mainstream continuum theories are, for my taste, too commonplace and unspecific, or have already been responded to within the usual mainstream theories. Some of the papers cited to support the author's criticism of the mainstream theories are known to present misguided views that have been clarified elsewhere in the literature. It is also not really clear how the author's approach would get around the criticized issues.

In conclusion, I think the book project does not meet the quality expectations of FTPH. I would not like to endorse it, even though FTPH is open to more speculative approaches and non-mainstream philosophical viewpoints.

Reviewer 2

I think that the proposal is kind of esoteric, ignoring 80 years of successful quantum theory. Now, there are problems with QED and QFT in general and they are of various kinds, *position operator for photons is one such problem, infinities another one, and the author is only focusing on those. But the first question one would have to address is, when one wants to change the world, how does the world in which we actually live fit into that. The author ignores that or hides the discussion somewhere, where it is hard to find. That's a second issue, the book is all words, hardly formulas, almost like a book of philosophy. I cannot endorse that proposal.*

In the reviews, everything is as usual: they don't understand anything and don't want to understand, but since I don't have QFT, they immediately send me away. What is especially strange here: this section in Springer is called FTPH - fundamental theories of physics, and the rules say that you must offer something fundamentally new, not standard. So, the reviewer should understand that there may be something unusual. But, as usual, the rules are not written for them, and if they do not understand, they immediately reject it. For example, although one of my main goals is to explain that standard continuous mathematics is a special degenerate case of finite mathematics, and not vice versa, the way of thinking of this dumbass is this: he believes that discrete is an approximation to continuous and writes a negative report. My response to those reviews is:

Monograph proposal: "Finite Quantum Theory and Applications to Gravity and Particle Theory" by F. M. Lev

Author's Comments on FTPH Reviewer Reports

My first observation is about the attitude of the reviewers from the formal point of view. My experience is that in many cases reviewers do not think that they are bound by the editorial policy of the journal for which they write a report and they believe that they know better what should or should not be published.

The FTPH editorial policy says in particular: "Although the aim of this series is to go beyond established mainstream physics, a high profile and open-minded Editorial Board will evaluate all contributions carefully to ensure a high scientific standard". As follows from this sentence, the reviewers MUST read the author's proposal carefully and at least to have a minimal understanding of what the author proposes. Without this understanding it is not possible to make a conclusion whether "a high scientific standard" is met or not. In addition, the reviewers should be open-minded, i.e., they should accept that in physics different approaches have a right to exist and so they should not reject the proposal only because it is not in the mainstream.

In my proposal I describe the motivation in great details but the reports do not give any indication on whether the reviewers carefully read the proposal, whether they made any efforts to understand it and whether they are qualified to understand.

As I explain, in my approach quantum theory is based on finite mathematics, it is more fundamental than standard continuous mathematics and the latter is a degenerated special case of the former. So for understanding those key statements the reviewers should have at least very basic knowledge in finite mathematics. However, the reports do not show any sign that the reviewers have this knowledge.

Let me quote an extract from my proposal: "... the majority of physicists do not have even a very basic knowledge in finite mathematics. This is not a drawback because everybody knows something and does not know something and it is impossible to know everything. However, many physicists have a mentality that only their vision of physics is correct, they do not accept that different approaches should be published and if they do not understand something or something is not in the spirit of their dogmas then this is pathology or exotics which has nothing to do with physics". This extract fully applies to the reviewer reports.

For example, Reviewer 1 thinks that since my approach is based on discrete mathematics then it is simply an "approximation to the standard continuum theory". First of all, if my approach is only an approximation then it is not FTPH at all. So it should be rejected right away and the remaining part of the report is obsolete. The mentality of the reviewer is that discrete is an approximation to continuous. This mentality is based on standard mathematical education where, for example, integral sums are treated as an approximation to the "true" value obtained by integration. In my proposal I explain why in the given case standard mentality does not work and below will *explain this again.*

Reviewer 1 writes that "The criticisms of the mainstream continuum theories are, for my taste, too commonplace and unspecific..." First of all, my remarks about problems of those theories are not a criticism but simply a reminder of well-known facts. The reviewer says that this "have already been responded to within the usual mainstream theories" but gives no specifics. For example, does he/she think that the problem of infinities has been already solved? Or in his/her opinion this problem is not important? For example, Weinberg, who is a famous physicist, writes in his textbook on QFT: "Disappointingly this problem appeared with even greater severity in the early days of quantum theory, and although greatly ameliorated by subsequent improvements in the theory, it remains with us to the present day". The title of one Weinberg's paper is "Living with infinities". He also writes that a new theory may be "centuries away". Do those Weinberg statements have been already refuted and if yes then when and where? Do we have quantum gravity where the renormalized perturbation series does not contain infinities?

As I note in the proposal, several famous physicists discussed a possibility that fundamental quantum theory will be based on finite mathematics and one of the arguments is that in this case infinities cannot exist in principle. Reviewer 1 says that "Some of the papers cited to support the author's criticism of the mainstream theories are known to present misguided views that have been clarified elsewhere in the literature" but does not give any explanation on what is misguided, what has been clarified and no references are given.

Reviewer 1 says: "It is also not really clear how the author's approach would get around the criticized issues". I do not see any meaning in this statement because the reviewer does not say specifically what is not clear to him/her and, as noted above, there is no indication that he/she has at least a basic understanding of my approach. Scientific ethics imply that any negative statement should be substantiated, i.e., the words "too commonplace", "unspecific", "not really clear", "speculative" and others should be explained.

In summary, the report of Reviewer 1 contains nothing specific, contradicts scientific ethics and fully contradicts the FTPH policy because he/she recommends rejection without any understanding of my approach and results.

The report of Reviewer 2 also does not follow standards of scientific ethics. He/she says that I ignore "80 years of successful quantum theory". This is a very serious accusation but no explanation is given. Does he/she think that any attempt to improve the theory means ignoring it? In particular, does he/she think that relativistic mechanics ignores nonrelativistic one? Or does quantum theory ignore classical one? He/she also thinks "that the proposal is kind of esoteric" but again does not explain why he/she thinks so.

In contrast to Reviewer 1, Reviewer 2 acknowledges that there are problems with the photon position operator and with infinities but says that "the author is only focusing on those". This immediately shows that, in full contradiction to the FTPH policy, Reviewer 2 even did not carefully read my abstract where it is indicated what problems are discussed. Reviewer 2 says: "But the first question one would have to address is, when one wants to change the world, how does the world in which we actually live fit into that. This sentence is fully puzzled. Why does he/she think that I want to change the world? If I show that standard photon position operator is inconsistent then does it mean that I want to change the world? Does it mean that any improvement of standard theory means changing the world?

Reviewer 2 says: "The author ignores that or hides the discussion somewhere, where it is hard to find". Why was it hard for the reviewer to find? Was it hard to read the title of paper [15]?

Then he/she writes: "...the book is all words, hardly formulas, almost like a book of philosophy". Probably Reviewer 1 read only the introductory chapter because the other chapters contain extensive mathematical derivations of new results which have never been published. The existing version of the manuscript contains 259 pages. Again, in contradiction to scientific ethics, Reviewer 2 does not explain how many pages he/she treats as "all words" and how many as "hardly *formulas".*

In summary, my conclusion on the report of Reviewer 2 is absolutely the same as the conclusion on the report of Reviewer 1.

In view of the FTPH policy, the author should submit to FTPH a fundamentally new approach, not just a variation of mainstream one. So the reviewers should be ready that standard mentality is not sufficient for understanding the proposal. In particular, standard mentality that discrete is only an approximation to continuous, does not imply in the given case. In my proposal I tried to explain this point and below will try to explain again.

The notions of infinitely small, continuity etc. were proposed by Newton and Leibniz approximately 370 years ago. At that time people did not know about atoms and elementary particles and believed that any object can be divided by arbitrarily large numbers of arbitrarily small parts. But now it is obvious that when we reach the level of atoms and elementary particles then standard division loses its meaning and one cannot obtain arbitrarily small parts. It is immediately clear from this observation that the notions of infinitely small and continuity are not fundamental on quantum level. Moreover, it is rather strange to think that fundamental quantum theory should be based on mathematics involving infinitely small and continuity. The founders of quantum theory were highly educated physicists but they used only standard continuous mathematics, and even now discrete and finite mathematics is not a part of standard mathematical education at physics departments. For understanding my statement that finite mathematics is more fundamental than standard continuous one and that the latter is a degenerated special case of the former (see e.g. paper[16]), at least a very basic knowledge of finite mathematics is needed. The reviewer reports show that the reviewers do not have this knowledge. As I note above, this is not a drawback. However, scientific ethics implies that it is not decent to judge an approach without having at least very basic knowledge about the approach.

In particular, finite mathematics does not involve continuity, derivatives or integrals; those notions are approximations which might or might not work in different situations. In finite mathematics finite sums are possible. In some cases such sums can be approximated by integrals. So in this case not discrete is an approximation of continuous but vice versa. In my proposal I also explain that the continuous spectrum is an approximation of the discrete one but not vice versa.

Following this response, Angela Lahee wrote to me to send her my suggestions for reviewers. I sent them and thought that now I have to wait for what the reviewers will write and what she will say. But suddenly I received this email:

I have now received back some further comments on your manuscript. Although two of the reviews by persons you had suggested were positive about the work you present, I'm afraid that other established researchers in quantum theory remain skeptical. In particular they question the sense of applying finite mathematics to QFT in place of the well established renormalisation theory.

They are nonetheless open to new approaches. But they propose (and I agree) that the better way to disseminate new ideas of this kind is first to publish a series of short(er) self-contained papers demonstrating the power of this approach. If the published results have some impact in the community, this would be the right moment to publish a longer book-length treatment.

So I am sorry, but we will not change our decision about this proposal. I hope you will be successful in publishing your ideas as one or more journal papers.

That is, again, since I don't have QFT, I should went away, and the words that "They are nonetheless open to new approaches" contradict the previous one. My answer was this:

Dear Angela,

Thank you for this info. To be honest, it looks rather strange for me. You say that "other established researchers in quantum theory remain skeptical. In particular they question the sense of applying finite mathematics to QFT in place of the well established renormalisation theory." Did they send you their reports or these are only words? Do they have at least very basic understanding of finite mathematics? They propose me to publish new papers. My proposal is based on papers published in known journals: Annals of Physics, J.Math.Phys., J.Phys.A, Phys.Rev. D, Physics of Particles and Nuclei and Theor. Math. Phys. (the last two journals are published by

Springer). If this is not sufficient then what are their requirements for publications? You say that "If the published results have some impact in the community...". Several physicists support my approach. You say that you received two reports from physicists I proposed. But my list contains six names. Will you wait for other reports? Indeed, many physicists do not accept my approach but so far I failed to receive clear explanations of their reasons and to be honest, I suspect that one of the main reasons is that they do not have at least very basic understanding of finite mathematics. For the problems I discuss I do not need QFT and renormalization theory because I consider only systems of free particles in the framework of standard de Sitter symmetry or de Sitter symmetry based on finite math. I show that those symmetries result in effective interactions which have not been discussed in the literature, they change the notion of elementary particles, conservation laws etc.

Let me also note that in 2017 Springer published a monograph by Vourdas where applications of finite math are discussed and this monograph has nothing to do with QFT and renormalization theory. And finally my MOST fundamental result is: standard continuous math with infinitely small, continuity etc. (which was started by Newton and Leibniz approx.. 370 years ago) is a degenerated special case of finite mathematics in the formal limit when the characteristic of the field or ring in the latter goes to infinity. Moreover, in view of existence of elementary particles it is obvious that in nature there are no infinitely small quantities and no continuity but fundamental quantum theories are based on continuous math and many physicists oppose results where the other math is used. This result fundamentally changes the usual philosophy on what math and what physics are the most fundamental. I have no doubt that sooner or later this result will be acknowledged.

In summary, I would be very grateful if you explain me the following. Will you wait for the reports of other physicists proposed in my list? Could you tell me what are the requirements that my results have an impact in the community? And to be honest, I would be very grateful if you tell me without diplomacy whether I have real chances to be published by Springer. If the clear answer is "no" then no questions will be asked and I will not bother you anymore.

And after that I received the following answer:

Dear Felix,

Given the consensus among four long-standing advisors who I have now consulted, I am afraid that your book will not be further considered by Springer. It is a difficult case, as one of the reviewers commented, so perhaps another publisher will come to a different conclusion.

That is, now she is already clearly saying that there is no chance of publishing a monograph. I can't blame her because she can only make decisions that she is allowed to make. And in this situation, she had no choice.

But two years later the situation changed. I revised the monograph and sent her a new request. I wrote in it that my request is simple: since I send a proposal to the Fundamental theories of physics section, then those who will consider my proposal are obliged to consider it as part of the editorial policy of this series. I will give in full this policy:

The international monograph series "Fundamental Theories of Physics" aims to stretch the boundaries of mainstream physics by clarifying and developing the theoretical and conceptual framework of physics and by applying it to a wide range of interdisciplinary scientific fields. Original contributions in well-established fields such as Quantum Physics, Relativity Theory, Cosmology, Quantum Field Theory, Statistical Mechanics and Nonlinear Dynamics are welcome. The series also provides a forum for non-conventional approaches to these fields. Publications should present new and promising ideas, with prospects for their further development, and carefully show how they connect to conventional views of the topic. Although the aim of this series is to go beyond established mainstream physics, a high profile and open-minded Editorial Board will evaluate all contributions carefully to ensure a high scientific standard.

That is, a lot of good things are being said about what can be non-conventional approaches, new and promising ideas and that members of the Editorial Board should be open-minded. As I already wrote, the previous reviews in this series had nothing to do with this policy. Angela

Lahee replied that she agreed that my request was reasonable and would ask members of the Editorial Board to write a review in accordance with this editorial policy. And she asked many people to write a review, but no one wanted to. As she wrote, someone dragged on for a long time, and then wrote that he was busy with his own affairs and did not have time, but most simply did not answer.

I can't understand their logic: if they agreed to be in this Editorial Board, then it would seem that they took on a moral obligation to write reviews within the editorial policy. And if they don't want to, then why are they in this editorial board? I think that the main reason for this attitude is that they, like the vast majority of physicists, do not even know the very foundations of finite mathematics. Maybe there were other reasons as well.

After failing to get a review, Angela wrote that because no one was against the publication of the monograph, and the reviews she requested two years ago were generally positive, then the monograph can be published, but not in the Fundamental theories of physics series, but as a stand-alone book. And it has been published! Moreover, Angela suggested that I change the title of the monograph, and I liked her suggestion. Therefore, the monograph was published with the title: "Finite Mathematics as the Foundation of Classical Mathematics and Quantum Theory. With Application to Gravity and Particle theory". See [22] for a more detailed link to the book.

So, the general conclusion from communication with Springer: everything is as usual, if you offer something new that the establishment does not understand, then there are almost no chances, but you only need what fits into their dogmas. And, as usual, everything that is written in the editorial policy has no meaning, i.e., Springer does not follow the rules that it proclaimed. But I was very lucky that Angela Lahee turned out to be a very decent person. As part of her official duties, she could have acted differently, but she acted according to the highest standards of decency. I am very grateful to her for her help in publishing the book.

Chapter 9

Attempts to publish a rigorous proof that finite quantum theory and finite mathematics are more fundamental than standard quantum theory and classical mathematics respectively

I have argued above that FQT is more general (fundamental) than standard quantum theory and that finite mathematics is more general (fundamental) than classical mathematics. These arguments were given at the level of rigor generally accepted in theoretical physics. We can say that since the problem is fundamental, it must be proved rigorously. And have I found a rigorous proof. It would seem that once a rigorous proof of such a fundamental fact is given, then any journal should be glad to publish this proof. But it turned out that the publication of such a fundamental result is a big problem. Below I describe my long misadventures with the publication of this fundamental (and even the most fundamental) result.

One of the attempts to discuss my approach with mathematicians was as follows. José Manuel Rodriguez Caballero wrote to me that New York University has a FOM – Foundations of Mathematics forum. The description of his policy is as follows.

About FOM

FOM is an automated e-mail list for discussing foundations of mathematics. It is a closed, moderated list, subscriptions and postings must be approved by the moderator, currently Martin Davis. Approval of a posting does not imply agreement with the views expressed in the posting. FOM subscribers typically have advanced training in mathematics, philosophy, computer science or related fields, and either have professional activity in one of these directions or are preparing for such a career. The FOM list is intended to provide a venue for discussing the provocative, sometimes controversial, ideas which drive contemporary research in foundations of mathematics and which often do not find their way into journal articles. FOM postings must be highly relevant to issues and programs in foundations of mathematics. They should reflect high intellectual and scholarly standards. However, FOM is not a venue for papers that should be submitted to journals. Generally, detailed proofs and technical details are not welcome. Of course, pointers to more extensive accounts, *published in print or on the Web are welcome. Postings should be thoughtful, well-reasoned, and lively. Although controversy is both expected and desired, personal invective and other irrelevant discussions will not be permitted. Quotation from previous postings should be limited to what is absolutely needed for understanding, and quotations within quotations are particularly to be avoided. All postings are available in full on the archive. FOM postings must consist of single-spaced, plain text and have an informative subject line in the e-mail header. Extended quotes from other FOM postings should be avoided.*

Postings to FOM (by subscribers only) should be addressed to fom@cs.nyu.edu
The FOM Editorial Board currently consists of:
Stephen Simpson
Harvey Friedman
Martin Davis
Andreas Blass
William Tait
John Baldwin
Alasdair Urquhart

So, again, very good things are being said about how different approaches are welcome, even "the provocative, sometimes controversial, ideas which drive contemporary research in foundations of mathematics, and which often do not find their way into journal articles." These words are similar to those spoken in the editorial policy of Foundations of Physics and are also breathtaking. FOM is not a magazine, but a forum for discussing different ideas, so the FOM policy looks very attractive. Most of the FOM bosses are against standard mathematics and for finitism, i.e., the approach when there are no infinities. But, on the other hand, in the mathematics that they promote there are no operations modulo a number. This happens, for example, in Robinson arithmetic which is considered incomplete and is not used in applications. But in general, I felt that what I was trying to do should be welcomed by them. It is clear that when I found out about FOM, I immediately wanted to participate in it. But - this is not an open forum, and first you need to get the heads of the FOM to approve your participation. So, this is analogous to the arxiv moderation system.

I sent the following application to the FOM:

From Felix Lev:

I am a physicist. For many years I'm working on a quantum theory over finite math. The results are published in known physics journals. In addition, in my papers for physicists I argue that finite math is more fundamental than standard one: the latter is a special degenerated case of the former in a formal limit when the characteristic of the field or ring p in finite math goes to infinity. Since I am a physicist, I can post my mathematical results in the mathematical section of arXiv only if someone agrees to endorse, while many my papers can be found in the physics section of arxiv if you search the author F Lev.

In my last paper http://vixra.org/abs/1811.0044 I give a simple rigorous proof of the above fact. In general, introducing infinity automatically implies transition to a degenerate theory because in that case operations modulo a number are lost. So, even from the pure mathematical point of view (i.e. to say nothing about the fact that in nature there are no infinitely small and infinitely large quantities, no continuity etc.) the notion of infinity cannot be fundamental, and theories involving infinities can be only approximations of more general theories. In particular, standard quantum theory is a special degenerate case of quantum theory over finite math when $p \to \infty$*. In many cases math with infinities works with a high accuracy because at the present stage of the Universe the number p is huge. At the same time, as shown in my papers, several physics phenomena can be explained only if p is finite. In particular, in my approach gravity is a consequence of the fact that p is finite: the gravitational constant is proportional to* $1/lnp$*, i.e. gravity disappears in the formal limit* $p \to \infty$*. My estimation is that p is of the order of* $exp(10^{80})$ *but since the gravitational constant depends on lnp, the effect of finite p is observable.*

I graduated from the Moscow Institute for Physics and Technology, got a PhD from *the Institute of Theoretical and Experimental Physics in Moscow, and Dr. Sci. degree (in Russia there are two doctoral degrees) from the Institute for High Energy Physics (also known as the Serpukhov Accelerator). In Russia I worked at the Joint Institute for Nuclear Research (Dubna, Moscow Region). Now I live in LA CA and work at a software company.*

The answer to my request was:

Your request to the FOM mailing list

Subscription request has been rejected by the list moderator. The moderator gave the following reason for rejecting your request: "Your interests aren't an appropriate match for this list." Any questions or comments should be directed to the list administrator at: fom-owner@cs.nyu.edu

My response: *Dear fom-owner,*

The reason for rejecting my request is: "Your interests aren't an appropriate match for this list." This reasons seems very strange to me because in my papers I argue that finite math is fundamental while standard math is a special degenerate case of finite one. This approach seems to be fully in the spirit of the FOM forum. I would understand if, for example, the reason was that my results are erroneous etc. If the Editors think so I would be very grateful if they explain this opinion. However, the actual reason for rejecting seems very strange. Could you please tell me whether this is a collective opinion of all Editors or only one of them proposed this formulation?

Thank you. Sincerely, Felix Lev.

Reply to my answer:

Dear Felix Lev,

The decision and the wording were mine after consulting the editors. Of course, you are welcome to write them yourself. They are:

Alasdair Urquhart, urquhart@cs.toronto.edu,

John Baldwin, jbaldwin@uic.edu,

Harvey Friedman, hmflogic@gmail.com,

Steve Simpson, simpson@math.psu.edu,

John Burgess, jburgess@princeton.edu,

Andreas Blass, ablass@umich.edu,

Best wishes, Martin Davis, Moderator

My response to this letter:

Dear Professor Davis,

Thank you for your response to my query. So, in my understanding, none of the Editors found anything erroneous in my papers. Then the decision is fully unclear to me. As I noted, in my papers I argue that finite math is the most fundamental and standard math is a special degenerated case of finite one. Needless to say that this fact is fundamental for foundation of math. In my understanding, this fact is fully in the spirit of the FOM forum, and the goal of the forum is just to find strong arguments in favor of this fact. Of course, there can be different approaches in this direction but to my understanding it is just the goal of the forum to discuss different approaches.

My results are published in known journals on physics and mathematical physics and, for example, my paper in Finite Fields and Their Applications is one of the three most downloaded. So, I do not see any reasonable explanation of the Editorial decision. I will apologize if I am wrong but the only reason which comes to mind is that the Editors allow only their approaches and do not want the participants to know about other approaches.

I would be grateful if the Editorial decision is reconsidered.

Thank you. Sincerely, Felix Lev. Finally, the final rejection is:

Dear Felix Lev,

We have read your archiv article. The physics is not relevant to f.o.m. We found the mathematical claims to be unsubstantiated.

Best wishes, Martin Davis

There are two statements in this denial. First, that physics does not apply to FOM. But I use physics only for illustration, and I only claim my mathematical results. I argue that, in contrast to my previous works, where the proof of the fundamental nature of finite mathematics was given at a level more or less accepted in theoretical physics, in the new paper a purely mathematical proof is given. The second claim is that my results are unfounded. But there are no explanations; as usual, little things like scientific ethics don't bother them. But even this does not fit into the logic. After all, FOM is not a journal, but a forum, and the purpose of my work is the same as theirs - to give arguments that infinities are not needed. And, if I'm wrong, then it would seem that they should explain to all forum participants that my approach is wrong and will not lead to anything. So now I have no doubt that in my letter to them I gave the right reason: the bosses do not want the forum members to know about my approach. My approach implicitly says that their approach doesn't make much sense, so bosses don't want forum members to question whether bosses are really that great.

José Manuel Rodriguez Caballero wrote the following letter to the FOM chiefs in support of me:

Dear FOM Editors,

As a member of FOM and a young researcher with publications in important journals, e.g.,

Caballero, José Manuel Rodríguez. "On a function introduced by Erdös and Nicolas." Journal of Number Theory 194 (2019): 381-389. https://www.sciencedirect.com/science/article/pii/S0022314X18301999 Caballero, José Manuel Rodríguez. "On Kassel-Reutenauer q-analog of the sum of divisors and the ring F3 [X]/X2F3 [X]." Finite Fields and Their Applications 51 (2018): 183-190.https://www.sciencedirect.com/science/article/pii/S1071579718300169 I would like to support the possibility of subscription of Dr. Felix Lev in FOM mailing list. In my own research, motivated just by mathematics, not by physics, I studied some of Dr. Lev mathematical publications, e.g.,

Lev, Felix M. "Why is quantum physics based on complex numbers?." Finite Fields and Their Applications 12.3 (2006): 336-356. https://www.sciencedirect.com/science/article/pii/S1071579705000687

I consider that it is very important for both, the foundations of mathematics and physics, to have researchers, like Dr. Lev, who are interested in learning about the progress in both fields. I remember that it was very important for the foundations of mathematics to have mathematicians interested in both number theory on the one hand and computability theory, on the other hand, e.g., in the resolution of Hilbert's tenth problem. So, I celebrate the inclusion of researchers with diverse interests and I hope they will contribute to the solution of new open problems in the future, putting together knowledge from several seemly unrelated fields.

I consider that to be aware of the progress in non-standard models of natural numbers, a subject related to FOM, will be useful for the development of Dr. Lev's research concerning his finitist reformulation of quantum mechanics. Indeed, I remarked that some post from FOM are related to biology, e.g., Rene Vestergaard's post "Proofs of life" (see below).

Finally, the CV of Dr. Lev is superior than the CV of some of the young members of FOM, including myself.

Sincerely yours, José Manuel Rodriguez Caballero

but this letter did not help.

Another attempt to publish this work was to send it to arXiv. Because they allow me to send only to gen-ph, then in order to send to Number Theory, it was necessary that someone endorse and, at my request, Dima Logachev did it. Arxiv's response was:

Dear arXiv user,

Our moderators determined that your submission is on a topic not covered by arXiv. As a result, we have removed this submission.

While arXiv serves a variety of scientific communities, not all subjects are currently covered. Submissions that do not fit well into our current classification scheme may be removed. We encourage you to find another open access forum that serves your discipline.

So, rejected not because something is wrong, but because "your submission is on a topic not covered by arXiv"! So, the problem of whether finite mathematics is more fundamental than standard mathematics is not in the Mathematics section! It is clear that this is complete nonsense, and they simply had to find a reason to kick back, and did not find anything smarter.

ArXiv has an arXiv Math Advisory Committee, which includes renowned mathematicians. It would seem that the task of this Committee is to give advice on scientific issues. One of the members of this Committee is Professor Iosevich. I wrote to him about the attitude of arXiv to my submissions:

"Dear Professor Iosevich,

You are a member of the arXiv Math Advisory Committee, and I would be grateful for your advice on my problems with arXiv.

I have many papers published in known journals, 49 papers in arXiv, and Springer published my monograph: Finite Mathematics as the Foundation of Classical Mathematics and Quantum Theory. With Application to Gravity and Particle theory. ISBN 978-3-030-61101-9. Springer, https://www.springer.com/us/book/9783030611002 (2020). More detailed information about me can be found in my ORCID: https://orcid.org/0000-0002-4476-3080 .

My main area of research is quantum theory based on finite rings or fields. So, arXiv treated my papers as belonging to physics. Before 2009 they placed my papers in sections which I proposed. However, then their attitude to my papers significantly changed. They often required that my papers can be posted only after publications in known journals, and this, obviously, contradicts the very meaning of arXiv. However, even when a paper was published, they placed it in physics.gen-ph and several of my papers were rejected altogether. The only exception was that in 2012 they agreed to reclassify my paper published in Physical Review D to hep-th. It is obvious that quantum theory based on finite mathematics has nothing to do with general physics. I wrote many appeals but typically they were ignored.

I worked with physicists for many years and know their way of thinking. Unfortunately, when many physicists see a paper with mathematics which they don't know, they immediately conclude that this is pathology or exotics which has nothing to do with physics. Most physicists are not familiar even with the very basics of finite mathematics. This is not a drawback because everybody knows something and does not know something, and it's impossible to know everything. But typically, those physicists do not accept that in science different approaches should be allowed.

My understanding is that the goal of arXiv is to let scientists know what other scientists are doing. But in my case, the impression is that their goal is the opposite. The matter is that if a paper is posted in gen-ph then it is not allowed to cross-list the paper to other sections, and physicists and mathematicians interested in quantum theory over finite mathematics do not go to gen-ph.

Let me describe two latest examples.

My paper "Discussion of foundation of mathematics and quantum theory" published in Open Mathematics https://doi.org/10.1515/math-2022-0011 is in fact a popular description of some results of my book. I requested to post this paper in quant-ph and math.HO. However, they again posted the paper in gen-ph although it is obvious that the problems of foundation of mathematics and quantum theory have nothing to do with general physics. I wrote an appeal and after a month they informed me that they reclassified the paper to physics.gen-ph quant-ph. So, they refused to reclassify the paper to math.HO, and the paper was not posted in sections new and recent of quant-ph. Moreover, since the primary section is still gen-ph, I have no right to cross-list the paper to other sections.

Another example is the following. I had a talk "Obtaining information about nature with finite mathematics" at the international online conference organized by known universities, in particular by UCLA. The talk was published in Proceedings, MDPI, 2022, 81 (1), pp.8. 10.3390/proceedings2022081008. Nevertheless, arXiv rejected my submission with the following motivation: "Thank you for submitting your work to arXiv. We regret to inform you that arXiv's moderators have determined that your submission will not be accepted and made public; Our moderators determined that your submission does not contain sufficient original or substantive scholarly research and is not of interest to arXiv." This motivation is given without any explanation and has no hint that the moderators tried to understand the submission or were able to understand. It also contradicts their rules that the moderators are not referees. And the phrase that the submission is not of interest to arXiv contradicts scientific ethics because if the moderators do not understand the submission, it does not mean that it is not of interest for arXiv readers. Fortunately, I have no problems with the French archive HAL and here it is posted as https://hal.archives-ouvertes.fr/hal-03605174 .

In summary, the attitude of arXiv to my submissions fully contradicts scientific ethics and does not give a hint that they understand what the submissions are about.

I would be grateful if you tell me about your opinion on whether it is possible to convince the moderators that their decisions are not based on scientific criteria, and they should be reconsidered.

Thank you. Sincerely, Felix Lev."

But I didn't get any answer.

The Chairman of this Committee is Professor Kuperberg. After I didn't get a response from Professor Iosevich, I wrote to him too:

"Dear Professor Kuperberg,

You are a chair of the arXiv Math Advisory Committee. I believe that the attitude of arXiv to my submissions fully contradicts scientific ethics and does not give a hint that they understand what the submissions are about. I described my problems in a letter to Professor Iosevich but he did not respond. Please find attached my letter to Professor Iosevich. I would be very grateful for your help.

Thank you. Sincerely, Felix Lev."

but also received no response. Therefore, it is not clear why this Committee exists, what these scientists are doing in this Committee, if they do not react in any way to obvious violations of scientific ethics.

One more attempt to publish my work on foundation of mathematics was to send it to Finite Fields and Their Application. It was there for a month and the answer was:

Dear Dr. Lev,

Thank you for your submission to Finite Fields and Their Applications. Unfortunately, the Editors feel that your paper is not suitable for publication in the journal and unlikely to be favorably reviewed by the referees. We suggest you consider submitting the paper to another more appropriate journal.

Thank you for your interest in Finite Fields and Their Applications.

Sincerely, G. Mullen Editor Finite Fields and Their Applications

So, he doesn't say that the paper is off topic, but that the editors feel that way. It would seem that why talk about how editors feel: there is an editorial policy, and you just need to say whether the paper corresponds to it or not. The second part of the sentence is completely meaningless. It would seem, why not just send the paper for a review and see what the feedback will be? I think that the only explanation is this: he was afraid that suddenly the review would be positive, but he does not want to publish the paper. And it took a month to write this "thoughtful" answer.

I also had a correspondence with Harald Niederreiter, who is a famous mathematician, author of a book on finite fields and a member of the editorial board of Finite Fields and Their Applications. When I wrote to him about my paper in Dubna, he replied:

Dear Dr. Lev:

Thank you very much for your message and the link to your paper. I find your article highly interesting and the ideas enunciated therein truly original. As you can imagine, I also like a discrete and finite view of the world, with the continuous models of physics being the limit as the cardinalities of the finite models tend to infinity. It is fascinating that this can be proved rigorously!

With best regards,

Harald Niederreiter

This answer made me happy because it turned out that the famous mathematician generally supports me. He writes that he was very glad that the fundamental nature of finite mathematics could be rigorously proven. But when I wrote to him about a paper where this was rigorously proven, he replied:

Dear Dr. Lev:

Thank you for your message and the link to your paper. Actually, after studying your paper, I came to the conclusion that in the final analysis your work is really of more interest to physicists than to mathematicians. For a mathematician, the notion of infinity needs no explanation or interpretation, it is covered by cardinality theory. For a physicist, who deals with nature as it is and not with abstractions, the situation is different, and so I think your paper is better placed in the physics community.

With best regards, Harald Niederreiter

So, now he writes that for mathematicians the concept of infinity needs no explanation or interpretation; this is important only for physicists who deal with reality. So, in fact he is saying that set theory is a purely abstract science that has nothing to do with real life. This contradicts what he wrote in the first answer. Why did he write one thing first and then another?

Another attempt to publish the paper was to send it to the Forum of Mathematics, Pi. In their editorial policy they write that "Forum of Mathematics, Pi is the open access alternative to the leading generalist mathematics journals". So, they hint that they are, sort of, different from the establishment. They have a Foundations section, i.e., again, it seems that my paper is fully consistent with their rules. There you need to suggest an editor responsible for the paper, and I suggested Terence Tao. He is a star in mathematics and has many awards. And I got this response:

Dear Professor Lev,

This message concerns the manuscript A Simple Proof That Finite Mathematics Is More Fundamental Than Classical One by Felix Lev submitted to Forum of Mathematics, Pi. Unfortunately, we are uable to accept it for publication.

Sincerely,

Terence Tao

Apparently, he wanted to kick back quickly and did not even check if there were errors in the answer. And, as in the previous examples, the fact that he is a great scientist does not mean at all that he bothers to observe at least the minimum rules of scientific ethics. He doesn't even write if the paper complies with the journal's rules, and he doesn't make any attempt to explain why "we are uable to accept it for publication". Again, it seems like the role of the journals is not just to answer whether they take a paper or not. It seems to be assumed that when an author submits a paper to a journal, he wants to know not only whether the paper will be accepted or not, but also the opinion of qualified scientists. But, as in the previous examples, this is not in sight.

The next attempt was to send the paper to the journal Fundamenta Mathematicae, which is published by the Mathematical Institute of the Polish Academy of Sciences. One of the topics of the journal is Mathematical Logic and Foundations of Mathematics, i.e., again, it would seem that my paper is fully consistent with the theme of the magazine. According to the rules of the journal, you can send a paper to one of the members of the editorial board. The editor-in-chief of the journal is the President of the Polish Academy of Sciences Stefan Jackowski. I decided to send it to him because he started his career as a physicist, and my arguments come from physics (but the proof is strictly mathematical). So I hoped that he might be interested. At the end of the letter, I wrote in Polish that my parents until 1939 were Poles, so I understand Polish and I can answer in Polish.

He answered in Polish, that it was very good, that I had not forgotten Polish and that it was possible to correspond in Polish. But since he is not an expert in this, he forwarded the paper to the secretary of the journal Henryk Toruńczyk. I waited a month and finally asked him in what condition the paper is, probably, under review? But no answer. I again ask about the paper and again no answer. I have never met such disgusting, that the editors did not answer the author at all. So I wrote to him:

Dear Professor Toruńczyk,

On Jan 13th I sent to Fundamenta Mathematicae my paper titled "A simple proof that finite mathematics is more fundamental than classical one". However, I still have no information on the status of the paper and even have no idea what's going on with the paper. On Feb 14th and 18th I asked you about the status but no response has been received. Such an attitude to the author is disgraceful regardless of your opinion about the paper. I withdraw my paper from Fundamenta Mathematicae.

Felix Lev, Feb 20th, 2019.

and wrote in Polish:

P.S. Moi rodzice lubili przyslowie: "Pieniedzy nie mam, ale honor mam". Nie wiem o pieniadzach, ale v takim redakcyjnym stosunku do autora oczywiscie nie ma honoru.

The translation is this:

My parents loved the saying: I have no money, but I have honor. I don't know about money, but there is obviously no honor in such an editorial attitude towards the author.

The next attempt was the Russian journal Theoretical and Mathematical Physics and almost immediately I received the following answer (and the entire correspondence was in Russian):

Dear Felix Meilakhovich, Your paper "A simple proof that finite mathematics is more fundamental than classical" does not correspond to the theme of the TMF magazine.

Rep. TMF secretary V.V. Zharinov

I answered almost immediately:

Dear Professor Zharinov,

The assertion that my paper does not correspond to the subject of TMF seems unfounded to me. As I note in the cover letter, it corresponds to the TMF clause "Articles report on current developments in theoretical physics as well as related mathematical problems." And from an informal point of view, as noted in the abstract and cover letter, the main (and fundamental) result is that quantum theory over a finite ring or field is more general than standard quantum theory. The fact that finite mathematics is more general than classical is a consequence of the main result (and this is explained in detail).

Therefore, if the name gives the wrong impression, then it can be changed (although I explain in detail the meaning of the name and note that it complies with the TMF regulation). I also note that on this topic I have an article in the TMF, to which I refer (volume 138, pp. 208-225, 2004). Therefore, I would be grateful if the decision to reject the paper was reviewed.

Sincerely, F. M. Lev.

But since there was no answer, then three days later I sent the following letter:

Dear Viktor Viktorovich,

In addition to my reply to your letter dated 02.21: As I note, the article meets the requirement: "Articles report on current developments in theoretical physics as well as related mathematical problems." There is even a phrase in the paper that the question of which mathematics is more fundamental is a question of physics, not mathematics. The fact that finite mathematics is more fundamental than classical is just "related mathematical problems". However, I understand that the physicist reader may immediately decide that the paper is purely mathematical. If the title of the paper is changed to "A simple proof that finite quantum theory and finite mathematics are more fundamental than standard quantum theory and classical mathematics, respectively", then, probably, there will be no doubt that the paper is in the subject of TMF. Such a change does not require any change to the body text. From a purely formal point of view, such a title is unnecessarily long, since if finite mathematics is more fundamental, then quantum theory over finite mathematics is automatically more fundamental than standard quantum theory. Nevertheless, in this case, the reader-physicist will probably no longer have the impression that the paper is purely mathematical.

If you think that the new title does not contradict the subject of TMF, then I can re-upload the paper with a new title. I would be grateful if my request for a review of the decision on the paper would be reconsidered.

Sincerely, Felix Lev.

But since again there was no answer, I wrote a new letter:

Dear Viktor Viktorovich,

Please let me know the status of the review of my article. The paper is not in the TMF database. Will my request for a review be considered?

Thank you. Sincerely, Felix Lev.

and after 13 minutes received this answer:

Dear Professor Lev

RK TMF reviewed your letters dated 2019-02-21 and 2019-02-25 and remained convinced to reject the paper on the subject.

V.V. Zharinov

So, even quoting the TMF rules, I tried to explain to V.V. Zharinov that the paper is completely on the subject of the journal. But he didn't stoop to tell me where I was wrong. In his first reply, V.V. Zharinov did not even write that this was the opinion of the editorial office, and it could hardly have been since the first response was received 4 hours after the submission of the paper. So with a probability of almost 100%, this is just Zharinov's opinion. And after my several questions, the final answer, received after 13 minutes, says that this is the decision of the editorial office. But then it is not clear why this decision was not written to me earlier, but only in response to my requests. So, it is very likely that there was no meeting of the editorial office on my paper, and all this is only the decision of V.V. Zharinov.

When I was rejected in other journals, the editor-in-chief nevertheless stooped to signing the decision of the editorial board. So, the decision to reject the paper was made only by the responsible secretary. Whatever he was a great scientist, but this is out of the question.

The next attempt was the Journal of Mathematical Physics and received this answer:

Dear Dr. Lev,

I regret to inform you that we do not find your manuscript 19-0323, "A Simple Proof That Finite Quantum Theory And Finite Mathematics Are More Fundamental Than Standard Quantum Theory And Classical Mathematics, Respectively," suitable for publication in the Journal of Mathematical Physics. The Associate Editor's comments are enclosed.

Sincerely, Jan Philip Solovej

Editor...

Editor Decision: Reject without review

Associate Editor Decision: Reject without review

Associate Editor Comments to the Author:

The paper is not of sufficient mathematical quality to warrant publication in Journal of Mathematical Physics.

And it took 23 days to write such a "thoughtful" answer. That is, it is said that the quality of mathematics is insufficient for publication in such a great journal, but again they do not stoop to explain why. Of course, I sent an appeal:

Manuscript 19-0323 "A Simple Proof That Finite Quantum Theory And Finite Mathematics Are More Fundamental Than Standard Quantum Theory And Classical Mathematics, Respectively" by Felix Lev

Author's appeal on editorial decision

The only reason for rejection is the statement of Associate Editor that "The paper is not of sufficient mathematical quality to warrant publication in Journal of Mathematical Physics". If this is the real reason then it is not clear why it took 23 days to inform me about this decision. Such an attitude to the author fully contradicts scientific ethics.

The decision contradicts scientific ethics also from the formal point of view. The decision does not say that my paper is out of the scope of JMP. The editorial policy of JMP states that "Specifically, the articles focus on areas of research that illustrate the application of mathematics to problems in physics", and my paper fully satisfies this requirement. In science it is treated as fully unacceptable when official negative conclusions about a paper are given without any substantiation. If such a practice is acceptable for JMP then I believe that it would be at least fair to say in the editorial policy that the editors are not obliged to substantiate their decisions even on papers which are in the scope of JMP. In that case scientists sending papers to JMP will be aware that their papers can be rejected without any substantiation. The way how my paper was treated also fully contradicts Prof. Solovej's statement that JMP "...should publish high-quality papers of interest to both mathematics and physics, and this criterion should be applied vigorously in the review of papers". In my case the paper even has not been sent for a review.

With such an attitude as in the given case, any scientific discussions become meaningless because if the editor does not like a paper or is not qualified to understand it then he can claim that "the paper is not of sufficient mathematical quality". I have papers in many known journals (Annals Phys., Finite Fields and Their Applications, J. Phys. A: Theoretical and Mathematical, Nucl. Phys. A., Phys. Lett., Phys. Rev. C and D, Phys. Rev. Lett., Theor. Math. Phys. and others). In particular I have two rather long papers published in JMP when the editor was Prof. Biedenharn and those papers are done in the framework of the approach used in the present paper. Typically, my papers were accepted, in some cases they were rejected but this is for the first time when the "motivation" for rejection was that "the paper is not of sufficient mathematical quality". With this "motivation" the editor in fact says that my mathematical level is low, his level is high and he considers it beneath his dignity to explain why he thinks so. Meanwhile his approach to the mathematical level in JMP is very important in view of the following. One of the requirements of the JMP editorial policy is

1) JMP welcomes original research of the highest quality in all active areas of mathematical physics.

Probably highest quality implies that the level of mathematics in JMP papers also should be of highest quality, right? At the same time, the policy also contains the requirement that

2) The mathematics featured in the articles are written so that theoretical physicists can understand them.

and it is not clear whether 1) and 2) are mutually consistent. For example, in my paper I prove that quantum theory based on finite mathematics is more fundamental than standard quantum theory. Needless to say that this problem is fundamental and is in the scope of JMP. However, the absolute majority of physicists do not have even very basic knowledge in finite mathematics. I tried to satisfy 2) as much as possible. As I note in my cover letter, I believe that to understand my results the reader should only understand the meaning of operations modulo a number. Since there is no doubt that the problem discussed in my paper is fundamental and is in the scope of JMP, I believe that the paper should be judged not from the point of view whether somebody likes my results or not and whether my mathematical level is low or not but whether the proofs of my statements are mathematically correct or not. I would appreciate it if the editorial decision were reconsidered and editors inform me about the opinion of qualified experts on my paper.

And got this answer: *Dear Dr. Lev, We regret to inform you that your request to appeal the decision on the manuscript cited above has been declined. We are sorry that you find that your paper was not treated properly. The time it took to reach the decision may seem long, but, in fact, the paper has to go through several quality controls and editors have many papers to review. We believe it is beneficial for the whole review process to decide as fast as possible and that will often mean to decide without involving a referee. I have looked at your paper again and I agree with the decision that the mathematical level does not seem appropriate for Journal of Mathematical Physics. It is certainly not enough that the statements are correct. Your paper seems better suited for a journal addressing fundamental issues of physics.*

Sincerely, Jan Philip Solovej
Editor Journal of Mathematical Physics

He writes that he agrees with the original decision that "...the mathematical level does not seem appropriate for the Journal of Mathematical Physics." But after all, the original decision was: "The paper is not of sufficient mathematical quality" and it is obvious that these two sentences have completely different meanings. So, he plays dumb. But what he writes next shows his way of thinking. In my appeal, I write that because my main result (that the standard quantum theory is a special degenerate case of FQT) is certainly fundamental, the main question is whether my proof is correct. So, it would seem that judging a paper is simple: say the correct proof or not. But he writes that this is not enough. What then is enough? As I wrote above, Rovelli of Foundations of Physics also wrote that correct mathematics is not enough. When a physicist writes this, it's all right. But here this is written by a mathematician, editor-in-chief of the Journal of Mathematical Physics. And then he writes that the paper is better suited to the journal "addressing fundamental issues of physics". So, it turns out that such a seemingly fundamental journal does not really deal with fundamental issues. And then what does it do or what should it do? He, like many physicists and mathematicians, has such a way of thinking that foundations are some kind of philosophy (or rather chatter), and a normal physicist or mathematician should not deal with this, but solve some specific problems.

With such a way of thinking, it is clear that no work that does something out of the ordinary stands a chance, whether it is correct or not. And it is not clear from what considerations it is decided whether a paper is suitable or not. So, everything is as usual: what is proclaimed in the editorial policy does not matter, and the main thing is whether the editor will like the paper or not.

Another attempt - Journal of Physics A. Here I understood that with a probability of 99.99% there was no chance. As I wrote above, it used to be a very decent journal, I had 5 papers in it and the reviews of the reviewers were very decent. But then the journal changed, became stupid and rejected all my non-standard papers even without a review. And I estimated these 0.01% chances based on the fact that all of a sudden, they are not completely stupid and still understand that the work is fundamental. But their answer dispelled these illusions:

Dear Dr Lev, ... To be publishable in this journal, articles must be of high quality and scientific interest, and be recognized as an important contribution to the literature. Your Letter has been assessed and has been found not to meet these criteria. It therefore does not warrant publication in Journal of Physics A: Mathematical and Theoretical and has been withdrawn from consideration. ...

Yours sincerely,
Eimear O'Callaghan
On behalf of the IOP peer-review team...

So, everything follows the same pattern: they don't say that the paper is off topic, but just a low level and don't go down to explain why.

Another attempt is the journal Foundations of Science. The editorial policy of the journal is:

Foundations of Science focuses on significant methodological and philosophical topics concerning the structure and the growth of science. It serves as a forum for exchange of views and ideas among working scientists and theorists of science, and promotes interdisciplinary cooperation. The journal presents foundational issues of science in a way that is free from unnecessary technicalities, yet faithful to the scientific content. Its aim is not simply to identify and highlight foundational issues and problems, but to suggest constructive solutions. While acknowledging that various sciences have their own approaches and methods, the editors hold that important truths can be discovered about and by the sciences and that these transcend cultural and political contexts. The editors believe that the central foundational questions of contemporary science can be posed and answered without recourse to sociological or historical methods.

My article is 200% suitable for these requirements. However, I got this response:

Dear Dr. Lev,

We received your manuscript entitled

"A Simple Proof That Finite Mathematics Is More Fundamental Than Classical One"

Many thanks for considering Foundations of Science for your submission. We had a look at your article and it seems to us that your manuscript covers themes and technical results that make it more suitable to be submitted to a more specialised journal. As you can imagine, due to the highly interdisciplinary character of our journal, we receive in Foundations of Science a huge amount of submissions, and we have to carefully select those contributions whose content strictly fits the scopes and objectives of the journal - you can have a look at the webpage http://www.springer.com/philosophy

In this case, your manuscript falls outside the scopes of the journal. This is why we regret informing you that we cannot consider your manuscript for publication in Foundations of Science. We wish you all the best exploring other venues of publication in more specialised journals.

Best regards,

Sandro Sozzo

Managing Editor of Foundations of Science

Diederik Aerts

Editor-in-Chief of Foundations of Science

What does not fit under the editorial policy is a complete lie. And again the question arises: why not send it to the reviewers, let them give a conclusion. But again, the editor himself, who, judging by his work, has never dealt with finite mathematics, decides that the paper is not suitable. And so the question again arises: does he understand what the paper is about?

The next attempt is Quantum Studies: Mathematics and Foundations. Even the words quantum, mathematics, and again, as in the previous case, foundations immediately say that my paper is just about this, and the editorial policy confirms this. Chief editor: Yakir Aharonov. He is a renowned physicist and perhaps his most famous achievement is the Aharonov-Bohm effect. The first work on this topic was published in 1959 when he was 26. In the Wikipedia photo, he is cool: smoking a pipe in front of a chessboard with a chess clock, but the general public does not know at what level he plays. One can understand that let's say, he is analyzing the game. But the watch is clearly for coolness. He is not playing with anyone in the photo. Among his many titles is President of the IYAR, The Israeli Institute for Advanced Research. He and his co-authors discuss non-linear effects and paradoxes in quantum theory. So, he seems to be really interested in foundations. One of his places of work is Chapman University, where he teaches seminars on quantum theory. When I wrote to him and his people about my blurring paradox and asked for a seminar, it is clear that I received no response. Given his age and the fact that he has many collaborators, it is not clear if he plays an active role in the journal or just a wedding general. It seems that the second is true, since I got this response:

Dear Dr Lev,

I have read your manuscript, QSMF-D-19-00046 "A Simple Proof That Finite Quantum Theory And Finite Mathematics Are More Fundamental Than Standard Quantum Theory And Classical Mathematics, Respectively"

With regret, I must inform you that I have decided that your paper cannot be accepted for publication in Quantum Studies: Mathematics and Foundations. I would like to thank you very much for forwarding your manuscript to us for consideration and wish you every success in finding an alternative place of publication.

With kind regards,

Fabrizio Colombo, PhD

Managing Editor Quantum Studies: Mathematics and Foundations.

So, he paper was read by the Managing Editor and he decided that it should not be published. So it seems that the great Aharonov did not even see this paper. Other great editors in chief, such as ‘t Hooft and Wilczek, have at least one eye on the paper they receive. Fabrizio Colombo is a standard continuous mathematician. From his papers it is not even clear whether he understands quantum theory, and even more so whether he can judge the ultimate quantum theory. And he is one of Akharonov's co-authors, for example, in the paper "Some mathematical properties of superoscillations", Aharonov, Y, Colombo, F, Sabadini, I, Struppa, J. Phys. A 44, 365304. And such a person decides whether to take papers in Quantum Studies: Mathematics and Foundations. Does he understand that he is acting dishonestly? And the usual story is that there is no explanation, i.e., scientific ethics is not for him. I immediately replied:

"Dear Professor Colombo,

Could you please tell me why you decided so. The journal name contains the words quantum, mathematics, foundations and the paper is fully in the scope of QSMF, right? Did you find any errors? Do you think that the results are of no interest for physics and math? Do you have at least very basic knowledge in finite mathematics? Thank you, Felix Lev.", but it is clear that he did not stoop to an answer.

Another try: Physics Open. There is a Quantum Physics section in it, one of the topics is Foundations. It would seem obvious that my paper is just right. But got this response:

Dear Dr. Lev,

Thank you for submitting your manuscript for publication in Physics Open. After an initial evaluation of your work, I regret to inform you that we are unable to accept your manuscript for publication. Unfortunately, the content of your submission is inadequate to be considered for publication in this journal, as it fails to meet the standards of high quality and credibility of Physics Open. Please refer to the authors' basic guidelines and best practices recommended by Elsevier should you consider submitting you work to an Elsevier journal in the future:

https://www.elsevier.com/journals/physics-open/2666-0326/guide-for-authors

With my best regards, Gabrielele Panarelli, PhD

Although this Panarelli is already a PhD, he does not have any papers on quantum theory and on finite mathematics. But he concludes that the paper is low-level, since it "fails to meet the standards of high quality and credibility". It is not clear in the answer who "we" is and why the editor himself did not stoop to the answer. One of the editors, M. Mishchenko, is a MIPT graduate and now works at NASA. Of course, I wrote to him that the answer again did not correspond to scientific ethics. But again, the same story that they kicked me off with some words, he is not going to communicate anymore, and the notion that the author has the right to appeal is not for him.

Another try: Scientific Reports. The journal is highly advertised, it seems to have the largest circulation of all scientific journals, a high impact factor, etc. Very good words are spoken in the editorial policy: "Scientific Reports publishes original research in all areas of the natural and clinical sciences. We believe that if your research is scientifically valid and technically sound then it deserves to be published and made accessible to the research community. . . . Referees and Editorial Board Members will determine whether a paper is scientifically valid, rather than making judgments on significance or whether the submission represents a conceptual advance." That is, it would seem that everything is simple: if the results are new and correct, then the paper should be accepted without subjective judgments about potential importance. But got this response:

Dear Felix Lev,

Many thanks for submitting the manuscript "A Simple Proof That Finite Quantum Theory And Finite Mathematics Are More Fundamental Than Standard Quantum Theory And Classical Mathematics, Respectively" to Scientific Reports. However, we regret that as the subject of your study is beyond the scope of the journal we cannot consider it for publication as pure mathematics is beyond our scope. Scientific Reports publishes original research from all areas of the natural and clinical sciences. I apologize that this was not discovered earlier during the quality control stage.

Thank you for the opportunity to consider your work. I am sorry that we cannot be more positive on this occasion and hope you will not be deterred from submitting future work to Scientific Reports.

Best regards,
Mark Daly
Editorial Board Member
Scientific Reports

This Daly is a member of the Editorial Board, and the journal reports the following about him:

Mark joined Scientific Reports in November 2017 after an undergraduate degree in physics at the University College Cork, Ireland, and PhD at the Okinawa Institute of Science and Technology, Japan. His interests lie in optical manipulation and structured light.

So, he already has a PhD. And with such a “high” level, he thinks that my paper is on pure mathematics (and the journal has sections on Mathematical Physics, Quantum Physics, etc.). So, again, everything is as usual, that the question of a fundamental paper that changes the standard paradigms in physics and mathematics is decided by those who have no idea about fundamental physics and mathematics. In the instructions for authors, they write that the author must suggest which of the Editorial Board can be the handling editor. I suggested Igor Yurkevich, who works at Aston University, but they didn’t even send him. When I wrote to him about it, he replied. The answer is very interesting because it shows the opinion of a physicist who is in these circles:

I am pretty sure that they even did not bother to send abstract to anyone. Usually such an invitation to handle a submission takes a week or so until someone picks it up. If not, they send another ’chasing up’ invitation to other Editorial Board members. Quick response means that some technical clerk read and decided not to proceed with formalities. This is now standard procedure in Nature and Science publishing journals. Someone’s taste decides everything - era of scarce resources!

So, a highly qualified physicist understands that something is not right in the Danish kingdom, but little depends on highly qualified physicists.

Another attempt is the journal Mathematics, which seems to be not highly rated and often takes the work of authors who are not in establishment. Unlike the so-called prestigious journals, here the refusal was motivated by reviews:

Review 1: The Autor of submitted manuscript believes that he presents and proofs fundamental ideas concerning general relations between the quantum theory and foundations of mathematics. He refers to the previously published papers (authored by him) and to results presented there. Some of those articles can be found in the references. Those papers were devoted to the finite mathematics and proposed by the Author "finite quantum theory". Unfortunately, it is difficult to judge positively the submitted manuscript. The Author presents in the first part of the paper some historical and philosophical considerations and remarks. Next, the Author presents proposed by him a statement and mathematical proof and discussion (Section 3), and finally, he goes back to the discussion of rather philosophical nature. Concerning the more mathematical part of the manuscript, it contains the figure and some considerations which have already been published by the Author in Physics of Particles and Nuclei Letters 14:77 (2017) – this article was not mentioned in the list of references. Concerning the latter, one can find there only the references to four book devoted to the philosophy and those concerning the papers which were written by the Author. In my opinion, the manuscript does not fit the topics of the Mathematics journal. Maybe it will be more suitable for publishing in one of the journals devoted to the philosophy. Moreover, the form of the paper does not meet the standards of the journal. Thus, I can conclude that the article should be rejected.

Review 2: *This is another paper of the Author dealing with a kind of modular arithmetic and its purported application in physics. The method is motivated by the verification principle. According to Wikipedia (I am not a philosopher and therefore have to rely on external sources) , "Verificationism, also known as the verification principle or the verifiability criterion of meaning, is the philosophical doctrine that only statements that are empirically verifiable (i.e. verifiable through the senses) are cognitively meaningful, or else they are truths of logic (tautologies)". This is in contrast* *to the Author's definition "A proposition is only cognitively meaningful if it can be definitively and conclusively determined to be either true or false (see e.g. Refs. [1])". — the term empirical is missing. Then the Author continues "Popper proposed the concept of falsicationism [3]: If no cases where a claim is false can be found, then the hypothesis is accepted as provisionally true". — I am afraid I am not able to see the relevance of this discussion to the mathematical content. I also find it trivial to demand that "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)". This is a rather general principle for physical theories; not only quantum mechanics. I am afraid that the main statement of the paper is almost trivial: "Main Statement: Even classical mathematics itself is a special degenerated case of finite mathematics in the formal limit when the characteristic of the field or ring in the latter goes to infinity". Is that not done in analysis all the time? The Author also misses out the metamathematical debates on *) constructive mathematics; as exposed, e.g., in ...* and then comes the bibliography.

A phrase from the second review "Is that not done in analysis all the time?" shows that the reviewer does not even understand what is at stake. But the first reviewer seems to understand that the problem is fundamental. In such a case, it would seem that he should simply say whether the results are new and correct. But he does not specifically consider the results, but says that the paper is more suitable for a philosophical journal.

I sent a question to the journal secretary Mr Zhang:

Thank you for your email informing about the editorial decision. The referee reports seem to be strange for the following reasons. The referees say that my results are not for a math journal but they do not judge a paper as a math paper. My paper contains new mathematical statements and in my understanding the main goal of the reports is to say whether the proofs are correct and new. However, the reports say nothing specific about this. The referees discuss philosophy, advise me to consider constructive math, finitism etc. but nothing specific is said about my results and one of the referees even says a strange phrase that "Is that not done in analysis all the time?". As noted in the paper, philosophy is discussed only for illustration while the results are mathematical and do not depend on philosophy.

Let me also note that the special issue is titled "Mathematical Physics II" and I prove that standard quantum theory is a degenerated special case of FQT. This is a fully new result, but the referees even do not mention this result. From the reports it is not clear to me whether the referees treat their recommendation as final or they accept that the author has a right to appeal. If I have a chance to appeal, I could try to submit a revised version. Is this acceptable? But to be honest I am puzzled because the reports say nothing specific on whether my results are correct or not. I can include the discussion of constructive math, finitism etc., but the main problem is whether the referees agree that my results are correct and new. Unfortunately, I could not find a clear explanation of this point. I would be grateful for your explanation.

The editor of this special issue "Mathematical Physics II" is Dr. Enrico de Micheli. Mr Zhang replied: The final decision was made by our academic editor according to his opinion along with the collected reports during the peer review. Hope your gentle understanding. We would like to thank you for having considered Mathematics and wish you every success in the future. So, the editor made the final decision without even giving the author the right to appeal.

Above, I wrote about my attempt to publish a paper on the problem of time in the Journal of Physics Communications. After that, I decided that it was pointless to deal with them. But I received a standard letter from them (which, for sure, was sent to many) with an invitation to send a paper to the journal. I told them that after my first experience I did not plan to send them any more. But if the editors send me an official invitation to the paper https://hal.archives-ouvertes.fr/hal-02262153 in HAL, then I will send this paper. And got this response:

Dear Dr Lev,

Thank you very much for your recent message. We are very sorry to hear about your unfavourable experience with your previous submission to Journal of Physics Communications

(JPCO).

With regards to your Article submitted in April-2018, we can see that an Editorial Board member was approached to expedite the review process as a result of difficulty obtaining reviewer reports of a high standard however, none were available to do so. This is a rare occurrence and we apologise for any inconvenience the delayed and ultimately withdrawn article caused. In response to your latter query, we'd be glad to consider your new paper and will strive to obtain quality and efficient reviews to report on the manuscript. Our Editor has viewed the article via the link you provided and advised that it would be a great fit for JPCO. We hope this helps. If there is anything we can assist you with, please don't hesitate to contact us.

Kind regards,

Isabella Formisano & Blythe Rowley

Editorial Assistants

So, now they give a new explanation for why they rejected my paper in April 2018: because they could not find a reviewer. And then they swear that they will do everything to get a qualified review of a new paper, that the Editor looked at it and decided that it would be a great fit for the journal. It would seem that after such an answer, there is hope that the paper will be considered on its merits. But after I sent the paper, I immediately received a standard response:

Dear Dr Lev,

Re: "Why Finite Mathematics Is More Fundamental Than Classical One" by Lev, Felix Article reference: JPCO-101317

Thank you for your submission to Journal of Physics Communications.

To be publishable in this journal, articles must be of high scientific quality and be recognised as making a positive contribution to the literature.

Your Paper has been assessed and has been found not to meet these criteria. It therefore does not warrant publication in Journal of Physics Communications and has been withdrawn from consideration.

We are sorry that we cannot respond more positively and wish you luck in publishing your article elsewhere.

Yours sincerely

Sarah Hunter

My response to this letter was:

Dear Editors,

After my first experience with JPCO I did not plan to submit new papers. However, in response to Dr. Messaritaki's invitation I wrote that will submit my paper https://hal.archives-ouvertes.fr/hal-02262153 only if I receive an official invitation to submit this particular paper. In your response of Sep 18th you wrote "In response to your latter query, we'd be glad to consider your new paper and will strive to obtain quality and efficient reviews to report on the manuscript. Our Editor has viewed the article via the link you provided and advised that it would be a great fit for JPCO". However, when I submitted this paper, I immediately received a rejection letter. Such an attitude to the author is obviously indecent.

Sincerely, Felix Lev.

So, I openly wrote to them that such an attitude towards the author is indecent. It seems that after that they should be offended and not have anything to do with me. But got this response:

Dear Dr Lev,

Re: "Why Finite Mathematics Is More Fundamental Than Classical One" by Lev, Felix

Article reference: JPCO-101317

Thank you for your email. We apologise that your paper was rejected. Due to a miscommunication, we were not aware your paper had been commissioned. I have now consulted the Editor and we are happy to reconsider your manuscript and continue processing it in JPCO. Please could you let us know if you are happy for us to continue processing your paper in this journal?

Yours sincerely

Sarah Hunter

So, they apologize that there was a miscommunication (i.e., the left hand did not know what the right hand was doing), they reconsider their decision, they decided to review my paper again and ask me to let them know if I am happy. So, I sent them a paper, wrote that I was ready to pay 1495 dollars for open access, but they still rejected it, and now they want to review the paper again. But I decided that after that it would be too much if I tried again to pay 1495 dollars: because of their stupid dogmas, at first, they refused, and now they seem to be willing to receive this money again. So, I wrote them this answer:

Letter reference: HAA01

Dear Editors,

I am grateful that you have reconsidered your decision to immediately reject my paper. I thought about your request to confirm that I agree if you continue processing my paper. However, my decision is that I will not try to publish my paper in JPCO. I don't know whether or not you are interested in my reasons, but they are described below.

First about me. I graduated from the Moscow Institute for Physics and Technology, got PhD from the Institute of Theoretical and Experimental Physics in Moscow and Dr. Sci. Degree (in Russia there are two doctoral degrees) from the Institute for High Energy Physics also known as the Serpukhov Accelerator. In Russia I worked as a leading scientist at the Joint Institute for Nuclear Research (Dubna, Moscow region) but in the US I work at a software company.

I gave talks at many international conferences, have many papers published in known journals on physics and mathematical physics (Annals Phys., Finite Fields and Applications, J. Math. Phys., J. Phys. A: Mathematical and Theoretical, Nucl. Phys. A, Phys. Lett., Physics of Particles and Nuclei, Phys. Rev. C and D, Phys. Rev. Letters, Theor. Math. Phys. and others) and 44 papers in arXiv.

My experience is that when I sent to known journals papers done in the framework of more or less mainstream approaches then typically such papers were accepted without problems. However, when a paper was based on non-mainstream approaches then great problems arose. This was not because the editors could say something specific or refute my results. Typically, they even did not understand what the paper was about but they saw that the paper was not based on what was sacred for them.

The editorial policies of known journals are typically very impressive. However, when I dealt with those journals then typically it became obvious that the referees and board members did not feel obliged to follow those policies, they thought that they know better what papers should or should not be published and the referees often even did not understand that it was disgraceful to write a negative report if they understood nothing in the paper.

For example, I have five papers published in JPA, the last of them was published in 2004. All those papers have been done in frameworks of more or less mainstream approaches. The referee reports were very professional and helped to improve the papers. For example, in the last case there were two referee reports, positive and negative, the adjudicator advised in my favor, and this is a reasonable situation. However, all my next submissions to JPA have been rejected without any explanations, and nobody tried to understand my results. Typically, they sent me the same standard text as you sent on Sep 26th that "...articles must be of high scientific quality and be recognised as making a positive contribution to the literature. Your Paper has been assessed and has been found not to meet these criteria." So, in fact the statement is that my paper is not of high scientific quality and does not make a positive contribution to the literature. Scientific ethics implies that any negative statement should be substantiated but in all those cases no explanations have been given, and the phrase that the paper has been assessed gives no info on how it has been assessed.

For me it's interesting whether the editors understand that their actions contradict scientific ethics. I propose papers where quantum theory is based not on complex numbers but on finite math. I explain that my approach is more fundamental than standard one and moreover I have *rigorously proved that standard quantum theory is a special degenerate case of quantum theory based on standard math. I have no doubt that my papers are fundamental and sooner or later (rather later than sooner) this will be acknowledged. My observation is that the 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. I believe the mentality of physicists should be such that in physics different approaches should have a right to compete. However, the mentality of many physicists is such that if they don't understand something then this should not be published. My observation is that when physicists see that my papers are based on finite math then they immediately conclude that this is philosophy, pathology, exotics etc. and contradicts their dogmas (although, as I noted, typically they do not have even very basic knowledge in finite math).*

When JPCO was created I was impressed by its editorial policy. The policy says that JPCO differs from other journals, that it "does not make a subjective assessment on the potential future significance of a paper, instead providing a rapid platform for communicating research that meets high standards of scientific rigour and contributes to the development of knowledge in physics". However, my experience with two papers shows that at least in my case the editors do not feel obliged to follow the editorial policy. They do not understand that my papers give FUNDAMENTAL contributions to the knowledge in physics. Therefore, the papers not only fully satisfy the JPCO policy but should be welcome by the editors. The most plausible explanation of such a situation is that when they see the words "finite mathematics" then their intention is to reject the paper right away and probably for them a strong argument in favor of their belief is that I am not from a university. It seems to me that the mentality of all editors should be such that they should welcome nonstandard approaches because this will make their journals more attractive. Especially, in view of the JPCO policy, this should be the case for the editors of JPCO. However, I see that the editors of JPCO have the same mentality as the editors of many other journals and if a submitted paper is not in mainstream then the paper has no chances to be published.

You acknowledged that the treatment of my paper was not fair because it had been commissioned. However, even if it had not been commissioned your response contradicts your policy and scientific ethics. So the fact that you have reconsidered your decision does not mean that mentality of the editors has been changed. In view of this situation, I think that if I agree that you continue processing my paper then the most probable scenario is the following. Probably you will not find referees who have even very basic knowledge in finite math and the mentality of majority of physicists is that if they do not understand something (e.g. if the words "finite mathematics" contradict their dogmas) then probably they will write a meaningless referee report with the advice to reject the paper. They will not care that their treatment of the paper contradicts the editorial policy and scientific ethics. In view of my experience, for editors this will be a good pretext to reject the paper. According to your policy, the authors have a right to appeal the decision. However, my experience with the first paper shows that all my arguments that the reports contradict the editorial policy and scientific ethics will not be taken into account and the appeal will not be considered. Since I am not young and do not want to have additional negative emotions, I have decided not to try to publish my paper in JPCO.

Sincerely, Felix Lev.

Next try: Taiwanese Journal of Mathematics. Their response was this:

We do not have a full referee report, but quick opinions gathered suggest that it would be difficult to convince the editorial board to accept the article. Therefore, rather than begin a refereeing process that could take months, I am returning your manuscript to you now so that you have the chance to submit it elsewhere without delay.

I will not comment on this answer, but, in any case, they answered after two days and thanks for that.

Another try: Notre Dame Journal of Formal Logic. There was no answer for more than a month and I asked about the status:

Dear Professor Pillay, The status of my paper is "With Editor" from November 6th, i.e. the paper is with the Editor for more than a month. I understand that the Editor is very busy. On the other hand, the paper is short (10 pages) and in my understanding rather simple. My understanding is that the paper is under review, right? Could you please tell me when (even very approximately) the referee reports are expected.

Thank you. Sincerely, Felix Lev.

So, naively, I thought that since an editor was appointed, who keeps the paper for more than a month, then the paper is under review. But within less than an hour I received the following responses from the editor-in-chief:

Dear Felix,

The Editor-in-Chief on the philosophy side (Mic Detlefsen) died in October. We have appointed a replacement who will take over his papers. There are papers submitted in July which have not been dealt with yet.

Anand Pillay (Editor-in-Chief)

Dear Felix,

Actually I took the opportunity to look at your paper myself, and I can say quickly that it is not suitable for the Notre Dame Journal. The statement about Z and the Z/pZ (i.e. F_p) is obvious. (Also if you are interested there is a big literature about "pseudofinite" structures in logic. Easily found on google ..) So I will reject the paper.

Regards,

Anand Pillay

Dear Felix,

As I said in the email to you I am rejecting the paper. Sorry.

Anand Pillay

Editor-in-Chief

Notre Dame Journal of Formal Logic

So, in the first answer, he seems to justify that because the former editor-in-chief in charge of philosophy has died, many papers are being delayed. Firstly, it is not clear why he decided that my paper refers to philosophy. And it is not clear if the status was "With Editor", then which editor was sent. After all, obviously not the one who died, and then it is not clear why he justifies. But the second answer came 40 minutes later. So, in those 40 minutes, he looked at the paper and decided to reject it. From his answer it is clear that no one has looked at the paper before, despite the status "With Editor". And the third answer came in 5 minutes.

From his answer it is clear that he did not understand the paper and did not try to understand, and, most likely, he is not able to understand. I sent him this letter:

Thank you for the info about your decision on my paper. I will not appeal the decision. However, let me note that when I send a paper to a journal, I am interested not only whether the paper will be accepted or not but also in knowing the opinion of qualified referees.

In fact, you were my referee and my understanding is that, although the formal status was "With Editor" for more than a month, nobody looked at the paper till Dec 10th, when it took you less than 40 minutes to come to the conclusion. From the formal point of view the reason of rejection was "The statement about Z and the Z/pZ (i.e. F_p) is obvious." And also you advise me to look at the literature on "pseudofinite" structures. I would be very grateful if you answer the following questions.

I understand that the statement is simple, have no doubt that for you the statement is indeed obvious, and several mathematicians said the same. However, in my understanding, in mathematics the statement that something is obvious needs to be explained. Could you please give me a direct reference where this statement is proved and how the limit is understood. You and several mathematicians told me that this is obvious from ultraproducts, "pseudofinite" structures etc. and I agree. However, those notions are rather sophisticated. My paper is titled "A new look at potential vs. actual infinity". Those notions are discussed in the framework of actual infinity. The mentality *of many mathematicians is that problems with characteristic 0 are fundamental while finite rings or fields can be used as something auxiliary for tackling those problems. My observation is that the majority of mathematicians do not care that standard mathematics has foundational problems (as follows e.g. from Gödel's incompleteness theorems and from other considerations). My hope was that NDJFL does care about this.*

My math professor was a famous mathematician M.A. Naimark and I was very impressed by his lectures on calculus and group representations. As I note in the abstract, the technique of standard math involves only potential infinity while the basis does involve actual infinity: the theory starts with Z, then rational, real, complex numbers and sets with different cardinalities are introduced etc. As a rule, in mathematics legitimacy of every limit is thoroughly investigated but in standard math textbooks it is not even mentioned that Z is the limit of Z/p (by the way, $Z/p = F_p$ only if p is prime) and nothing is said on whether the limit is legitimate. The matter is that when Z/p is replaced by Z we arrive at standard math which has foundational problems.

I came to my ideas from physics where I proved that quantum theory based on finite math is more fundamental than quantum theory based on standard math: the latter is a special degenerated case of the former in the formal limit $p \to \infty$, and in my paper I argue that analogously, standard math is a special degenerate case of the former in the formal limit $p \to \infty$.

So I believe that the fact that $Z/p \to Z$ when $p \to \infty$ should be proved without reference to ultraproducts, "pseudofinite" structures etc. but directly by analogy with the proof that some sequence $(a_n) \to \infty$ when $n \to \infty$. Unfortunately, this is not easily found in google and the majority of mathematicians prefer to work with Z from the beginning without caring whether or not Z is a limit of a finite set.

The main meaning of the letter is this: in mathematics there should not be an argument that something is simply obvious. Any claim that something is obvious needs to be proven or explained. And I received in response two letters written with an interval of 22 minutes:

I am not saying that your Statement 1 follows from some machinery such as ultraproducts, I am just saying that Statement 1 is obvious. Given integers a, b for all stuff. large primes p, $a + b$, $a \times b$ in Z coincides with $a + b$, $a \times b$ in F_p.

Let me clarify. It is not just obvious it is a matter of definition. For a prime p, the field F_p consists of elements $0, 1, ..., p - 1$, with addition and multiplication modulo p. Namely for a, b in F_p, $a + b$ (in F_p) is the remainder when $a + b$ is divided by p. Likewise for $a \times b$. So by definition, if a, b are in F_p, then for p bigger than max of $a + b, a \times b$, $a + b$ and $a \times b$ coincide in Z and in F_p.

So, at first, he writes that this is obvious and tries to explain. And after 22 minutes he already writes that this is not only obvious, but also a definition.

I wrote a great answer:

I am disappointed with the treatment of my paper at NDJFL. For more than a month the status was "With Editor", and my naïve expectation was that somebody is reading the paper. However, only after my query you spent 40 minutes or less and the only reason of rejection was "The statement about Z and the Z/pZ (i.e. F_p) is obvious". In mathematics the statements that something is obvious should always be explained but I thought that since Editor-in-Chief of such a prestige journal makes such a statement then maybe indeed it is extremely obvious and I don't understand something trivial. That's why I wrote that I would be very grateful if you explain your words. However, I was amazed by your response. In the first email you continue to state that Statement 1 is obvious: "Given integers a, b for all stuff. large primes p $a + b$, $a \times b$ in Z coincides with $a + b$, $a \times b$ in F_p." (probably "stuff." is a misprint of "sufficiently") but after 22 minutes you wrote another email: "Let me clarify. It is not just obvious it is a matter of definition. For a prime p, the field F_p consists of elements $0, 1, ..., p-1$, with addition and multiplication modulo p. Namely for a, b in F_p, $a + b$ (in F_p) is the remainder when $a + b$ is divided by p. Likewise for $a \times b$. So by definition, if a, b are in F_p, then for p bigger than max of $a + b$, $a \times b$, $a + b$ and $a \times b$ coincide in Z and in F_p.", so now you are saying that this is the matter of definition.

My first remark is technical. The problem deals only with rings and has nothing to do with division. So, it not necessary to consider the field F_p and only primes. The problem is whether Z is the limit of rings $R_p = (0, 1, \ldots p-1)$ (with operations modulo p) when $p \to \infty$.

Again, in mathematics, mathematical statements should be formulated unambiguously such that different interpretations should be excluded. For this reason the words "for any" and "there exist" are often used in mathematical statements. However, saying about a and b you are not using those words, and one can only guess what you mean. Consider you "definition" "So by definition, if a, b are in F_p, then for p bigger than max of $a+b$,$a \times b$, $a+b$ and $a \times b$ coincide in Z and in F_p." literally. For example, if $a = 0$ and $b = 0$ then for $p > 0$, $a+b$ and $a \times b$ coincide in Z and in F_p. Or if $a < 10$ and $b < 10$ then for $p > 100$, $a+b$ and $a \times b$ coincide in Z and in F_p. So your "definition" is indeed obvious.

I guess that probably you meant something like this: for any p_0 there exists a set S such that for any $a, b \in S$, $a+b$ and $a \times b$ coincide in Z and in F_p for any $p \geq p_0$, and $card(S) \to \infty$ when $p_0 \to \infty$.

However, even if my guess is correct, this still cannot be a correct definition that $R_p \to Z$ when $p \to \infty$. The definition should be such that not only for two elements from S their sum and product coincide in Z and in R_p but that it is possible to find a number n such that for any $m \leq n$ the result of any m operations of multiplication, summation or subtraction of elements from S should be the same in Z and in R_p, and that $n \to \infty$ when $p \to \infty$.

The exact formulation of the definition is given in my paper, and I prove that with this definition indeed $R_p \to Z$ when $p \to \infty$. As I said, the definition should be to some extent analogous to the definition that the sequence $(a_n) \to \infty$ when $n \to \infty$: for any $M > 0$ there exists n_0 such that $a_n \geq M$ for any $n \geq n_0$.

I asked several mathematicians to give me a reference where this is proved but nobody gave such a reference. The response of some of them was analogous to yours: this is obvious. Then I asked that if this the case, then why in mathematical textbooks this is not even mentioned and standard math starts from Z from the beginning, but again no response. As I wrote, they don't care that standard math has foundational problems (as follows e.g., from Gödel's incompleteness theorems and other considerations). But when I asked Prof. Zelmanov (who is the Fields Medal laureate) he did not say that this is obvious and advised me to look at Terence Tao's blog where ultraproducts are considered. In my paper I thank Prof. Zelmanov for his advice and refer to the blog.

Technically indeed it is possible to prove that $R_p \to Z$ follows from the results on ultraproducts although in ultraproducts they consider only fields and their goal is to use finite fields for proving some features of fields of characteristic zero. Nevertheless, this is not direct proof and the construction is rather sophisticated.

In summary, I think that, with the probability 99.99% in the literature there is no direct proof that $R_p \to Z$ when $p \to \infty$ and so my proof is new. Let me note that my paper contains not only this result: I explain that this result is the first step in proving that finite math is more fundamental than standard one: the latter is a special degenerated case of the former in the formal limit $p \to \infty$.

However, it seems obvious that you even did not try to carefully read my proof and other results of the paper. You noticed that I prove that $R_p \to Z$, immediately (within minutes) decided that this is obvious (as you say, even without the machinery of ultraproducts) and immediately wrote a rejection. I am amazed that the attitude to my paper at such a prestigious journal was on such a level.

For me it is not a great tragedy that my paper will not be published in NDJFL. I have no doubt that the results are fundamental, they will be acknowledged sooner or later and published elsewhere. However, I treat such an attitude to me as disgraceful from the professional point of view. Such an attitude in fact means that you treat me as unprofessional who submitted to NDJFL a junk which does not deserve consideration.

Of course, you have a right to have such an opinion. However, if you think that your *attitude was a mistake, I would be grateful if you tell me this and will be fully satisfied. I understand that we are only people, everybody makes mistakes, you are very busy handling such a journal, you have to look at many papers and probably some of them are indeed junk, so probably mistakes in your work are inevitable. However, decent people acknowledge that they make mistakes when this becomes obvious.*

In this answer, I first popularly explain that his explanations do not make sense. I am writing that I was very surprised that in such a prestigious journal my paper was considered at such a level.

In the end, I write that the main thing for me is not that the paper will not be published in his journal, but that the attitude towards the paper was shameful. As if I'm completely unprofessional and sent garbage to the journal that is not worth wasting time on. I am writing that we are all human and make mistakes, he is very busy with the journal, he has to look at many papers and, probably, some of them are rubbish, so, probably, mistakes cannot be avoided in his work. But decent people admit they make mistakes when they find out. And if he admits that he was wrong, then I will be satisfied.

According to my understanding, any scientist, and even more so a mathematician, must admit that he is wrong when this is explained to him. Let's say that he decided that my answer was not very polite. But, according to my understanding, when the editor-in-chief is explained that the attitude towards the paper and the author was boorish and that he wrote the answer completely wrong from a mathematical point of view, then any decent scientist should apologize and at least say that everything will be reviewed. I did not insist on this, but only asked him to admit the mistake and that then I would be completely satisfied. But he did not even answer, which shows that he does not care about scientific ethics.

Another try: Israel Journal of Mathematics. The first response was the usual:

Unfortunately your paper is out of the scope of the Israel Journal of Mathematics. Therefore we cannot consider it for publication. we do thank you for considering our journal. Sincerely yours, Tamar Ziegler Editor in Chief Israel Journal of Mathematics

So, they reject the paper, allegedly because it is not in the topic of the journal. But the description of the journal's editorial policy is: "The Israel Journal of Mathematics contains high-quality research papers on **all** aspects of mathematics and theoretical computer science." So, they write that the journal considers papers of high quality on all topics of mathematics. Therefore, the paper cannot be rejected with the pretext that it is off topic; the only reason for the rejection can only be that the paper is of poor quality. And this already means that there should be a review, which shows that the paper is really of poor quality. But when I wrote it to them, the answer was:

The Editorial Board had a look at your paper and decided that the Israel Journal of Mathematics is not the right place for it. Therefore we will not further consider the paper. This decision is final.

So, now they say that the editorial board looked and decided that their journal was not the right place for the journal. And why - no explanation. And they also warn me that this decision is final, so that I don't bother them anymore. And they wanted to spit on scientific ethics.

Attempt to publish paper in European Physics Journal H:

The first answer is again the standard one:

Dear Dr Lev,

Thank you very much for having submitted your manuscript entitled: Analogy Between Finite Mathematics and Special Relativity to The European Physical Journal H.

Your manuscript has been carefully considered by our Editorial Board, and it appears that your manuscript does not belong to the Aims and Scopes as specified at https://epjh.epj.org/epjh-aims-and-scope

Therefore, we regret to inform you that your manuscript will not be considered further for publication in The European Physical Journal H. We are sorry not to be able to bring you a positive outcome and hope that you will consider EPJH in a future occasion.

Yours sincerely, EPJH Managing Editors

Comments from the editors and reviewers: (and no comment is given).

My response: *Dear Editors,*

Thank you for your email informing about the editorial decision on my paper. The email says that "your manuscript does not belong to the Aims and Scopes as specified at https://epjh.epj.org/epjh-aims-and-scope". However, this link contains the following sentences:

"Contributions addressing the history of physics and of physical ideas and concepts, the interplay of physics and mathematics as well as the natural sciences, and the history and philosophy of sciences, together with discussions." I believe that my paper fully satisfies these requirements. So, I would be grateful if your decision is reconsidered.

Thank you.

Sincerely, Felix Lev.

Response of the journal:

Dear Dr Lev,

The new Editors in Chief confirmed the rejection of your article. I copy their response: "Even if it would be scientifically sound, it does not account for "the historical development of ideas in contemporary physics" (as we demand in our aims and scope)." We are sorry not to be able to bring you a positive outcome and hope that you will consider EPJH in a future occasion.

So, they say that even if the paper is "scientifically sound", it still doesn't fit because it does not account for "the historical development of ideas in contemporary physics". So, they didn't even try to figure out if the paper was "scientifically sound", but decided to kick it right away because supposedly there is no history in it. But even the title of the paper: "Analogy Between Finite Mathematics and Special Relativity" immediately says that a parallel is being drawn between finite mathematics and the theory of relativity, which is history because it was proposed in 1905. So, again, what is written in the editorial policy does not matter much, the main thing for them is to kick back with some meaningless words.

Attempting to get an invitation from AVS Quantum Science to write a review:

According to the rules of this journal, they usually publish reviews by invitation. To receive an invitation, you must first fill out a form with questions. The editorial policy of the journal says that the journal considers the application of quantum science in various fields and all papers must be "all through the foundations of Quantum Science". And in the papers of the editor-in-chief advertising the journal, there are the words "quantum journey", "quantum science", i.e., all quantum. From these words, it would seem that readers should be interested not only in the applications of quantum theory, but also in its foundation. And logically, how can one deal with applications of quantum theory if one does not understand its foundations? So I sent them a proposal for a review on the foundations of quantum theory. This proposal is quite long, and I will not quote it. And the first answer was again the standard one:

Thank you for your interest in the journal. After reviewing your material, the editors do not think it is an appropriate fit for AVS Quantum Science at this time. It might be more appropriate for another AIP Publishing or AVS journal and you can review the portfolio here: https://publishing.aip.org/publications/find-the-right-journal/. Please keep AVS Quantum Science in mind in the future.

This "thoughtful" response took three weeks. It can be seen that there are no arguments, but they simply do not want to take it. And, as usual, I sent an appeal:

The editorial policy of AVS Quantum Science claims that all applications should be discussed "all through the foundations of Quantum Science" and that the journal "covers recent advances in established fields or an emerging area of importance within quantum science". Those sentences indicates without doubts that the journal is devoted not only to pure applications but also to foundations of Quantum Science. The title of my proposal explicitly indicates that my review will be devoted to foundation of quantum science. As indicated in the proposal, the review is based on my results published in J. Phys. A, J. Math. Phys., Phys. Rev. D, Finite Fields and Applications, *Int. J. Mod. Phys. B and other known journals. Therefore, the review is fully in the scope of AVS Quantum Science. However, my proposal has been rejected with the statement that the editors "do not think it is an appropriate fit for AVS Quantum Science" and no other explanations have been given. As follows from the above remarks, this statement fully contradicts the editorial policy of AVS Quantum Science. Scientific ethics implies that any negative statement should be substantiated, and so the statement that the editors only think something without explanation contradicts scientific ethics. This statement poses a question whether or not anybody tried to understand my proposal. I would be grateful if the editorial decision is reconsidered.*

And, as expected, I received confirmation in response that my proposal was being rejected, and it took two weeks to write such a "thoughtful" answer:

Dear Dr Lev,

In your latest mail to AQS, you appealed the editorial decision not to consider your proposal for contribution to AVS Quantum Science. AVS Quantum Science is a new journal which aims at providing the community with a wide range of publications that cover all fields related to quantum physics. While our ambition in the future is to host original research, original results and eventually novel ideas, we are currently focusing our interest in review articles. We are focusing mostly on reviews that have been invited but we are keeping open the possibility for non-invited contributors. In all cases, the editorial team is carefully selecting the topics, formats and authors before we actually propose to submit. This is why you were requested to send a preliminary Editorial Summary form.

Members of the editorial team have assessed your proposal with extreme care, and, as mentioned in our earlier correspondance, did not feel it would fit with our current journal objectives and would be more appropriate to other journals. While our criteria may evolve in the next years, when the journal is opening to wider ranges of contributions, our editorial decision cannot be reconsidered at this time.

Our analysis was neither a peer-review process nor a critical analysis of the work you were proposing to published. Our decision should therefore not be considered as negative statement about your work.

Best regards,

Philippe Bouyer AVS Quantum Science

In my appeal, I wrote that in their editorial policy they swear that everything should be "all through the foundations of Quantum Science", and when I propose a fundamentally new approach to foundations, they reject it, i.e., "Your decision is contrary to your own editorial policy". And it does not correspond to scientific ethics and there are no explanations. And in the second answer everything is the same, they swear that they looked carefully, but again there is nothing concrete. Apparently, they have no idea what I offered them, but the main thing is again the same story, that the editorial policy of the journal is not carried out by the editors themselves.

My general conclusion is this: if you have proposed something fundamental, but you are not considered a great scientist and do not work in a prestigious place, then, having no connections (speaking in Russian, "blat"), publish it in the so-called prestigious journal is almost impossible. Very often, the issue of publishing in a journal that claims to consider fundamental problems is decided not by editors, but by people who have no idea about fundamental science but have no moral problems deciding which papers to consider and which ones to immediately reject. And even the editors of such journals often have no idea about basic science and do not consider themselves bound by what is written in the editorial policy of their journals.

I am very grateful to the journals Physics of Particles and Nuclei and Physics of Particles and Nuclei Letters, editors of which are at JINR in Dubna, for the fact that my works were considered in accordance with scientific criteria. It would seem that since Springer publishes these journals in English, the wide scientific community should be interested in these journals. Indeed, many are interested, but, apparently, there are also prejudices that since the editorial office is in Russia, then the journals, it seems, are not very prestigious. And in fact, many so-called prestigious journals publish nonsense and the public swallows it.

I am also grateful to the editors of the Symmetry journal and the editor-in-chief Sergey Odintsov for the fact that my paper submitted to this journal was considered in accordance with all the rules of scientific ethics and published in [13].

In chapter 8, I described my misadventures with an attempt to publish a book in Springer. As I wrote, the book was published (see [22]) largely because Angela Lahee turned out to be a very decent person. And one of the main rigorously proven results of the book is that classical mathematics is a special degenerate case of finite mathematics, and that the standard quantum theory is a special degenerate case of quantum theory based on finite mathematics. Therefore, my attempts to publish these results, were eventually realized, despite the adventures described in this chapter.

But then there were such considerations. As Angela Lahee wrote to me, almost all universities have subscriptions to Springer books. But not all people have access to university libraries, and even the electronic version of the book costs 109 USD (and the paper version costs 150 USD). In addition, most of those who want to read a book are unlikely to want to read all 291 pages, and will probably only look for what they are interested in. The presentation of the problems that I am now discussing begins on page 169. Therefore, I decided to write a short paper where these problems are discussed at the popular level. There are several known journals that publish popular articles on mathematics, and it seemed to me that such an article would be of interest to these journals.

My first attempt was "The Mathematical Intelligencer". The editorial policy of the journal says that they do not take the usual mathematical style of theorem-proof, i.e., everything should be at a popular level for a wide audience. One of the chief editors is Sergey Tabachnikov, who graduated from the Mechanics-Mathematics faculty of Moscow University. When I studied at the Moscow Institute of Physics and Technology, some people thought that this faculty was almost the highest caste. In connection with the problem that I am now discussing, it was interesting for me to know the opinion of mathematicians, since it seemed to me that it was obvious to them what a finite ring or a finite field is.

The review of my article was as follows:

Reviewer 1: I have read the article, and do not recommend publication. I am in principle very interested in things like ultrafinitism or questioning the role of the real numbers in physics, but this article struck me as having very little to say about such matters that wasn't too obvious to count as a genuine contribution. For instance, everybody understands (or at least all serious mathematicians and physicists understand) that infinite precision is not possible. So we use the real numbers not because we think that they map directly on to reality, but because it turns out to be convenient to do exact calculations within the real number system, obtain exact answers, and then use those exact answers to make predictions that can be verified, not exactly of course, but often to a high degree of precision. An argument against the real numbers has to offer some advantage of using a different system.

It is clear that I wrote appeal:

Author's appeal on Editorial Decision

The decision to reject my paper was based on the advice of Reviewer 1, and there were no other referee reports. The motivation of Reviewer is as follows.

Reviewer says that "everybody understands that infinite precision is not possible" and that "So we use the real numbers not because we think that they map directly on to reality, but because it turns out to be convenient to do exact calculations within the real number system, obtain exact answers, and then use those exact answers to make predictions that can be verified, not exactly of course, but often to a high degree of precision."

At this point, my approach is completely the same as the approach of Reviewer. However, Reviewer concludes the report with the sentence: "An argument against the real numbers has *to offer some advantage of using a different system.", and only this sentence is the reason for the advice to recommend rejection.*

This sentence shows that Reviewer even did not carefully read the paper. From the very beginning of the paper, I explain that mathematics with infinitesimals cannot be universal. For example, as I note, many physicists "... say that, for example, dx/dt should be understood as $\Delta x/\Delta t$ where Δx and Δt are small but not infinitesimal. I ask them: but you work with dx/dt, not $\Delta x/\Delta t$. They reply that since mathematics with derivatives works well then there is no need to philosophize and develop something else." Thus, the mentality of these physicists on the application of real numbers is the same as the mentality of Reviewer.

I fully agree that mathematics with infinitesimals is very powerful in many applications. However, I note that "The development of quantum theory has shown that the theory contains anomalies and divergences."

The idea of the paper is to explain on popular level the results of my monograph "Finite mathematics as the foundation of classical mathematics and quantum theory..." recently published by Springer. Even the title of the monograph shows that "advantage of using a different system" is discussed in detail, and in the manuscript, I explain on popular level why finite mathematics is more general (fundamental) than classical one. I note that my results are fully in the spirit of the history of science. For example, nonrelativistic theory works in many cases with a very high accuracy, but it cannot explain phenomena where it is important that the speed of light c is finite and not infinitely large. I note that, analogously, in nature there are phenomena (e.g., gravity) which can be explained only in the framework of finite mathematics where it is important that the characteristics p of the ring is finite and not infinitely large.

The Reviewer's remarks show that he/she is completely unfamiliar with the fact that the problem of infinities is one of the main problems of quantum theory and many famous scientists wrote that fundamental quantum theory should be based on finite mathematics.

In summary, the Reviewer's advice to recommend rejection is completely unfounded. My paper satisfies all the requirements specified in the editorial policy of "The Mathematical Intelligencer". I would be grateful if the Editorial decision is reconsidered.

Since there was no answer to this appeal for a long time, I wrote to Sergey Tabachnikov in Russian, and below is the translation:

Dear Sergey,

I decided to write to you in Russian about my article, which has just been rejected in The Mathematical Intelligencer. The problem is not that it is rejected, but at what level it is rejected. First, very briefly about myself. I graduated from the Moscow Institute of Physics and Technology, defended my Ph.D. and Dr. Sci. thesis in Russia and worked in Dubna. I have many papers in known journals, and recently Springer published my monograph "Finite mathematics as the foundation of classical mathematics and quantum theory. With applications to gravity and particle theory". More information about me is in my ORCID: https://orcid.org/0000-0002-4476-3080 .

One of the main problems of quantum theory is that a theory based on classical mathematics (with infinitesimals, continuity, etc.) leads to divergent expressions (the problem of infinities). Therefore, many well-known scientists have suggested that the most general (fundamental) quantum theory should be built on finite mathematics. It is rigorously proved in the book that classical mathematics is a special degenerate case of finite mathematics in the formal limit $p \to \infty$, where p is a characteristic of a field or ring in finite mathematics. The meaning of this statement is that any phenomenon that classical mathematics explains can in principle be explained with any accuracy in finite mathematics if p is very large. But there are also phenomena that can only be explained if p is finite, not infinite.

No one argues that the apparatus of classical mathematics is very powerful and in many (but not all) cases works with very high accuracy. And in such cases, there is absolutely no need to apply finite mathematics. The situation is completely analogous to that in physics. For example, a nonrelativistic theory is a special degenerate case of a nonrelativistic one in the formal limit $c \to \infty$

(where c is the speed of light), but in everyday life the nonrelativistic theory works with very high accuracy and then there is no need to apply the relativistic theory. Similarly, classical physics is a special degenerate case of quantum physics in the formal limit $\hbar \to 0$, where $\hbar$ is Planck's constant. But in those cases where classical physics works with great precision, there is no need to apply quantum theory; for example, there is no need to describe the motion of the moon by the Schrödinger equation; In principle, this is possible, but leads to unjustified complications.

In my article, I tried to explain the results of my book on a popular level. It is clear that the book could only come out after the approval of highly qualified reviewers. And even from the title of the book it is clear that the arguments in favor of finite mathematics are presented in detail. The article fully complies with all the criteria of the editorial policy of your journal. However, the article was rejected with the following review:

And I bring this review.

So, the way of thinking of the reviewer is such that since classical mathematics works in many cases, there is no need to philosophize and apply something else. As I write in the article, many have such a philosophy. The reviewer writes that there should be arguments in favor of applying other mathematics. But the whole point of the article is precisely to make such arguments, and it is clear that the reviewer did not even read the article carefully. Only one day passed from the status of "Under review" to "Reviews completed", so the review was written very quickly. The article was in the editorial office for more than a month and I hoped that a serious review would be prepared during this time. However, it is clear from the review that the reviewer did not even read the article carefully. I just sent Prof. Fernando Gouvea my appeal which is attached. The editorial policy does not say anything about appeals, but it is common practice that the author has the right to appeal. I hope that my appeal will be considered.

Thanks in advance.

Sincerely, Felix Lev.

His answers were:

Dear Dr. Lev:

Let me start by asking you to communicate with the journal in English: I am the only member of the editorial board who speaks Russian, and we conduct all the magazine-related business in English. Thank you for your understanding.

Concerning your article and the editors' decision, let me assure you that the referee is a highly qualified mathematician who had studied your article in detail. Obviously, he will remain anonymous to you, but let me say that, as an author, I'd be very happy to have a reviewer of this caliber and quality. Let me be clear: we firmly stand by his recommendation to reject the article.

Let us look at your arguments.

You claim that "every phenomenon explained by classical mathematics, in principle, can be explained with arbitrary precision in finite mathematics, if p is very large. But there also exist phenomena that can be explained only if p is finite, and not infinite".

What does this statement mean? Perhaps your book provides enough detail, but it is not at all clear from your article. You illustrate it by examples, claiming that arithmetical identities such as 10+20=30 are ambiguous, whereas 10+20=30 (mod 40) or 10+20=5 (mod 25) are not. These examples are unconvincing, and they do not clarify the meaning of the general statement above. More generally, you describe the simple relations between the rings Z/Zp and Z as an argument toward this general statement; we find this unconvincing as well.

You say that the purpose of your article was to popularize your book. Unfortunately, this goal was not achieved: it is not clear from your article whether the approach that you promote is capable of obtaining new results or of consistently explaining known results in a new way. Perhaps one needs to read your book to make sense of your theory and to appreciate it, but your article comes short of being compelling.

The final paragraph of your article (in bold) is its main message. In our opinion, to convince the reader of the truth of this credo, and even to make precise sense of it, would need much *more elaboration than presented in your article.*

Sincerely yours,

Sergei Tabachnikov The Mathematical Intelligencer Professor of Mathematics, Penn State

And then he sent a second reply:

Dear Felix, I have finally read your letter; sorry for the delay, it's a very busy time for me.

I am not a physicist, and I am not familiar with the culture of the physics community, so I cannot comment on the phenomenon that you lament about: lack of acceptance, or even a meaningful criticism, of your theory. What I can try to comment upon is the mathematical side of the discussion.

In my opinion, everything in mathematics can be used to create models of natural phenomena, be this the classical differential calculus or calculus of finite differences, be this standard or nonstandard analysis (in which infinity is not a limit), be this based on the right of integers or the modular arithmetic (the rings Z/pZ), be this classical or constructivist logic. From the mathematical point of view, one needs to obtain a consistent and, preferably, elegant theory capable of explaining the relevant phenomena in the framework of the model at hand. From this point of view, both the Galilean and the Lorentz transformations are parts of mathematics on equal footing, although RT provides a more accurate description of the nature than NT.

My criticism of your article - and in it I agree with the referee - is that it essentially just a declaration that one can build quantum theory based on the rings Z/pZ*, but it doesn't provide examples or any details. Perhaps one needs to read your book to appreciate your approach, but one cannot expect the reader to be familiar with the book. It well could be that your subject is too technical to be explained in an expository article in a magazine for general mathematical audience.*

You also seem to claim that mathematics could be rebuilt starting with the rings Z/pZ*, instead of* Z *(Peano arithmetic). This may be the case, but such an undertaking would take an enormous amount of work and, in my opinion, even if successful it will have little bearing on modeling nature. As I said, no mathematics is off limit if it's relevant in description of nature, and there is no need to rebuild the foundations for this purpose.*

It may not be directly relevant, but let me mention something that is close to my research interests. Recently, the field of discrete differential geometry has emerged, and it continues to be an active research area (the name itself is an oxymoron). The situation is somewhat similar to what you described: instead of smooth objects, such as curves and surfaces, one studies discrete ones (polygons, polyhedra), and the former can be obtained from the latter as the limiting objects. Btw, this discrete differential geometry is intimately related with completely integrable systems, which are so common in mathematical physics.

These are my thoughts. Best regards, yours Sergei

P. S. Thank you for the note about D.B. Fuchs. He is 81 now, and we continue our collaboration, working on a joint paper now.

My response to his letters was as follows: *Dear Sergei,*

Thank you for your response to my detailed letter. However, you probably will not be surprised if I say that I am disappointed with your response. You asked whether my approach "is capable of obtaining new results or of consistently explaining known results in a new way". I was very glad that you asked this question and hoped that you will read my response. But now I am not sure that you were interested in my response at all and probably you decided from the beginning that the answer is negative.

I tried to answer your question in such a way that (in my understanding) the answer should be appreciated and understood by any mathematician, even by students of mathematical departments. For example, I give a popular explanation why in modular mathematics I have one irreducible representation (IR) which splits into two IRs in the formal limit $p \to \infty$*. This (mathematically beautiful!) example immediately shows that, even from a pure mathematical point of view,* *modular theory is more general than standard one. Since you said nothing about this example then either you even did not read it at all or were unable to understand it.*

I also give other simple MATHEMATICAL examples which show that there are cases when modular theory can solve problems which standard theory cannot. However, again, no specific comments on those examples are given, and so you either did not read those examples or were unable to understand them.

I understand that everybody has his/her own problems, and nobody can insist on what other people should or should not read. But it is beyond any logic that you asked a question and said nothing explicit about my response. You say: "My criticism of your article - and in it I agree with the referee - is that it essentially just a declaration that one can build quantum theory based on the rings Z/pZ, but it doesn't provide examples or any details."

In my paper and the last letter, I give many simple MATHEMATICAL arguments but neither you nor the referee give any comments on these arguments. And so again, you either did not read the arguments or were unable to understand them. In the literature, criticism is defined as the practice of judging the merits and faults of something. But since there is no sign that you and the referee tried to understand my arguments, the word "criticism" in your letter is fully inappropriate. If it were only a discussion between two people, then everybody has a full right to read or not to read what he/she wants. But, in the given content, your opinion is understood not only as your personal opinion but as the opinion of the readers of your journal. I am not sure that your understanding of this opinion is realistic. For example, several physicists and mathematicians told me that they would be interested in reading a popular discussion of my approach. You say, "It well could be that your subject is too technical to be explained in an expository article in a magazine for general mathematical audience." But I just tried to explain my results in an extremely popular (expository) level, and, in my understanding, this is fully what the editorial policy requires.

You explain to me that "From the mathematical point of view, one needs to obtain a consistent and, preferably, elegant theory capable of explaining the relevant phenomena in the framework of the model at hand". According to the present knowledge, quantum theory is the most general model of nature which mankind has developed. So, in the content of your letter, your words can be understood only such that you do not think that my theory is elegant and capable of explaining the relevant phenomena. But in your letter, as I already noted, I do not see any sign that you tried and/or were able to understand what my theory is capable of.

Now let me comment on the following extract of your letter: "You also seem to claim that mathematics could be rebuilt starting with the rings Z/pZ, instead of Z (Peano arithmetic). This may be the case, but such an undertaking would take an enormous amount of work and, in my opinion, even if successful it will have little bearing on modeling nature. As I said, no mathematics is off limit if it's relevant in description of nature, and there is no need to rebuild the foundations for this purpose."

In my paper and the last letter, I note that (during the last 80 years) there is a great problem that, by using the existing mathematics, physicists cannot construct a quantum theory which is mathematically consistent and can explain many existing experimental data. This is acknowledged by famous scientists and even Nobel Prize laureates (as I noted, even one of them wrote a paper titled "Living with Infinities"). Also, many authors and even some Nobel Prize laureates wrote papers conjecting that the ultimate quantum theory will be based on finite mathematics. Of course, constructing such a theory would take an enormous amount of work. However, in your opinion "even if successful it will have little bearing on modeling nature" and "there is no need to rebuild the foundations".

So, you do not know about existing fundamental problems, do not have ideas how to solve them, the opinion of famous scientists is not important to you, but your opinion is that "there is no need to rebuild the foundations". So, your remarks are like those from the known Chekhov's story "Letter to a learned neighbor" when a man writes to his neighbor: "You say that there are spots on the Sun; this cannot be because this can never be".

You note that there are even similarities between our approaches because both start from a discrete approach. You will probably be indignant if someone, without any attempt to figure it out, says that your results are only declarations. You will probably say that you already have recognized works on this topic and this topic is related to generally recognized problems. But I can also say that I have papers in so-called prestigious journals, there are many other papers on this topic, and even Nobel Prize laureates wrote about this.

In summary, you asked me a question, I tried to answer this question in detail, there is no sign that you and the referee read my arguments and/or were able to understand them, but you say that my paper "is essentially just a declaration". Giving negative statements about my arguments without any explicit mentioning them, contradicts scientific ethics and is simply indecent. I understand your last letter such that you do not want to spend any time on our discussion, and I also think that your attitude is such that any further correspondence is meaningless.

I wish all the best to you and your journal. Felix.

P.S. Your statement that "both the Galilean and the Lorentz transformations are parts of mathematics on equal footing" is not correct. As explained in the famous Dyson's paper "Missed opportunities" published in 1972 in Bull. Amer. Math. Soc., the Lorentz group is more general than the Galilei one because the latter can be obtained from the former by contraction $c \to \infty$. *In turn, being semisimple, the Lorentz group (it is more correct to talk about its covering group SL(2,C)) has a maximal possible symmetry and cannot be obtained from a more symmetric group by contraction. And, as I tried to explain in the paper, finite mathematics is more general than classical one because the latter can be obtained from the former by contraction* $p \to \infty$. As usual, I bring this long correspondence, realizing that hardly anyone wants to read it all. But I must cite this correspondence lest they say that I am tendentious, arguing that the consideration of my article was not in accordance with either editorial policy or scientific ethics. Indeed, Sergey Tabachnikov writes that my statements are unconvincing and that my arguments are only declarations.

I don't know if he understands that such statements, without any attempt to substantiate them, are contrary to scientific ethics. Also, as I write in my answer, his word "criticism" doesn't match its meaning. The definition of "criticism" is: "the expression of disapproval of someone or something based on perceived faults or mistakes". That is, it is assumed that some arguments are given. And his letters do not contain any hint that he was at least trying to figure something out. And the review also shows that, as I note, the reviewer did not even read the article carefully. At first, he writes that he is interested in ultrafinitism, then he writes that it is really possible to calculate something only with real numbers, and if not, then there should be arguments. But the whole point of the article is to make such arguments and it seems that he did not even understand it.

My next attempt is Archiv der Mathematik, and Editor-in-Chief Ralph Chill's response is this:

"...We are aware of your having discussed this paper with Clemens Heine, and we have evaluated the paper by ourselves. It is true that the paper is of general nature, and could be of interest for a broader audience, but we feel that the paper does not fit into this particular journal. We want to encourage you to submit your paper to a another journal, where it certainly will find its place..."

That is, he admits that "It is true that the paper is of general nature, and could be of interest for a broader audience...". That is, he actually recognizes that the article is in full compliance with the editorial policy. But, for some reason, "but we feel that the paper does not fit into this particular journal". And he's a mathematician, not a poet, so when he appeals to his feelings, it's strange. It would seem that the question is very simple: does the article correspond to the editorial policy? Yes or no? It is clear that I wrote an appeal, but he did not answer it. That is, again, he probably does not understand that such an answer is contrary to scientific ethics.

My next attempt is Expositiones Mathematicae. Their editorial policy is:

Our aim is to publish papers of interest to a wide mathematical audience including graduate level students. This is a peer-reviewed journal that publishes papers in all branches of Mathematics under the headings "Main Articles" and "Mathematical Notes":

Main Articles are either original research papers or expository/survey articles on a current research topic or area.

Mathematical Notes must contain new results or novel points of view.

Clarity of exposition, accuracy of details, interest of subject matter and quality of research will be the decisive factors in our acceptance of an article for publication.

Editor-in-Chief Liming Ge's response:

"...Though your manuscript falls within the aim and scope of this journal, it is being declined due to lack of sufficient novelty..." That is, he explicitly admits that "your manuscript falls within the aim and scope of this journal". It would seem that in this case, he should immediately send the article for review. But he rejects the article "due to lack of sufficient novelty". This answer shows that he does not even understand what the article is about, because to say that the statement "classical mathematics is a particular degenerate case of finite" is not new enough is nonsense. But even if he does not understand this, he does not yet understand that the statement "due to lack of sufficient novelty" without any explanation is contrary to scientific ethics.

Of course, I wrote an appeal. In particular, I wrote that the statement "due to lack of sufficient novelty" can only be made in a review, but the article was not sent for review. Journal Manager response: "Please find the below response from the editor. The manuscript does fall into our aims and scope. But as a Mathematical note, we only publish short articles within ten pages or fewer. The viewpoints expressed in the paper are mostly conclusions of the discussions in the author's book. The editorial board does not find these points of view sufficiently novel, so we go beyond our requirement for a short research note..."

That is, he confirms that the article is within the editorial policy. But he writes that "But as a Mathematical note, we only publish short articles within ten pages or fewer." But the editorial policy says nothing about ten pages. In addition, it is not clear how pages are counted: ten pages in a journal or in a manuscript, and if in a manuscript, then with what font. He goes on to say that the point of view of the article is basically the conclusion of the discussion in the book, and again, the editors do not find this point of view sufficiently new. What is not new, the approach of the book or the discussion in the article? He certainly does not understand that what is in the book is completely new. And the article is made according to their policy, which even graduate level students should understand. Finally, Editor-in-Chief and this editor have the same phrase: "sufficiently novel". And what is it? It would seem that something can be either new or not new, but what is "new enough"?

Finally, another try is Historia Mathematica, and the answer is Editor Nathan Sidoli, Ph.D. such: "...Unfortunately, the Editors feel that your paper is not suitable for publication in the journal and unlikely to be favorably reviewed by the referees..." So, again, the Editor, who refers to himself as a Ph.D., and who seems to be a mathematician, not a poet, appeals to his feelings, but does not directly answer the question whether the article meets the requirements of editorial policy and says that it is unlikely that the article will be favorably evaluated by reviewers. It would seem that he must first send the work to the reviewers and only then see how it will be evaluated. But, just as I described Mullen's response from Finite Fields and Their Applications, he, like Mullen, already knows in advance that the reviews will be negative. He does not even have such an idea that suddenly they will be positive. That is, again the same example that the editor of a journal does not care about scientific ethics. And in this case, I did not even write an appeal.

This popular paper is now in vixra, in the French HAL archive and, after all my misadventures, the paper was published in the journal Open Mathematics [23]. After that, arXiv agreed to post this paper, but only in gen-ph. I wrote appeals that it is obvious that the problems discussed in the paper have nothing to do with gen-ph. After that, they put this paper also in quant-ph, they didn't want to put it in mathematical sections (although the journal is mathematical), and still gen-ph remains the main section, so I can't cross-list to other sections.

Chapter 10

Attempts to publish papers on the problem of the baryon asymmetry of the universe

This problem is as follows. According to modern particle theories and cosmological theories, when the universe was formed, it had the same number of baryons and antibaryons. And since the total baryon charge is a conserved quantum number, then at the present stage of the universe, the numbers of baryons and antibaryons should be the same. But, at least from what we see, it follows that in the world around us, there are much more baryons than antibaryons. If their numbers were the same, then, sooner or later, baryons and antibaryons would annihilate each other and there would be no ordinary matter left. In literature, the problem of the baryon asymmetry of the universe is called BAU (baryon asymmetry of the universe).

It is clear that in order to understand the BAU problem, one must first of all answer the question of whether the concept of particle-antiparticle and the conservation of baryon number is interpreted correctly in modern theory. In modern theories, the baryon number is conserved. This explains why the proton is stable. Indeed, a proton is a baryon with the smallest mass, so it cannot decay into particles with smaller masses.

But at one time, GUTs (grand unification theories) were in vogue, in which the baryon number is not strictly conserved, and there is a small but non-zero probability that the proton will someday decay. Different models gave different estimates for the proton lifetime. Some models gave a proton lifetime of the order of 10^{28} years. Of course, for a single proton, we cannot wait 10^{28} years for it to decay. But large underground laboratories were built (to eliminate the background from cosmic rays), in which large masses of water were surrounded by counters in the hope that one of the protons would decay and this would be registered. But in no such experiment was the decay of the proton recorded. Now they write that a more realistic estimate for the lifetime of a proton is about 10^{34} years, but then it is unrealistic to register the decay of a proton on Earth. The possibility that the baryon charge is not strictly conserved was first noticed, it seems, by A.D. Sakharov, and he wrote that this could be the explanation for the BAU problem [24].

In my papers and in the book, I explain that the concept of particle-antiparticle is not universal. Historically, this concept arose after Dirac showed that his equation had solutions with positive and negative energies. Solutions with positive energies correspond to electron, and with negative ones - to positron, which was found after some time.

This was a big event and convinced many that Dirac's equation was of fundamental importance. Therefore, as usual in QFT, no attention was paid to the logical contradictions in the Dirac equation. The first logical contradiction is this. Since the Dirac equation is linear, the superposition of two solutions is also a solution. In particular, the superposition of solutions with positive and negative energies is a solution. But the superposition of an electron and a positron is prohibited by the superselection rule, since it is believed that electric charge is a conserved quantity.

In Sec. 2.6 I have detailed why local fields should not be present in quantum theory. Sec. 4.1 explains in detail that conservation of electric charge and baryon number takes place only in those special cases when, in the irreducible representations (IRs) of the symmetry algebra, energy is either only positive or only negative. This is true for the Poincare symmetry and the anti-de Sitter symmetry in classical mathematics. But for more general symmetries, for example even for the de Sitter symmetry in classical mathematics and, even more so, for all symmetries in finite mathematics, IRs are such that each IR has both positive and negative energies. And then there is no standard concept of particle-antiparticle, such concepts as electric charge, baryon and lepton quantum number are not universal, but make sense with some accuracy only under certain conditions.

At present, Poincare symmetry works with very high accuracy, and therefore these concepts also work with very high accuracy. But in the early stages of the universe, symmetry cannot be Poincare or the standard anti-de Sitter. Therefore, in the early stages of the universe, the baryon number conservation law does not make sense, and the BAU problem does not arise.

So two fundamental problems are solved. The most important: since the concept of particle-antiparticle is only approximate, then all the so-called fundamental particle theories - Quantum Electrodynamics (QED), Quantum Chromodynamics (QCD) and the theory of the electroweak interaction - are not fundamental. And secondly, the problem of the baryon asymmetry of the universe, which many, including Sakharov [24], wrote about as a fundamental problem, in fact simply does not exist.

At one time I had intensive discussions of this situation with Vladimir Karmanov, which were very helpful. These discussions prompted me to write a paper about the BAU problem, and I thought that this paper should have two authors. But Volodya decided not to participate in the joint paper, and I had to write this paper alone. Well, then, as usual, my adventures with journals began when I tried to publish this paper. I will describe only a few adventures.

I decided to try Physical Review D first, although I realized that there was almost no chance. Although in 1994 they published my paper, but it was a paper on my old activities, which establishment more or less accepted. The positive thing about this journal is that, even when all the reviewers reject, you can ask that the paper be given to someone on the Editorial Board, and he must write his opinion and give his name. Therefore, there is hope that he will be ashamed to write complete nonsense. As a rule, they wrote nonsense anyway, but in chapter 5 I describe the story when the paper was taken because Misha Shifman was a member of the Editorial Board. In the same chapter I describe the story when the paper was not taken because it got to the Editorial Board member S. Pascazio. But at least he pretended to want to understand. And this article first came to a member of the editorial office Dr. Ansar Fayyazuddin. He immediately kicked it off with a meaningless reason. But I wrote that he now has to give the paper to someone on the Editorial Board. But he did not give, but tried to kick back again:

I am writing in reply to your letter of January 4. Our rejection was based on the fact that you provide no details or even a formulation of the theory that you purport to exist that allows for baryon asymmetry. In fact, it is not clear what you mean by baryon symmetry since you do not specify a theory of particle physics on which this symmetry would act. It is also not clear whether the (unspecified) purported theory satisfies the extensive tests that the Standard Model has passed over the last several decades. This paper is clearly not at a level that it can be reviewed because it fails to provide the elements that could be subjected to review. We maintain our earlier decision to not send it out for review.

And I wrote:

Second author's appeal on editorial decision

According to the editorial policy of Physical Review, "Authors may appeal a rejection *of their manuscript by the editors.... The Board member will present a signed advisory opinion to the editors, which will be sent to the authors".*

When my manuscript was rejected for the first time, I wrote my first appeal and indicated that the rejection did not contain any explanations of the reasons. Dr. Fayyazuddin (who wrote the rejection letter) is a known physicist but he does not understand that rejection without any explanation of the reasons is contrary to scientific ethics. According to the editorial policy, it was necessary to send my appeal to a Board member. However, instead of doing this, Dr. Fayyazuddin responded to my appeal in his letter of Jan 6th. Here he explains why the manuscript has been rejected. It is strange that those reasons have been given only in the second Dr. Fayyazuddin's letter. This letter is in fact a referee report. Below I explain why Dr. Fayyazuddin's arguments are not adequate in the context of the manuscript.

As noted in my first appeal, the problem of the baryon asymmetry of the universe (BAU) is fully in the scope of Physical Review D because it is fully in the scope of quantum cosmology. However, Dr. Fayyazuddin has two objections. First, he says: "Our rejection was based on the fact that you provide no details or even a formulation of the theory that you purport to exist that allows for baryon asymmetry. In fact, it is not clear what you mean by baryon symmetry since you do not specify a theory of particle physics on which this symmetry would act".

However, the problem statement is given in the very first paragraph of the manuscript: "The problem of the baryon asymmetry of the universe (BAU) is a long standing problem of modern physics described in a vast literature (see e.g. Ref. [1] and references therein). According to modern quantum theories, the baryon number is a conserved quantum number, and, according to modern cosmological theories, the universe has been created with equal numbers of baryons and antibaryons. Then a problem arises why there is an imbalance in baryonic matter and antibaryonic matter in the observable universe."

What is unacceptable in this paragraph? The conservation of the baryon number in all modern particle theories is a well-known fact. The fact that modern cosmological theories state that the universe has been created with equal numbers of baryons and antibaryons is known to all quantum cosmologists. Ref. [1] contains several references where the BAU problem is discussed, and the title of Ref. [1] contains the words "Baryon Asymmetry of the Universe". My manuscript is not a review of particle and cosmological theories, and the only purpose of the first paragraph is to mention facts known to all quantum cosmologists (and even the abbreviation BAU is well-known to them). Those facts are described even in Wikipedia in an article titled "Baryon Asymmetry". So, any physicist interested in the BAU problem can easily find a vast literature on this problem.

The second Dr. Fayyazuddin's objection is: "It is also not clear whether the (unspecified) purported theory satisfies the extensive tests that the Standard Model has passed over the last several decades".

Standard Model is a successful model, but it is only a model based on Poincare symmetry. However, quantum theories describing early stages of the universe cannot be based on Poincare symmetry. As I note in the introduction, in his famous paper "Missed Opportunities", Dyson explains that de Sitter symmetry is more general (fundamental) than Poincare one. As shown in my publications (e.g., in paper [3] in Physical Review D), the latter is a special degenerate case of the former in the formal limit $R \to \infty$ where R is the parameter of contraction from the de Sitter algebra to the Poincare one, and (as shown e.g., in section 2 of Ref. [3]) in semiclassical approximation R coincides with the radius of de Sitter space in General Relativity. Since now this radius is very large, Poincare symmetry works with a high accuracy. However, at early stages of the universe this parameter cannot be large, and Poincare symmetry cannot work at those stages.

So, Dr. Fayyazuddin's argument with Standard Model is inadequate in the context of my work. Moreover, in view of this argument, all physicists working on de Sitter quantum theories should justify their results by investigating their agreement with Standard Model, but this is not consistent.

Let me also comment on Dr. Fayyazuddin's terminology where he talks about my theory *only with an adjective "purported" and says: "you provide no details or even a formulation of the theory that you purport to exist".*

For explaining the BAU problem I do not need any theory describing specific interactions (e.g., QED, QCD and electroweak theory). I need only properties of irreducible representations (IRs) of the de Sitter algebra. Those properties are described in detail in sections 2 and 3. Dr. Fayyazuddin does not explicitly mention those sections, and so a question arises whether he carefully read them.

Let me now briefly describe why I think that the results of the manuscript are fundamental.

The notions of particle-antiparticle, baryon number and its conservation arise because the energy in IRs describing particles in Poincare invariant theories can be either strictly positive or strictly negative. The corresponding IRs are associated either with particles or with antiparticles. However, this is not the case for more general (fundamental) IRs of the de Sitter algebra. I note that one IR of the de Sitter algebra contains both positive and negative energies. When symmetry is broken such that de Sitter symmetry becomes Poincare one then one IR for the former splits into two IRs for the latter with positive and negative energies. So, the very notions of particle-antiparticle, baryon number and its conservation arise as a result of symmetry breaking from a more general symmetry to a less general one. Since now the value of R is very large, Poincare symmetry works with a high accuracy, and those notions have a physical meaning with a high accuracy. However, they do not have a physical meaning at early stages of the universe. So, standard statements that the universe has been created with equal numbers of baryons and antibaryons do not have a physical meaning.

As I note in the manuscript, the Dyson paper appeared in 1972, and, in view of Dyson's results, a question arises why modern particle theories (e.g., QED, QCD and the electroweak theory) are still based on Poincare symmetry and not de Sitter symmetry. I think that the problem of constructing particle theory based on de Sitter symmetry is one the most fundamental problems of quantum theory. Probably, many particle physicists think that since now R is much greater than sizes of elementary particles, then there is no need to construct such a theory. This argument is not consistent because usually more general theories shed a new light on standard concepts. As noted above, the very notions of particle-antiparticle, baryon number and its conservation arise as a result of symmetry breaking from de Sitter symmetry to Poincare one. So, in de Sitter quantum theory those notions will be replaced by fundamentally new ones. The fact that such a theory does not yet exist does not mean that investigation of de Sitter symmetry on quantum level should be prohibited.

I understand that many physicists may not like those conclusions. However, they are based on rigorous mathematical results about IRs of the de Sitter algebra. As noted in my first appeal, those results are described in detail in my publications, e.g., in sections 4 and 5 of my paper in Physical Review D [3], in my paper in Journal of Physics A [8], in my Springer monograph [4] and in other my publications, e.g., in Journal of Mathematical Physics. Those publications could be possible only after approval of highly qualified referees, and my manuscript is based on my results in [3,4,8]. So, as noted in my first appeal, I believe that the only scientific way to reject my manuscript is to explicitly show that something is erroneous either in [3,4,8] or in the manuscript.

In summary, I believe that Dr. Fayyazuddin's objections against the publication of my manuscript are not based on consistent physical arguments. So, I think that, according to the editorial policy of Physical Review, the manuscript should be either sent for review or my appeal should be sent to a Board member.

Now my paper was given to a member of the Editorial Board, and I received this answer:

The above manuscript has been reviewed by Professor James M. Cline in his capacity as a member of our Editorial Board in accord with our standard procedure for a formal author's appeal. A copy of his report is enclosed. In view of this report, we regret to inform you that your appeal is denied. Our decision against publication is maintained, and here is what James M. Cline wrote: *This paper purports to say something about the baryon asymmetry, but in fact there is no* *physics to be found in it. The editor was perfectly justified in not sending it out for review, since it would be impossible, and a waste of time on the part of a referee, to find something wrong with a paper that makes no sense from the outset.*

That is, this great scientist James M. Cline (since he is in the Editorial Board of Physical Review D, then he is a great scientist by definition) did not even pretend that he read the paper and appeal (which, it seems, is his duty), but simply wrote that there is no physics in the paper, and it was fully justified not to send it for review, because it would be just a waste of time for a reviewer to look for something wrong in a paper that doesn't make sense to begin with. By this, he showed that he was not just unqualified, who did not understand anything, but also a boor who had no idea about scientific ethics.

The next attempt was Nuclear Physics B, which considers all these issues. And again the paper came to Hubert Saleur, who, as I wrote in chapter 5, rejected my other paper and then didn't even want to respond to the appeal. And now he has rejected a paper with the same text. That is, he has this text prepared for all occasions when he wants to reject a paper, and it does not matter at all what the paper is about.

Finally, another attempt was the Journal of Mathematical Physics. The paper was immediately rejected simply because The Associate Editor wrote, without any explanation, "This paper does not present an important result in mathematical physics.", demonstrating that he was either simply unqualified that he did not understand anything, or did not even try to understand. And, of course, I wrote an appeal:

The rejection of my paper was based on the Associate Editor's comment consisting of one sentence: "This paper does not present an important result in mathematical physics." This comment, given without any explanation, indicates that the Associate Editor did not carefully read the paper and/or was unable to understand its results. The fact that the paper contains fundamental new results in mathematical physics has been explained in the cover letter and in the paper itself. However, in view of the comment, I will try to briefly explain this point again. The concept of particle-antiparticle is a fundamental concept of mathematical physics and particle physics. This concept is considered in detail in my book recently published by Springer: Felix Lev, Finite Mathematics as the Foundation of Classical Mathematics and Quantum Theory. With Application to Gravity and Particle theory. ISBN 978-3-030-61101-9. Springer,

https://www.springer.com/us/book/9783030611002 .

Here it is explained that the concept has a physical meaning only in very special cases when the symmetry algebra is such that its irreducible representations (IRs) contain states with either only positive or only negative energies, i.e., the IRs cannot contain states with both signs of energies. For algebras important for particle physics this takes place only for IRs of the Poincare and anti-de Sitter Lie algebras over complex numbers. Those algebras are special degenerate cases of more general algebras for which IRs contain states with both signs of energies, and therefore for such algebras the concept of particle-antiparticle does not have a physical meaning. At the present stage of the universe the Poincare symmetry works with a very high accuracy and that is why at this stage the concept of particle-antiparticle also is valid with a very high accuracy. However, at very early stages of the universe the symmetry algebras cannot be such that the concept of particle-antiparticle has a physical meaning. This immediately explains that the known problem of the baryon asymmetry of the universe (BAU) does not arise. The explicit consideration of relevant IRs requires lengthy calculations, and they were described in the book and in my papers published in known journals (in particular, in my two rather long papers in JMP). But the BAU problem has been mentioned in the book very briefly. On the contrary, in the given paper (which is rather short) I explain only the meaning of the results on IRs with references to the book, and then explain how the results on IRs are applied to the BAU problem.

When Professor Solovej became the Editor in Chief of JMP, he wrote in his introductory note that "...It should publish high-quality papers of interest to both mathematics and physics, and this criterion should be applied vigorously in the review of papers. ...We should put quality before *quantity." I believe that my paper fully satisfies these requirements because it considers not only mathematical results of constructing different IRs but also shows how those results are applied to the known physical problem. I believe that for quantum physicists it should be obvious that the concept of particle-antiparticle is fundamental, my approach to this concept is completely new, the BAU problem is fundamental, and my approach to this problem also is new. So, I believe that my paper should be published or not depending only on whether my results are correct or not. So, when the Associate Editor writes that "This paper does not present an important result in mathematical physics" then this sentence can be treated as a scientific conclusion only if he/she explicitly explains why he/she treats my results as non-important. I think it is obvious that when the author sends a paper to JMP, he/she is interested not only in whether the paper will be published or not but also in knowing the opinion of qualified physicists/mathematicians. However, my previous paper also has been rejected with only one sentence: "The paper is not of sufficient mathematical quality to warrant publication in Journal of Mathematical Physics." and without any explanation. This poses a question whether JMP understands that official negative statements without any explanation contradict scientific ethics.*

When I wrote an appeal on the first rejection, Professor Solovej responded: "It is certainly not enough that the statements are correct. Your paper seems better suited for a journal addressing fundamental issues of physics." I was surprised that JMP does not consider papers "addressing fundamental issues of physics". But maybe, my paper was treated as only a mathematical one? In any case, the present paper fully satisfies Professor Solovej's criteria because it shows how mathematics is applied to a fundamental problem of physics. I would be grateful if the editorial decision on my paper is reconsidered. I still hope that JMP has highly qualified physicists and mathematicians who can judge my paper on the basis of scientific criteria.

And now the answer was written by editor-in-chief Jan Philip Solovej:

"We regret to inform you that your request to appeal the decision on the manuscript cited above has been declined. We found your manuscript to be speculative and the presentation to be superficial. Maybe many details can be found in your book, but we believe a paper should be a much more stand alone document. From this point of view the paper in itself does not make an important contribution to Mathematical Physics."

That is, without any explanation, he says that "...manuscript to be speculative and the presentation to be superficial" and therefore the paper is not an important contribution to Mathematical Physics. That is, again, he is either just unqualified that he didn't understand anything, or, most likely, he didn't even try to understand, and, of course, thinking about scientific ethics is below his dignity.

The fact that Dr. Fayyazuddin is not going to do scientific ethics, another story with my paper, which I also sent to Physical Review D, confirmed. He immediately kicked it off with the following text: "From our understanding of the paper's context, motivation, presentation, level of argumentation, and degree of importance and interest in physics research, we conclude that your paper is not suited for Physical Review D." He does not give any explanations, such a text can be written about anything and there is no hint here that he even looked at the article. It is unclear if Dr. Fayyazuddin understands that he completely violates scientific ethics and shames Physical Review D or he is so unqualified that he does not understand this.

So far, papers with a solution to the BAU problem have been published in vixra, in the French archive HAL [25], and recently it was published in Proceedings of the 25th Bled conference and in arXiv [26], but, as usual, arXiv didn't want to move the paper from gen-ph to hep-th where it should be: "After careful consideration, our moderators have denied your appeal. We understand this is a disappointing result, but please note this is the final decision and no further consideration will be given."

Chapter 11

The problem of neutrino oscillations

One of the fundamental problems of particle physics is the problem of generations. For example, there is the electron, the μ-meson, and the τ-lepton. Their masses differ greatly: the mass of the electron is 0.511 MeV, the μ-meson is 105.66 MeV, and the τ-lepton is 1.777 MeV. But they do not participate in strong interactions, they participate in electromagnetic interactions in the same way, and in weak interactions they participate almost equally. The meaning of "almost" will be explained below. Analogously, there are three kinds of neutrinos: electron neutrino, muon neutrino, and τ-neutrino. These neutrinos do not participate in strong and electromagnetic interactions, and participate in weak interactions almost equally. The meaning of "almost" is the same in both cases. It used to be that the electron and the electron neutrino have the lepton electron number +1, the muon and the muon neutrino have the lepton muon number +1, and the τ-lepton and τ-neutrino have the lepton tau number +1. The corresponding antiparticles have corresponding lepton numbers -1.

And for a long time, it was believed that the lepton quantum number is strictly conserved. For example, in the decay of the neutron into the proton, positron and neutrino, the electron neutrino is born, in the decay of $\pi^+ \to \mu^+ + \nu_\mu$ the muon neutrino is born, etc.

But then it was discovered that when a neutrino flies a relatively large distance, the neutrino lepton number can change: for example, the electron lepton number can become the muon lepton number, and so on. Perhaps the most impressive effect is that the number of electron neutrinos from the Sun turned out to be three times less than expected based on solar models. For the experimental discovery of this effect, Ray Davis and Masatoshi Koshiba received the Nobel Prize in 2002.

The following model was proposed to explain neutrino oscillations. There are three types of neutrinos with different masses. These states are elementary particles because, by definition, an elementary particle is described by an irreducible representation of the Poincare algebra with a certain mass. These mass states do not have lepton quantum numbers. And the electron, muon and τ-neutrinos differ in that they are different superpositions of mass states. Then the flavor of a free neutrino oscillates during the flight of this neutrino.

The principle of superposition in quantum theory does not prohibit states that are superpositions or direct sums of states of elementary particles. The concept of a direct sum is completely different from the concept of a tensor product. The tensor product of two elementary particles is two elementary particles, and the direct sum is one particle, which is not elementary because it is a superposition of elementary particles. In connection with the concept of a tensor product, the concept of entanglement is widely discussed in literature.

This concept became especially famous in connection with the discussion of the paper

[27]. In it, Einstein, Podolsky, and Rosen proposed such an experiment. Let us assume that a certain particle decays into two states A and B with the same probability, and then these states move away from each other. Let us assume that after a long time we detect state A at a large distance to the right of the decay point. Then we know for sure that only state B can be detected at a large distance to the left of the decay point. Conversely, if we have detected B, then we know for sure that A can be found on the left.

The authors of [27] believe that this experiment shows the incompleteness of quantum theory, since, after the experiment on the right, the wave function of the left state is immediately reduced, contrary to the requirement that no information can be transmitted faster than the speed of light. But there is no contradiction with quantum theory, since the wave function describes only probabilities and nothing more. If the observer found state A on the right, then the observer on the left will not receive this information immediately, but only after some time. In the given case, the wave function of the system is the tensor product of the wave function ψ_A in the Hilbert space H_A and the wave function ψ_B in the Hilbert space H_B, that is, two Hilbert spaces are needed.

However, the direct sum $\psi_A + \psi_B$ of the states ψ_A and ψ_B is an element of one Hilbert space H. Here we cannot detect both A and B: if, as a result of the experiment, state A was detected, then, according to the principle of reduction of the wave function, after the experiment, the wave function will no longer be superposition of $\psi_A + \psi_B$ and only A can remain and, even, for example, in the case of neutrinos, this state can be completely absorbed.

That is, the states $(f_1, f_2, f_3) = (\nu_e, \nu_\mu, \nu_\tau)$ with different flavors are no longer elementary particles, but superpositions of elementary particles (ν_1, ν_2, ν_3) with different masses m_i:

$$f_i = \sum_{j=1}^{3} U_{ij}\nu_j \ (i = 1, 2, 3) \tag{11.1}$$

where U_{ij} are elements of a complex 3x3 matrix.

Before the problem of neutrino oscillations, the direct sum of elementary particles was used only in QCD to describe quark mixing using the Cabibbo angle or Cabibbo–Kobayashi–Maskawa matrices. The fundamental difference between the cases of quarks and neutrinos is this. Since quarks cannot be in free states, the quarks in one direct sum are inside the same nucleon or meson, and the distances between such quarks cannot exceed the size of a given nucleon or meson. On the other hand, there are no theoretical limits on the distances between different neutrino mass states from the same direct sum.

Now the cardinal question arises: what should be the superposition of mass states? For some reason (apparently, to simplify life), it is assumed that different mass states have the same momenta, but there are no theoretical arguments in favor of this assumption. For example, the author of the paper [28] writes: "Why should one assume that the different mass eigenstates ν_j in a beam have a common momentum but different energies? Why not assume they have a common energy but different momenta? Or different momenta *and* different energies? And what oscillation pattern is predicted if one does make one of these alternate assumptions?" To this list of questions one can add: "Why not assume that different mass states have the same velocities and it is not even clear why the directions of momenta of all particles should be the same?"

The parametrization of the matrix U from the equation (11.1) depends fundamentally on which model for the direct sum we choose. If the momenta of the masses m_i are the same, then their velocities are different. Usually, the matrix is parameterized on the assumption that the momenta of the components are the same. Then, with the generally accepted values of the differences between the squares of the masses of the components, after a year, the distances between the components will be about one meter [29]. And for example, for neutrinos coming from Sirius (the distance to which is "only" 8.6 light years), the distances between the components will be about 8.6m. But for the bulk of neutrinos from stars, the distances between the components will be of the order of kilometers or more.

The problem arises whether the interaction of such neutrinos with a detector on Earth can still be described in terms of $(\nu_e, \nu_\mu, \nu_\tau)$. As [28] states, oscillations will not occur under these conditions. On the other hand, in a model where the velocities of the masses m_i are the same, the distances between their wave packets will not change with time. And if the momenta of the masses m_i have different directions, then it is not at all clear what theoretical predictions can be made. This problem is of great theoretical interest, but its experimental study is problematic. Most of neutrinos detected by neutrino observatories are either solar neutrinos or neutrinos produced when high-energy particles from space collide with particles of the earth's atmosphere. Therefore, it is very difficult to detect a neutrino that came to Earth from a distant star.

Since there are no theoretical arguments in favor of one or another model for the direct sum, the following is not clear: suppose that we have chosen some model and we managed to find the parameters of the matrix U, which describe the experiment with a good accuracy. Will it give any hint as to which superposition theory describes real physics? But, despite the large number of attempts to parameterize the matrix U, it is not clear whether there are theoretical arguments in favor of one or another choice of parameterization. For example, the paper [30] discusses that, assuming that the momenta of the components are the same, which parameters of the U matrix are known with a good accuracy, which are known with uncertainties, and which are completely unknown.

So, although the phenomenon of neutrino oscillations has been confirmed in a large number of experiments, there is no reliable theory describing the physics of this phenomenon. Therefore, I think that the physics of neutrino oscillations should be described in approaches that are fundamentally different from what is now, and different approaches to this problem should be welcomed in the literature.

In [29] I proposed an approach in which the neutrino remains an elementary particle, and oscillations arise due to the fact that in the AdS quantum theory, the kinematics of a free neutrino differs from the kinematics of a free neutrino in the Poincare invariant theory. There are still problems in this approach that need to be addressed. I submitted a paper to the journal "Universe". There were three reviewers, and after my responses to reviews, two of them recommended publication, Reviewer #3 was against it, and the journal rejected the paper. This reviewer did not comment on my arguments that the description of different kinds of neutrinos by direct sums is not based on any serious theoretical arguments; he/she still talks in terms of "mass and flavor eigenstates, Cabibbo-like mixing angles, PMNS matrix elements, MSW theory etc." That's all right. But perhaps his/her most "powerful" argument is that the paper is "the drastic deviation from the standard principles of quantum field theory and special relativity" and that it rejects

$$E^2 - p^2 = m^2 \tag{11.2}$$

In my response to the first review, I popularly explained the following. The theory of relativity does not reject $E = p^2/(2m)$, but says that this relation is approximate, and it works with a good accuracy when $v \ll c$. Similarly, AdS does not reject (11.2), but says that this relation works with a good accuracy only to some approximation. But Reviewer #3 completely ignored my explanation and again wrote that deviating from (11.2) is unacceptable. That is, he/she does not say that my explanation is wrong, but simply ignores it. Suppose it could still be understood somehow if he/she were in principle against de Sitter. But he/she even recommends me some papers on AdS/CFT. That is, he/she does not understand at all that for de Sitter symmetry, the relation (11.2) can only be approximate. I immediately wrote a short letter to the editors in which I wrote that Reviewer #3 does not understand the very basics of de Sitter symmetry and asked the editors to ask ANY expert on de Sitter symmetry whether the relation (11.2) is exact or approximate. And then I sent them this appeal:

Manuscript ID: universe-2083477: "de Sitter symmetry and neutrino oscillations" by F. Lev.

Author's appeal on editorial decision

My paper has been rejected on the basis of the report of Reviewer #3 and Academic Editor's note: "The author did not address the issues raised by reviewer n.3 in a satisfactory manner". The editors did not take into account that Reviewer #1 and Reviewer #2 recommended publication.

As I note even in the abstract: "Although the phenomenon of neutrino oscillations has been confirmed in many experiments, the theoretical explanation of this phenomenon in the literature is essentially model dependent and is not based on rigorous physical principles", and the discussion in the paper explains this statement in detail. I propose a fully new approach. Even from the title of my paper, it is clear that any reviewer should have at least very basic knowledge in de Sitter symmetry at quantum level. However, Reviewer #3 does not have this knowledge. His/her remarks about E2-p2=m2 immediately demonstrates this. In my reply to his/her report, I explain in detail that in AdS quantum theory, this relation can be only approximate. I also point out to the fundamental Dyson's result that AdS quantum theory is more general (fundamental) than Poincare quantum theory.

I believe that in this situation, any decent scientist should acknowledge that he/she was not right. However, in his/her second report, Reviewer #3 does not discuss my explanation at all and again repeats his/her statement about E2-p2=m2. Scientific ethics assumes that, in the discussion between the author and the reviewer, both sides should try to understand each other, and it should not be such that the reviewer's statement is the ultimate truth that is not subject to discussion. I have no doubt that for any expert in de Sitter quantum theory it will be ridiculous that E2-p2=m2 in this theory is discussed in a prestigious journal which has an impact factor of 2.813, and that this is in fact the main reason for rejecting the paper. It is also ridiculous that Reviewer #3 quotes papers on AdS/CFT without having any basic knowledge in AdS quantum theory.

I do not claim that my approach solves the problem of neutrino oscillations. The problem is very complex and what I propose is only an initial approach to solving the problem. I note that my paper is submitted to the section "Mathematical Physics". In my understanding, it is not assumed that a paper on mathematical physics should immediately describe some experiments: the goal of mathematical physics is to propose new mathematical approaches which, hopefully, sooner or later will be used in physics. However, in the report of Reviewer #3 there is no sign that he/she treated my paper as submitted to Mathematical Physics. In particular, there is no sign that Reviewer #3 is familiar with representations of the AdS algebra in Hilbert spaces.

In the essential part of his/her report, Reviewer #3 discusses mass and flavor eigenstates, Cabibbo-like mixing angles, PMNS matrix elements, MSW theory etc. So, he/she discusses the problem in the framework of the existing approach to neutrino oscillations. However, I state in the paper that this approach contains several very essential theoretical uncertainties. Reviewer #3 does not comment on my statements, in particular, he/she does not say that they are incorrect. Moreover, those statements essentially come from Kayser's paper which Reviewer #3 recommended. So, the idea of my paper is to consider the problem without representing flavor states as direct sums of mass eigenstates. Nevertheless, Reviewer #3 discusses my paper in the framework of the approach which he/she likes, but in the context of my paper this is meaningless.

Reviewer #3 writes that in my paper there is no prediction for neutrino masses and for theoretical change in the decay rate of a muon. I already responded to these remarks, but Reviewer #3 repeats them again without commenting on my response.

Reviewer #3 does not agree with my explanation why the status of the electron, muon and τ-lepton differs from the status of the neutrino. He/she writes: "But if we set a typical m of the order 0.01eV for neutrinos, we still get a very large mR." But I explain that the matter is that for the electron, muon and tau lepton, the variations of $\tilde{W}$ are much less than mR, while for the neutrino they are of the same order or greater.

Reviewer #3 writes: "I also don't see any alternative theory proposed in the article". He/she repeats that my paper contains "the drastic deviation from the standard principles of quantum field theory and special relativity" and again writes: "I didn't find any compelling argument to reject E2-p2=m2." So, Reviewer #3 even does not understand how ridiculous it is to recommend papers on AdS/CFT and at the same time accuse me of deviating from special relativity and from E2-p2=m2. Those statements show that Reviewer #3 is completely unqualified to understand my paper, because all its results come from the fact that in ADS quantum theory the relation E2-p2=m2 is not exact.

I would be grateful for the info on whether this appeal will be considered.

As you can see, in particular, I wrote that it does not fit in my head that a paper in a prestigious journal with an impact factor of 2.813 is rejected due to the fact that one of the reviewers does not understand that in the de Sitter invariant theory the relation E2-p2=m2 can be only approximate, and this despite the fact that the reviewer recommends papers on AdS/CFT. At the end of the appeal, I asked them to let me know if my appeal would be considered. In my opinion, they should be ashamed that the paper was rejected in such a way, and by all the rules of scientific ethics, they should consider the appeal. And got this response:

After considering your appeal, a member of the Editorial Board has decided to uphold the original decision, and in their link the reason is described as:

Academic Editor Notes: The appeal's document does not add any additional information and there is no reason to change our previous decision.

That is, it is clear that no one seriously considered my appeal and/or even did not want to consider it. Their letter says that this decision was made by "a member of the Editorial Board". During the submission of the paper, five potential reviewers had to be proposed. In my proposal, three of them were members of the Editorial Board. As I wrote above, of the three reviewers, two were in favor and Reviewer # 3 was against. These two wrote that they would sign their review, and Reviewer # 3 (who rejected the article) wrote that he would not sign. I fully admit that he/she was also "a member of the Editorial Board" at the same time. If so, it is clear that he/she had no desire to consider the appeal.

This journal also launched a Special Issue "Origin of the Flavor Structure in the Standard Model and Beyond" in which editor-in-chief is Prof. Dr. Fei Wang. They invited me to send them a paper for this Special Issue. I wrote to them that my paper [29] was completely on their topic, but it was rejected by Universe. I sent to the editors of the Special Issue my appeal to Universe and asked if they would consider my paper if I officially sent it to them. But I didn't get any response either. Such a behavior — neither yes nor no — also completely contradicts all the rules of scientific ethics.

Chapter 12

Conclusion

The main objectives of these notes are as follows. First, I wanted to describe in the most popular way possible my understanding of fundamental quantum physics and mathematics and what I was trying to do. The most important thing in my approach is, perhaps, what is stated in section 2.5 and chapter 5. Now I will repeat very briefly what is the most important.

The concept of infinitesimals was proposed by Newton and Leibniz. In those days, people knew nothing about elementary particles and atoms and thought that, in principle, any substance can be divided into any number of parts. But now it is clear that as soon as we reach the level of elementary particles, further division is impossible. So there are no infinitesimals in nature, and the usual division is not universal: it makes sense only up to some limit.

Would it seem obvious? And then it is clear that fundamental quantum physics must be built without infinitesimals. It would seem that everyone understands that the construction of such a physics is far from being an easy task, and it would seem that attempts at such a construction should be encouraged. However, my stories described above show that, as a rule, the establishment not only does not encourage such attempts, but does everything to ensure that the results in this direction are not published.

What's more amazing. As a rule, physicists even pronounce words that in nature there are small, but not infinitesimals. And, it would seem, from this it is obvious that standard mathematics with infinitesimals, continuity, etc. cannot be the theory on which the most fundamental physics is based; it can only be a good approximation. But here, physicists say that since standard mathematics generally works, then why philosophize and involve something else. As a rule, most physicists do not know finite mathematics, and when they hear something like a Galois field, then, for peace of mind, it is easier for them to consider that this is some kind of exotic or pathological.

I understand that, as a rule, physicists face problems that can be solved within the framework of conventional approaches. And I am by no means suggesting that all physicists should switch to finite mathematics. But, in any case, I think that physicists should not be aggressively opposed to attempts to build quantum physics without infinitesimals. But my stories show that, for some reason, many physicists are aggressively against and sometimes even are ready to fight to death against publications with attempts to consider approaches with finite mathematics.

When I studied at the MIPT and listened to lectures by M.A. Naimark, V.S. Vladimirov and other well-known mathematicians, it seemed to me that the Mechanics-Mathematical Faculty of Moscow University was almost the highest caste, since rigor is the highest priority for mathematicians. But then, talking to mathematicians, I was surprised that many of them know about Gödel's theorems and the problems with the foundation of mathematics, but their way of thinking is that since standard mathematics works in many cases, then there is no need to worry about problems in its foundation. In this sense, their way of thinking is similar to the way of thinking of physicists, who think that since the theory works in many cases, there is no need to impose rigor. But still, mathematicians generally know finite mathematics, and I hoped that it would be interesting for them to know that finite mathematics is more general than standard. And, since there are no problems with foundation in finite mathematics, mathematicians, in any case, should not be aggressively against my publications. But, as I described, it is very strange that even many "finite" mathematicians are aggressively opposed, and standard mathematicians even more so.

In addition to the infinitesimal problem, I have described other problems in which I proposed new approaches, but since they are not in the spirit of what the establishment does, I had big problems with the publication. But, of all these tasks, there is one that probably overshadows all the others. This is a dark energy problem.

It would seem that it is generally accepted in physics that when new experimental data appear, one must first try to explain them on the basis of existing science. Only if this does not work out, then you can attract some kind of exotic. But here it's the opposite: they immediately began to attract dark energy, quintessence and other nonsense. There is a lot of activity, writing articles, holding conferences, planning expensive experiments and even giving Nobel Prizes. And in all my articles on this topic (for example, in the last popular article [17]) and in my book [22] I explain that there are no problems with explaining the cosmological acceleration, everything is explained based on known science, and therefore, dark energy and quintessence are nonsense. It would seem that if the establishment is honest, then they should at least read [17] and directly say whether I don't understand something or they don't understand. But they pretend that they do not notice my publications on this topic.

Many physicists know that there are problems in standard quantum theory, such as divergences. In renormalizable theories, they can be formally eliminated (if one does not pay close attention to mathematical rigor). But the dogma is such that quantum gravity is a non-renormalizable QFT and there one cannot get rid of infinities even in the second approximation of perturbation theory. And even in renormalizable theories, the properties of the perturbation series are completely incomprehensible, for example, whether it converges, whether it is asymptotic, and so on. So, if the interaction constant is not small, then nothing can be calculated either.

Some physicists believe that all these problems are not serious, and those who consider these problems serious think that QFT or string theory needs to be improved somewhere and then these problems will be solved. But it is assumed that all this will be done in ordinary continuous mathematics, although, from what has been said above, it seems obvious that such mathematics cannot be fundamental at the quantum level.

Attempts to solve the fundamental problems of the discrete world with the help of continuous mathematics are well illustrated in the anecdote that Tolya Shtilkind told me and which I cited in Sec. 4.3. But since many readers of these notes may want to read only the introduction and conclusion (if any), then I will quote this anecdote again.

"A group of monkeys was given a mission to reach the moon. After that, all the monkeys began to climb trees. The monkey that climbed the highest thinks that he has the most progress and is closer to the goal than the rest of the monkeys." I even quoted this anecdote in my book [22], and it also contains the moral that in order to reach the Moon, one must first get down from the trees. In this case, climbing down the trees means admitting that the fundamental problems of quantum theory cannot be solved with continuous mathematics. But most physicists do not accept this; it is more comfortable for them to sit in trees and find out who climbed higher.

In these notes, I propose to solve the fundamental problems of quantum theory with the help of finite mathematics and give arguments in favor of this. The reader may have different opinions about how reasonable, fundamental, etc. my approach is. But, according to my concepts, science can develop only if different approaches have the right to exist. How to put it into practice?

From a formal point of view, there seem to be all the conditions for this. There are many journals where the editorial policy swears that all submitted papers on the subject of the journal will be carefully and objectively reviewed, and so on. However, in most cases, all these words have nothing to do with reality. As I wrote in chapter 3, I associate this situation with the fact that in the USSR the Stalinist constitution was very democratic, freedom of speech, assembly, etc. were allowed there, but everyone understood that if you want to live, then it's better to forget about it.

In fact, the situation is this. The vast majority of editors and reviewers have such a way of thinking that if it seems to them that a paper is not within the standards, then they don't even want to figure it out, but are looking for an excuse to kick the paper right away. In chapter 3 I described my vision of the reasons why this happens.

Consider, for example, my situation. In Sec. 4.3 I argue that sooner or later the fundamental quantum theory will be based on finite mathematics, and such approaches as quantum field theory or string theory are not based on strict physical principles and will sooner or later go down in history. My first work in this approach was published in 1988 in Sov. J. Nuclear Physics, followed by two large papers in the Journal of Mathematical Physics in 1989 and 1993. At that time, there were still no great difficulties in publishing papers that were not in the mainstream. Then I got much stronger results, but the situation in the physics community has changed a lot. In chapters 5-10 I described the problems with publishing my papers and how difficult each publication was. Despite the fact that I sent my papers, probably, to almost all the so-called. prestigious journals, so far has not received a single review, which would say that the approach is wrong, unrealistic, etc. The fact that it was possible to publish a paper on this topic in Physical Review D is an exception because it was just the way things were. But, as a rule, the editors tried to kick the paper even before a review, and if it came to a review, then the reviewers turned out to be not only unqualified, but, most importantly, vicious. The way of thinking of many of them was such that if a paper with finite mathematics was published, then the world would end, so they had to fight to the death to reject such a paper. I don't know if they realize what they're doing is vile, or if they think they should kill the article for some lofty scientific reason, even if they don't understand anything about it. Fortunately, in such Russian journals as "Theoretical and Mathematical Physics" and "Physics of Elementary Particles and Atomic Nuclei" the scientific level of reviewers is in no way lower, and sometimes even higher than in the so-called prestigious Western journals, and the level of scientific integrity is much higher.

In chapter 3, I expressed my point of view that the main problem in science now is the almost complete absence of any moral criteria, and that those who do not observe scientific ethics are not afraid that this will be known and their reputation will suffer. Typical violations of scientific ethics are: 1) even editors do not follow the editorial policy of their journals; 2) reviewers also consider it optional to follow these rules, as a rule, they don't even read the editorial policy because think that they know better what papers can be published; 3) reviewers give negative reviews, even if they do not understand at all what is done in the paper and do not make any attempts to understand; 4) reviewers do not admit that the problem considered in the paper can be solved in different approaches, they allow only those approaches that they understand; 5) editors and reviewers express negative judgments about the paper without any attempt to substantiate these judgments, i.e., they apparently do not understand that this is completely contrary to scientific ethics. The reader will be able to judge for himself whether the stories described above confirm this point of view. I think they fully confirm and therefore, as I have detailed, I believe, that I and the scientists listed below have different concepts of scientific ethics.

1. Gerard 't Hooft, Nobel Prize Laureate.
2. Frank Wilczek, Nobel Prize Laureate.
3. Alexander Polyakov, laureate of the Milner Prize, the Dirac Prize and other prizes.
4. Grigory Volovik, laureate of Lars Onsager Prize and Simon Prize.
5. John Heil, editor of Journal of the American Philosophical Association.
6. Bruno Nachtergaele, editor of Journal of Mathematical Physics.
7. Steven G. Krantz, editor of Notices of the Americam Mathematical Society.
8. Sven Heinemeyer, associative editor of European Physical Journal C.
9. Brian Greene, professor at Columbia University, chairman of the World Science

Festival and Chief Editor of "Annals of Physics".

10. Carlo Rovelli, Editor-in-Chief of Foundations of Physics, Centre de Physique Théorique de Luminy, Aix-Marseille University.

11. Marek Zukowski, Associate Editor Physical Review A.

12. Saverio Pascazio, Universita di Bari.

13. Michael Thoennessen, Editor-in-Chief of the APS.

14. FOM moderators:
Martin Davis
Alasdair Urquhart
John Baldwin
Harvey Friedman
Steve Simpson
John Burgess
Andreas Blass

15. Gary Mullen, Editor Finite Fields and Their Applications.

16. Terence Tao, laureate of Fields Prize and other prizes.

17. Alessandra Silvestri, Editor of Physics of the Dark Universe.

18. Diederik Aerts, Editor-in-Chief of Foundations of Science.

19. Hubert Saleur, Editor, Nuclear Physics, Section B

20. Michael Mishchenko, Editor-in-Chief of Physics Open.

21. Mark Daly, Editorial Board Member Scientific Reports

22. Enrico De Micheli, Consiglio Nazionale delle Ricerche Via De Marini, 6, 16149 Genova, Italy.

23. Anand Pillay, Editor-in-Chief of Notre Dame Journal of Formal Logic.

24. Tamar Ziegler, Editor in Chief of Israel Journal of Mathematics.

25. Sergei Tabachnikov, Editor-in-Chief, The Mathematical Intelligence.

26. Ralph Chill, Editor-in-Chief, Archiv der Mathematik.

27. Liming Ge, Editor-in-Chief, Expositiones Mathematicae.

28. Nathan Sidoli, Editor-in-Chief, Historia Mathematics.

29. Ansar Fayyazuddin, Ph.D. Associate Editor Physical Review D.

30. James M. Cline, Editorial Board, Physical Review D.

31. Jan Philip Solovej, Editor-in-Chief, Journal of Mathematical Physics.

32. Philippe Brax, editor of Physics Letters B.

If these scientists think that I am wrong, I will be grateful if they write their opinion. But I wrote appeals to the editorial offices, and they did not respond to them, i.e., they had every opportunity to respond. I write that the main reason I mention these people is because now in the scientific community, many who decide something are not at all worried that they are violating scientific ethics and that their reputation will suffer if people know about it. And, as I wrote, I think that this situation is one of the reasons for the degradation in modern physics. Also, mentioning these people and the above reasons why I included them in the list may be useful information for other scientists who are thinking about where to send their work and want to know what they can expect.

Finally, as described in many cases above, at least in relation to me, arXiv, as a rule, not only does not observe scientific ethics, but also does everything to prevent scientists from learning about my work.

When I started working on physics with finite mathematics, I, of course, understood that many would be against it and would try to do everything not to make my publications possible. Especially those for whom QFT is almost like a religion, they do not recognize anything else and believe that everything else, as they put it in ITEP, is pathology, onanism, etc. In this regard, I can recall the following story. When the question of the president of the international chess federation was discussed, Botvinnik said that Euwe was an ideal candidate, since he has no enemies. To which

Euwe recalled the saying of some philosopher that whoever had no enemies did not live. But the biggest disappointment was that many good acquaintances and even friends, who, as I hoped, would at least morally support me, did not want to do this. Fortunately, some people supported me, and I am very grateful to them. I'm afraid that if I want to list all of them, I can miss someone.

But still, I cannot but say that, starting from joint work with Leonid Avksent'evich Kondratyuk and up to the present time, all my works proceed from the idea of Leonid Avksent'evich that at the quantum level algebra is primary, and space is secondary. This idea is described in detail in Sec. 2.6. It seems to me that many problems have arisen because quantum physicists have not yet accepted this idea. For example, one of the obvious examples is how the problem of the cosmological constant and dark energy arose. As I describe in detail in Sec. 2.3, if this idea is accepted, it immediately becomes clear that the so-called the problem of dark energy is nonsense, and the problem of the cosmological constant does not exist. I also have no doubt that gravity should be considered from this idea, although not all problems have been solved here.

I am also very glad that I met Skiff Nikolaevich Sokolov. He had a great influence on me as a scientist and as a person. Eduard Mirmovich proposed the idea that only angular momenta are fundamental physical quantities. This idea and Dyson's famous paper "Missed Opportunities" gave me the impetus to study de Sitter invariant theories. I am also very grateful to Mikhail Aronovich Olshanetsky for supporting my work. And, of course, I was very lucky that I met my wife Natasha, without whom life would be completely different. I am grateful to Volodya Nechitailo, Misha Partensky and Theodor Shtilkind who read these notes and made important comments.

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