S.N. Artekha

FOUNDATIONS OF PHYSICS

(critical view)

Quantum Mechanics

The proposed book consists of two parts and is devoted to the systematic analysis of quantum mechanics and the modern theory of electromagnetic phenomena. Some inaccuracies of the application of mathematics in theoretical physics are discussed. This first part examines in detail the basic concepts of quantum mechanics, its apparatus, methods and applications and demonstrates their groundlessness, and often the internal inconsistency of the most common version of the Copenhagen interpretation of quantum mechanics. The book contains a critical analysis of the experiments underlying this theory. All this together proves that the generally accepted version of quantum mechanics is a temporary construction, and the creation of a new consistent theory of the microcosm is required. The book also discusses some alternative ideas applicable to the microcosm.

This book may be useful for students, postgraduates, teachers, scientific and technical workers and anyone interested in the basics of physics.

To the English version of the book. This is an English translation of the Russian version of the book. Quotations were also translated from Russian-language publications (therefore, for English-language works they are not a literal original, but they convey the essence of the quotations). However, references to pages or chapters have been corrected for the indicated English-language sources.

Contents

From the publishing house

This book continues the series "Relata Refero" (literal translation — I tell the story).

Under this label, the publishing house provides a platform for authors to publicly express new ideas in science, substantiate a new point of view, convey to society a new interpretation of known experimental data, etc.

In a dispute of different points of view, only the decision of the Great Judge — Time — can become decisive and final. The very process of searching for Truth is well characterized by the well-known statement of Aristotle, put on the cover of this series: the authority of the teacher should not dominate the student and hinder the search for new ways.

We hope that the texts published in this series will make, despite their deviation from the established canons, their contribution to the knowledge of the Truth.

Preface

People usually believe that it is better to err in a crowd than to follow the truth alone.
(K. Helvetius)

The great enemy of truth is often not a lie — deliberate, feigned and dishonest, — but a myth — persistent, fascinating and ephemeral.
(J. Kennedy)

During my school and student years, reality seemed naive and romantic to me: "The process of cognition — is a fascinating infinitely diverse process of searching for Truth, and people consciously and sincerely follow this path, helping each other"; "scientific progress (how beautiful and encouraging it sounds) — with its help, we will solve all the problems of the harmonious relationship between Humanity and Nature." And then, gradually, I began to notice that in many cases, science is not about the search for Truth, but about the banal competition of schools for access to finance (ordinary clan business), to the media, to means of influence, for the right to "broadcast from the pulpit." And in this struggle, the guiding principle — "all means are good". Many scientists since the time of perestroika seriously say that the main thing — is to "sell yourself" well, "present yourself and your work profitably", and "the main science — is the science of making money".

I think that this process has been gradually being prepared and has been taking place since the beginning of the XX century, when science everywhere became a "paid job", and people began to get into it not only by vocation, but also self-confident scammers. The number of scientists striving for the Truth began to decrease, and the number of highly paid researchers has grown enormously. All independent scientists, laboratories, groups, societies, scientific journals and organizations were crushed by a self-organized group according to the principle "whoever is not with us is against science." As a result of such (without exaggeration) political struggle, a powerful stratum has made its way into science, ready to "present black as white", "call white black", if only they would pay money. There were whole groups of dreamers engaged in literary creativity instead of science — unscientific fiction, not even verifiable in principle. Why don't they "stand" for election from literature? Probably, they are not confident in their talent, and the state does not act as a sucker sponsor there.

It seems that humanity is being artificially led away from real problems and from real verifiable science in the direction of trickery and complicated pseudo-science. For the first time, the author personally encountered the dominance of functionaries from science in scientific journals when trying to discuss the logical contradictions of relativity theories (the basical "theories" for all pseudosciences: [1], [5], http://www.antidogma.ru). If there is no place for discussion in science, then how does it differ from religion, where one does not argue with the postulates of faith? It is remarkable that there are many honest researchers in science who are ready to essentially discuss any complex issues concerning the foundations of science, regardless of the authorities. And there are — the majority of such people, they are simply not organized, and many are frankly afraid to express their opinion, fearing administrative prosecution or subsequent problems with the possibility of publishing their works.

The roots of the rigidity of modern science lie in the education system and textbooks, which try to bypass any "sharp corners", hide any contradictions and prevent discussions on the merits. I would like to sincerely thank Feynman for his unique approach: he tries to make the physics of the phenomenon (idea) more understandable, unlike the "chicanery" (mathematical exercises) of other theorists who dislike him for exposing many dubious aspects of the theory (and prevents them from pompously dominating). It's even better to follow J.W. Gibbs here: **"A mathematician can say whatever comes into his head, but a physicist must keep at least a grain of common sense."** In general, the author considers that the most productive approach is not theorizing, but the approach of general physics (or, more precisely, the historical approach), which allows you to take a step back at any time and make a more correct choice (without destroying the entire constructed building). It is obvious that there is nothing terrible in admitting one's own mistakes, rather, on the contrary, it is a sign of courage and professional honesty!

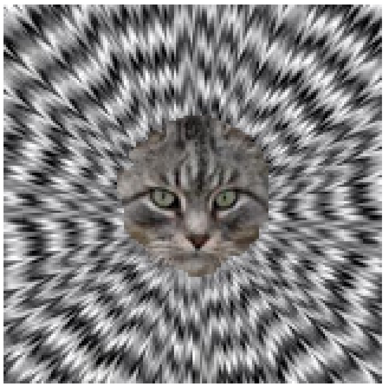
We must always remember that there is a real **World** and there are simplified **models of description** of one or another particular side of Reality. Therefore, the attempts of some "scientists" to declare a particular theory (model) a panacea for all occasions look strange (in fact, to equate the model and the Universe, it is necessary to "prove" the uniqueness and rigor of the solution and raise it to the rank of a principle). They need to look at the starry sky at least once in their lives at night to feel that we are at the beginning of the Path of Knowledge, and not at its end (and stop bragging like five-year-old children to their parents, as if we already know everything).

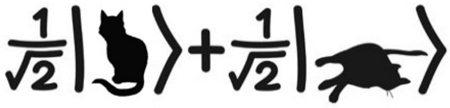
One of the key ideas of writing this book — is to explicitly voice **fundamental** problems existing in electrodynamics and quantum mechanics. Of course, in comparison with the theory of relativity, which has nothing to do with reality at all and has slowed down the development of science for a long time, these theories are more or less working theories. Quantum theory, considered in this first part of the book, gives a probabilistic description of the microcosm; electrodynamics, analyzed in the second part of the book, is an even clearer science. However, their grounds are clearly unsatisfactory. And in order to move forward, we need to determine where we really are and recognize the current state of affairs. It's time to stop "sweeping garbage under the carpet": it's useful for physicists to know about unsolved fundamental (and not just calculational!) questions to see the guidelines for further development. If our generation fails to overcome the existing difficulties, the next generations will do it, but not underground "from scratch" (and it is not necessary to "get stuck in the door", blocking the path of knowledge to others). It's funny and boring to swell up with importance like toads, pretending that we all know. Humanity is rather at the very beginning of the most fascinating path of cognition of reality, rather than at the end of it, and let's invite young people to this most interesting creative path of development.

This book sets several goals. First. Since quantum mechanics as such was born out of the problems of electrodynamics and supposedly solved them, this first part of the book will present a critique of some views (theoretical, philosophical and mathematical) of modern quantum mechanics and shows that it not only did not solve the existing difficulties, but added an even greater number of internal problems. The next goal — is to give a fairly detailed critical overview of the state of modern electrodynamics. This will be done in the next second part of the book. At the same time, there will be criticism of internal contradictions, inaccuracies and arbitrariness of electrodynamics itself (that is, its apparatus and fundamental theoretical basis), criticism of the modern interpretation of generally recognized basic electrodynamic experiments (working devices, etc.), and some (not generally recognized) experiments that contradict modern electrodynamic views will be discussed. The appendices to each Part contain brief remarks on some less common alternative theories. The author does not state his theories of the microcosm and electrical phenomena, because he believes that such works should be published in peer-reviewed journals, but a number of constructive ideas are scattered in the form of comments on both Parts of the book.

This book is intended for physicists, primarily specialists in the relevant fields, and is based on a sequence of critical comments on the most well-known (best) training courses, indicating the relevant pages. Moreover, these are not claims to specific textbooks (it's just necessary to rely on something in the logic of the presentation); the same points (ideas, techniques and methods) could be traced to other textbooks and books. The author apologizes, but, unfortunately, detailed quoting of paragraphs, formulas, drawings, etc. of the criticized textbooks, it would make a real book simply "unaffordable" neither in format nor in the possibility of publication. Therefore, although in many cases the essence of the issues discussed is clear, in some cases, when reading, it is desirable to have publicly available textbooks at hand (references in the book are usually given at the beginning of the paragraph under discussion). The author did not set out to "chew in detail" all the existing problems, but only to draw the attention of researchers to the numerous inconsistencies, gaps and contradictions of the discussed sections of physics (some key words and phrases are highlighted by the author with bold, with quotations, or marked with an exclamation mark). Therefore, a significant part of the comments is given briefly (theses), but, in principle, the comments could be expanded. In order not to frighten physicists too much and not to immediately alienate specialists, and not to cause anathema from fanatics of faith, the book adopted rather diplomatic forms of doubt about the validity of existing theories: "questions remain", "it must be proved", "it remains unclear", "it's not the same", "strange", "noteworthy", "doubtful", "unknown", "is alarming", etc. If only they would pay attention to these signal phrases and think on their own. Then only there will be hope that the matter will get off the ground. So, let's start discussing quantum mechanics. To a good way of knowledge!

Introduction





We should reject what seems false and shaky,
even if we have nothing to replace it with.
Delusion remains a delusion regardless of
whether we put truth in its place or not.
(Voltaire)

Since school education is clearly not enough for studying quantum mechanics, we will not touch on popular science literature on this topic, but will focus on textbooks intended for people who have chosen physics as their main specialty.

A vague "presentiment" of dissatisfaction with quantum mechanics begins to manifest itself from the first introductory chapters on quantum behavior [2, end of Chapter 37-1], where Feynman directly says that "we cannot explain how it works, but we will just tell". The suspicion immediately creeps in that there is simply no physical theory explaining the causes and revealing the mechanisms of phenomena.

A similar statement in [3] the fact that there will be no detailed analysis of experiments in the book is also alarming: it turns out that we will be told only what does not contradict the "required" the theories, and will be told so that we do not learn about possible exceptions and alternatives.

Planck's formula $E = \hbar\omega$, which is considered to be the basis of quantum mechanics (and its beginning), cannot be related to reality, since otherwise infinite energy would be obtained for any finite section of the experimentally (!) observed continuous spectrum. Let's take the sum of the energies at the ends of the selected frequency segment, add energy in the middle of this segment, then add energy again in the middle of the two new resulting frequency segments, etc. As a result, it is possible to obtain energy greater than any predetermined value.

Also, doubts about the correctness of quantum mechanics are fueled by the well-known paradoxes of this theory, for example, "Schrodinger's cat". How can the superposition of living and dead states be taken seriously? Where did that "live cat's share" or that "dead cat's share" that existed before the lid was opened go? A similar situation will occur in all cases when irreversible states are present among the possible states. And the phrase that it is necessary to observe not the lid, but the radioactive source, is not a panacea, since Nature in general, as a rule, dispenses with any observer, and the processes nevertheless occur (whether we look inside the "black box of Nature" or not, the results will be equally objective!). And then, quantum mechanics provides a good alibi for murderers: I'm not to blame, since I shot without looking, I'm not guaranteed to get there, so the one who discovered the body and "reduced" it to the state of a corpse.

The Einstein-Podolsky-Rosen paradox is less serious (one should hardly expect from **non-relativistic** quantum mechanics a description of high-speed phenomena or phenomena on large scales). However, the type of equations used in quantum mechanics still adds its own problems. For example, it has been repeatedly discussed in quantum mechanics that sub-barrier tunneling occurs instantly. It turns out that it is possible to pick up a chain of barriers (resonant) in such a way that the particle will fly all the way much faster than if there were no barriers at all. This is equivalent to the fact that the same athlete would run along an ideal track slower than if this track was repeatedly blocked with concrete walls and he would have to break through all these walls (absurd)!

So, not everything is in order “in the Danish Kingdom”, and there may be serious questions about quantum mechanics. So let’s start a detailed analysis of this theory.

Chapter 1

Basic concepts of quantum mechanics

So, I'v racked my brains more than yours, and my thought experiment refutes your thought experiment

Let's start the discussion with the uncertainty relation, which supposedly solved the electrodynamic problems of the atom and revolutionized the basic concepts of mechanics (abolished the concept of trajectory).

Using the concept of a quantum mechanical uncertainty relation, which characterizes only the "external" uncertainty associated with the measurement process, to "explain" the unattainability of the nucleus by an electron — is just a more complex paraphrase of this unattainability and nothing more. In fact, if we believe that with any accelerated motion, an electron loses energy in the form of electromagnetic radiation (flying away from an atom at the highest speed — the speed of light), then the average (mathematical) of states losing energy will also be a state in which energy is lost, that is, quantum mechanical averaging here does not save the situation (in physical essence).

It is surprising (!) the mental "experience with electrons" [2, chapter 37-4] flying through a plate with two holes. Usually thought experiments are built in such a way that logic suggests the only (!) possible answer (this is their essence). In this case, they make a conclusion not by the type of "yes-no", but "extract" a functional dependence for probability (which can be an infinite number of options!), and Feynman says at the same time that no one has ever put such an experience! Naturally, because the distance between the holes should be comparable to the size of an atom. Then this non-existent experience is further complicated by new fictional details, for example, illumination, and a result is composed **required** to substantiate quantum mechanics.

At the same time, the concept of the objectivity of the results of the experiment (even if it is mental) is overlooked. According to the description, it was "established" that the electron passes **either** through hole 1, **or** through hole 2. This fact is established already **after** the electron has passed into one of the holes. And, according to the **causality principle**, our **impact** ("peeping" behind the electron) **"after"** could not change the **objective state "before"** (i.e., the **already accomplished** fact of the passage of an electron into a certain hole). So, even without "peeping", the probabilities would have to add up. In the "experience" it is stated that this is not so: there is interference. Therefore, the point here is not in our influences and not in the "special properties" of the electron. In fact, it was necessary to explain the presence of interference in the experiments (that is, from a mathematical point of view — the rule of addition of complex probability amplitudes). And this is much easier to do. The first thing that comes to mind is the presence of a medium or ether in which disturbances will propagate in the form of waves (for example, with macroscopic interference on water, its molecules on average make circular movements in the wave and each molecule also passes through a specific hole). If you really don't like the ether, then remember that the electrons are charged and when they move according to modern electrodynamics, there is also a more rapidly propagating field that "feels" both the configuration of the experiment (device) and neighboring particles (again interactions through wave perturbations of the field). The uncertainty principle in this case may not be related to objective laws at all (just as the real finite accuracy of all classical devices without exception does not prevent us from making mental extrapolations to the exact laws of physics), but may reflect only one of the possible ways of our interaction with the outside world.

Feynman in [2, chapter 38-2] describes an "experience" when particles from a very distant source (that is, having no vertical component of momentum) pass through a slit of thickness B and a diffraction pattern appears. He says that quantum mechanics and the uncertainty relation relate to the possibility of **predicting** events in the future. But this is not the case in the modern view: quantum mechanics asserts that there is no **concept of trajectory** for particles at all (that is, the simultaneous existence of coordinates and momentum of a particle). But in this experiment, we have fixed the position of the particle in the slit and can find out its vertical component of the pulse (by calculation), knowing (with the help of a detector) where the particle got into the interference pattern. Consequently, a certain trajectory of the particle still existed. And the finite accuracy of real measurements has nothing to do with establishing strict laws, otherwise it would be necessary to admit that there are also no mathematical concepts of points, lines, planes, strict laws of geometry and classical physics (which have also always been confirmed with **finite** accuracy). But since nothing is measured exactly, then it is necessary to discuss in physics not what is "measured exactly", but what can be measured by physical devices at all, even with some finite accuracy (and this is not the same thing!). But we can fully agree with Feynman's remark that in classical physics the finite accuracy of practical measurements also greatly limits the accuracy of predictions.

The continuity equation for probabilities (preservation of local probability) proves the uniform nature of physical laws (that is, there is no difference between quantum mechanical laws and the laws of classical physics) and indirectly demonstrates the presence of the usual classical trajectory of particles in the microcosm.

The Landau-Lifshitz quantum mechanics course [3] (considered one of the best theoretical textbooks) can hardly be attributed to an objective scientific book that reveals the pros and cons of the theory and gives an analysis of physical moments, but rather it refers to a good apologetics of this theory (i.e. it is intended to create an army of believers ready to go in orderly rows in a certain direction chosen by someone). From this position, all supposedly "physical" proofs of the "objective necessity" of replacing classical physics with quantum mechanics are constructed. So, in the first sentence [3, p. 1] it is said that "classical mechanics and electrodynamics lead to results that are in sharp contradiction with experience". But let me, why not assume that these were contradictions to the planetary model of the atom, or the model of point electrons and nuclei (after all, in addition to mass and charge, they also have a magnetic moment and spin), or to some specific formulas of electrodynamics? Where is the limit of permissible hyperbolization of the alleged "crisis" (so, it would be possible to reach the "wall" at all and declare the crisis of the most scientific description of reality)? In fact, the energy of mutually moving magnetic rotators can have minima in the classics (the movement will coincide with the forceless movement). And not with every curvilinear motion, charges are obliged to radiate: movement along forceless trajectories (and not just uniform rectilinear motion) is not accompanied by radiation. Only when an external nonelectromagnetic force causes the charge to move along a forced trajectory (that is, with acceleration), then radiation is observed. So there are no “deep contradictions” at all, and fundamental changes are not necessary (at least, the example given in the textbook does not prove anything).

Next — another fraud — Landau “puts the horse behind the cart”. He cites electron diffraction as another “proof”, but it was “discovered” after the creation of quantum mechanics, and therefore its “explanation” was conducted immediately without alternative within the framework of this theory (and, therefore, this phenomenon cannot serve as proof without proving the impossibility of other explanations). A subsequent mental (!) experiment (and not a real experience! and not a logical refutation) with diffraction at two slits especially cannot serve as proof of anything. It is in this deceptive way that we are led to the postulate of quantum mechanics about the absence of the concept of a particle trajectory — the so-called “uncertainty principle”. Stop! Another fraud: this principle was artificially invented after the creation of the mathematical apparatus of quantum mechanics, that is, it is only one of the possible physical interpretations for the results of mathematical operations in quantum mechanics (far from the only and certainly not the best interpretation). It remains completely unclear how the absence of a smooth trajectory answers the first physical question of the textbook in question about the absence of radiation by an accelerated moving charge in an atom (we are not talking about mathematics!)?

The absence of self-sufficiency of quantum mechanics (on the one hand, the statement about the new non-classical nature of the laws, and, on the other hand, the need to rely on these very classical concepts and measurements, that is, to adjust to the classical "reference points") also speaks of the limitations of the generally accepted interpretation of quantum mechanics.

The advertised so-called "deep role of the concept of measurement in quantum mechanics" is perplexing: what if, without measurement, the physical laws in the microcosm cease to operate (after all, in the classics, if you constantly open the kettle, it may also not boil, but this example does not cancel the objectivity of the laws of thermodynamics)? And what about the possibility of calculating the dynamic characteristics? For example, even quantum mechanics does not forbid measuring exactly the coordinate of an electron at some point in time (flying through a point, electron-sized hole — see Fig. 1.1), and then put sensors on the ball surface away from the hole and simply record the time of arrival of the electron there. Therefore, by calculation, we will be able to find out what speed the electron had when flying out of the point hole. Thus, there is no need to replace the concepts: the particle still has all the dynamic characteristics at the same time (and the question of whether they can be measured simultaneously — is a completely different question, not concerning the need to change the classical representations).

And the question being raised about the non-existence of velocity in quantum mechanics (a kinematic concept!) in the classical sense, — causes complete disappointment in such a theory. The hypothesis of the simultaneous non-existence of exact values of coordinates and velocity of particles contradicts the real possibility of the existence of resting particles. And without this concept, no theory can budge, including quantum mechanics itself. "The impossibility of simultaneous measure-

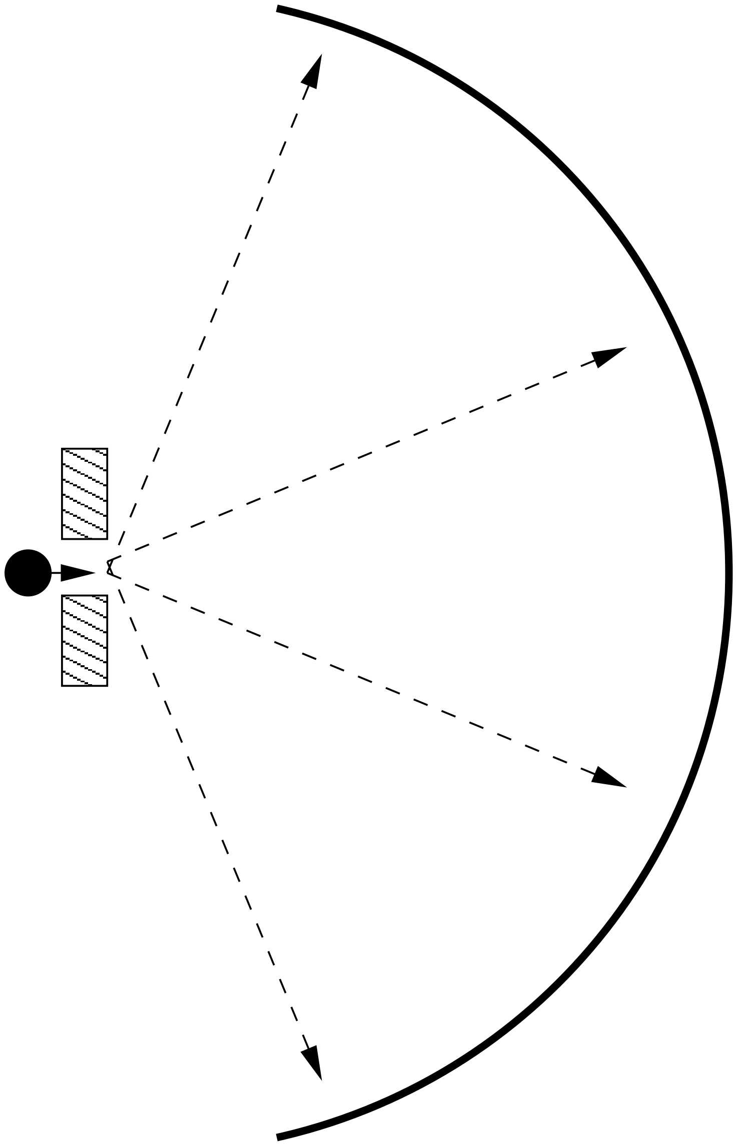


Figure 1: To determination of the coordinates of the electron.

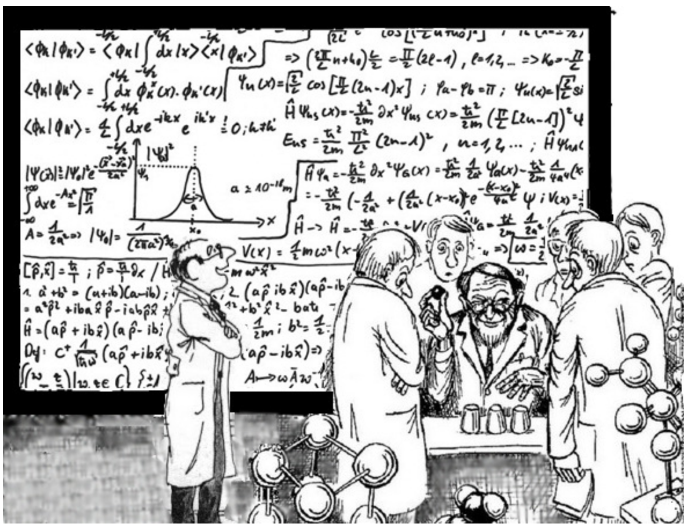
ment" and "non-existence" are not the same thing at all! In classical physics, too, it often happens that by measuring one quantity, you prevent the simultaneous determination of another property. But this does not mean that these properties are not objective for an object or they cannot be simultaneously attributed to an object (for example, it is impossible to simultaneously measure the melting point of a wire, the temperature of transition to a superconducting state, its tensile strength, or the critical pulse current that explodes the wire; and many similar examples) for one sample.

The claims of quantum mechanics for its peculiarity are completely incomprehensible: in terms of practical significance, not only quantum mechanics, but also classical statistical physics provides a probabilistic description of processes. But try to say in the classics (in statistical physics) that "if the measurement does not give an unambiguous result with certainty (exactly!), then the value does not have a certain value", and you will be laughed at. At least because any quantity is not measured with absolute accuracy, and there are always fluctuations, but a specific measurement always gives a specific result, and that's enough.

Thus, the basic concepts of quantum mechanics, claiming to be revolutionary, have a very shaky basis.

Chapter 2

Wave function



Let us now turn to the discussion of such a key tool of quantum mechanics as the wave function. What does the wave function describe? It cannot describe real particles, because, firstly, wave packets (like any waves) pass through each other, but particles collide with each other (and bounce off), and this is experimentally observable! And, secondly, wave packets deliquesce so quickly that the supposedly existing "classical problems" with the stability of orbits (electron radiation) would seem like "flowers". Thirdly, any measurement gives a specific experimental point (one!). So, the wave function has nothing to do with one individual particle! Apparently, it can only relate to the ensemble of particles, statistics, that is, to the "average temperature in the hospital", and this "temperature" of $36.6°C$ does not guarantee in this case for the "individual patient" (for theory) – health. Quantum mechanics boasts that it works with observable variables and only this, they say, makes sense. But after all, for example, when determining the energy levels of an atom, they deal with radiation frequencies, and in this case one of two — either the atom is still in a specific state, or the radiation frequencies are already there when the atom is no longer the same (not in the state that it was!). And in the atom itself, nothing fluctuates with the radiation frequencies, that is, quantum mechanics often deals with variables that are not observed in the state under study, contrary to its own ideology.

Conceptual issues can also be connected with the spatial distribution of particles (wave function). For example, for an atom: the electron distribution functions are continuous from $r = 0$ to $r = \infty$ regardless of which of the atomic levels the electron is located at; if the function definition areas were different, there would be no problems, but this is not the case, and then where does the electron that has moved away to some distance $r \neq 0$, can "know" to which energetic level it belongs (or which of the distribution functions it obeys)? The question is even broader: how does an electron know to which atom it belongs? It turns out that transitions between atomic energy levels (and even "teleportation" between different atoms at any distance!) can occur in any direction spontaneously and instantly? What about causality then?

It is hardly possible to say in a strict sense that quantum mechanics uses classical probability theory (the methods of which are sufficiently transparent for ordinary common sense). Indeed, it is not possible to give a physical meaning to the probability amplitude itself (as well as to explain why we take for probability **the square of the modulus** of the amplitude, that is, its product by a complex conjugate quantity). Therefore, in the strict sense, the phrase about a probabilistic quantum mechanical description — is a plausible deception.

In fact, the beginning of the book [4, Chapter 3-1] resembles a game of "what looks like what": for an artificially invented (mental) experience with the passage of electrons through two slits, it is "necessary" to artificially obtain an interference pattern, and Feynman simply postulates three "**general** principles of quantum mechanics", the application of which gives this "necessary" picture (without finding out from experience whether Nature is like that). All this resembles the well-known rules for adding and multiplying probabilities, only it is carried out on complex amplitudes. And after that the game with "three cheating cups" continues, when it turns out that the vaunted principles are not general at all. So, the "second cup" appears when describing scattering on a crystal [4, Chapter 3-3]. Now it turns out that we cannot represent neutrons in this experiment in the form of waves. The dependence of the neutron count rate on the angle contains either sharp peaks (for some reason with complete dips!), that is, "not quite" according to theory, or for other crystals — plus an almost uniform background. When describing scattering without a spin flip, the dependence of the scattering amplitude on the angle is not taken into account, but in order to get to a given place in space (counter), scattering from different atoms must be at different (!) angles. When describing scattering with a spin flip, for some reason, collective effects (spin waves, for example) are thrown out. And finally, the "third cheating cup" is manifested when describing the scattering of identical particles: for electrons, the probability amplitudes should be considered not adding up, but subtracting (and that's the general principles?!). Thus, we have the usual fitting of the theory to a previously investigated dependence, and the entire subsequent description of quantum mechanics will, in essence, be just putting verbal labels on these "three cheating cups".

From a physical viewpoint (more precisely, from the viewpoint of quantum mechanics itself), it is unclear how it was possible to localize the particles depicted in textbooks in one plane (for example, XOY, that is, $dz = 0!$), because then the projection of the momentum along the z axis will be infinite, not zero (and the probability of movement in this very selected plane will be zero!). However, descriptions of particle collisions in quantum mechanics are also carried out for one plane (and this is — not classical physics!). In addition, experiments show that all particles do not have point (zero), but finite dimensions, therefore, even from the classical viewpoint, the probability of particles moving strictly in the same plane [4, Chapter 3-4] before the collision is zero. The probability of moving along non-intersecting skew straight lines will not be zero. And this motion cannot be made planar by any choice of the observation system, and, consequently, **three-dimensional** scattering will be observed. And, as a result, all reasoning, even for two particles, ceases to be strict, for example, that $e^{i\delta} = \pm 1$ [4, Chapter 4-1]. So the scientific-like phrase about interference in the "phase" or "antiphase" for the collision of identical particles — is nothing more than a game of "what looks like what" (or a mnemonic rule).

Further, in classical physics, the rules of addition and multiplication of probabilities work successfully and, knowing the results for two particles, in principle it is possible to obtain results for any number of particles by induction. But how quantum mechanical probabilities behave during transitions to an arbitrary number of particles is unknown in advance, since they do not obey common sense, and not probabilities are added, but amplitudes that have no physical meaning. For example, it would be necessary to check: how the conclusion that $e^{i\delta} = \pm 1$ (approximately) from considering the situation for two colliding particles can be transferred to three or more particles. Again, for three or more particles, the scattering will be three-dimensional; in addition, various spatial configurations of the problem of multiple collisions are also possible. The number of possible manipulations with phase multipliers is also increasing. Moreover, since the trajectory of particles, and hence the time of movement along it, is indeterminate in principle, then how can we be sure at all for particle flows that we fix particles belonging to the same scattering act in experience? And according to Feynman's statement [4, Chapter 3-4], a number of cases also require an additional approach (scattering of unpolarized particles or scattering of bound particles, etc.). So the generality of the theory is under great suspicion.

When considering the scattering of two identical Bose particles on two **different** scatterers into the same state [4, Chapter 4-2], a "natural suspicion" arises that we are dealing with an obvious **resonant** effect (forced scattering of the second particle from the first — analog of forced or induced photon emission), which increases the corresponding probability. The phrase about a "photon in the final state" looks especially strange: if a photon has flown at the speed of light into open space, then it will not be able to "report" its further state in any way, and in a closed space its characteristics can constantly change during reflections. Therefore, we will be talking only about **simultaneous** (with a passage) induced radiation. But there really is no "quantum mysticism" here: the more stones "roll" down the mountain, the greater the probability of a rockfall (analogous to spontaneous + forced radiation). From the same point of view, the spectrum of an absolutely black body can be understood and derived classically. However, there are also questions about this concept [4, Chapter 4-5]: often a cavity with a hole is taken as a model of an absolutely black body, but then what do the states of atoms N_{base} and N_{exc} mean in this case? Is it not necessary in this model to take into account for real substances the effect on the spectrum of their density (the ratio between the volume occupied by atoms and the empty spaces between them; the collision mechanisms of the excitation of the atom and the induction of light emission)?

Generally speaking, the concept of an absolutely black body is "original": this is a not existing in Nature (that is, it is not experimentally verifiable!) body that would strictly obey the fictional theory of radiation of exactly **of such a fictional** body.

The brief paragraph on liquid helium [4, Chapter 4-6] does not stand up to criticism: the "explanation" of the superfluidity of helium as a quantum effect for Bose particles looks too strange. What is this "explanation" related to the multiplier $\sqrt{n+1}$? And in which area to count the number of particles n: near, or in the whole vessel, or plus a neighboring vessel, or in the whole Universe? Where is the quantitative calculation of the volume of mutual influence of particles? And what about other bose particles (for example, other inert gases) if we pump them into this vessel under **low** pressure? Where is the peculiarity of helium visible? But what about the "theory" of a two-component liquid? In general, it is strange to have several **"theories"** for one particular phenomenon in one particular substance that do not give practical (quantitative) returns.

With the behavior of fermi particles during scattering, there are also uncertainties in the description of the "exclusion principle" [4, Chapter 4-7]: what does "a certain direction and a given direction of spin" mean for particles? After all, the relative position of the particles is not determined in the experiment at all! For example, for the well-known classical model of two magnetic arrows: if they are placed side by side (side by side), they will tend to have opposite directions, and if they are placed sequentially behind each other, then their directions of magnetic moments will coincide. Thus, there are both options and all understandable and well-known. They themselves talk in quantum mechanics about the uncertainty of the position of a particle, and they themselves try to put two particles in one place. If we are not talking about one place in space, then what are the distances: angstroms, meters, parsecs and where is this reflected in the formulas?

Reasoning about the role of the "prohibition principle" in the large-scale stability of matter [4, Chapter 4-7] (why "atoms of matter do not collapse") looks naive, since it is unclear, for example, why hydrogen gas "does not collapse" at low temperatures, since the electrons in the molecule have joined in pairs and attract protons, being located between them.

Feynman [4, Chapter 4-7] admits that there is no quantitative explanation of ferromagnetism. But there are also problems with a qualitative (indirect) description: it does not matter at all whether the electron spins on each shell line up in the opposite direction or the inner electrons line up opposite to the free electrons, since the sum of the magnetic moments in both cases will be **equally** close to zero. So the real cause of ferromagnetism is unknown.

With nuclear forces, it is also unclear why there is no atom He^2 or n^2 (– the latter even more so!), because magnetic moments could be placed side by side ($\uparrow\downarrow$), and not sequentially one after another, that is most likely, there is no notorious isotopic invariance of nuclear forces.

It is obvious that there arise many questions to the chosen by Feynman path [5, Chapter 16-1] for the limiting transition from a discrete lattice to continuous coordinate values when determining the spatial dependence of the probability amplitude. The first limitation arises from the choice of zero energy: "in order that $(E_0 - 2A) = 0$". Indeed, if there will be one E_0 for one system (atom, molecule, etc.), then another E_0 would have to be chosen for another system and it would not be possible to consider them together (to agree on zero energy). The second limitation arises from the fact that we consider only small k. It is in this approximation that the used expression for the energy

$$E = (E_0 - 2A) + Ak^2b^2$$

is correct. Even with $b < b_0,\ b_0 \to 0$, it is always possible to find such $k > k_0,\ k_0 \to \infty$ so that the exact energy value differs significantly from the approximate one. Third, the choice

$$\lim_{b \to 0} Ab^2 = const$$

is arbitrarily invented (with the same success, any function could be instead of a constant). It is not obvious that it is always possible to introduce **constant** effective mass (in general, it may turn out to be a function).

It is noteworthy that in calculations in quantum mechanics, Feynman often uses the technique of arbitrary choice of zero energy. But in [4, Chapter 7-1], he even had to take the dubious value $E = mc^2$ as energy for "justification"' dependencies $\exp[-iEt/\hbar]$ (otherwise, if E can be arbitrarily changed, then this dependency would not express anything, since it would become undefined). Expression for the detection amplitude at the point x of a particle with a given momentum

$$\exp\left(\frac{ipx}{\hbar}\right)$$

has the same invented “rigor” (it has not been proven at all that it is evenly spread over the entire space and does not depend on other conditions of the problem).

From the fact that we introduced two probability density functions — for coordinates and for momentums — and found a connection between the half-width of the distribution of p and the half-width of the distribution of x [5, Chapter 16-3], it does not follow at all that the same particle does not exist at the same time the specific value of the coordinate and momentum, that is, the trajectory. This is in quantum mechanics — **additional hypothesis**. Similarly, you can enter the density of distribution of people on Earth by weight and height, but this does not refute the fact that each individual has a specific weight and a specific height.

When Feynman discusses the meaning of the wave function [5, Chapter 21-4] for macroscopic systems, it turns out that for photons it simply coincides with classical physics, that is, no special quantum physics is required. But for electrons, he writes: “for a very long time it was believed that the wave function of the Schrodinger equation would never have a macroscopic representation ...”. However, only the seemingly kind idea to extend the quantum mechanical wave function to macroscopic phenomena and consider it directly measurable pursues a hidden goal — at any cost to confirm faith in quantum mechanics. Because **for scientific verification** of this quantum mechanical **hypothesis**, strictly speaking, it is necessary to compare experimental data and the **theoretically calculated exact multiparticle** wave function (which is not yet possible to calculate even in fantasy).

It is noteworthy when on page 7 in [3] it is said that **“fundamental ambiguity”**, associated with the presence of a “phase” multiplier ($e^{i\alpha}$) is “insignificant, since it does not affect any physical results”, and then on page 37 it is exactly through this phase multiplier

$$\psi = a \ \exp\left(\frac{i}{\hbar}S\right)$$

a transition is made to **unambiguous** results of classical physics. So the algorithmicity of quantum mechanics is clearly not “on top”.

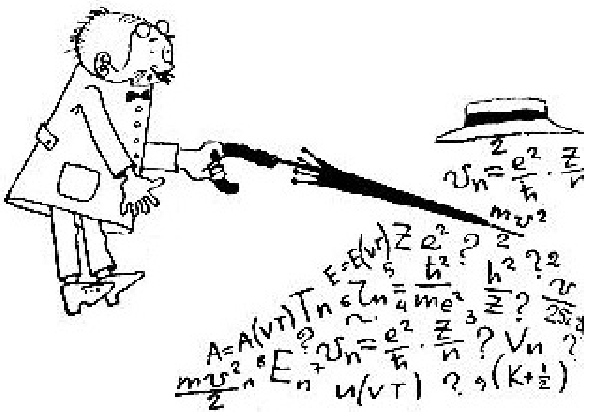
The declared “positive content of quantum mechanics” [3, p. 7] about the construction of a linear combination of wave functions for different states is very strange (and why is it called “positive”?). We decided, for example, to investigate some substance (its spectrum, structure, or other physical properties) and begin a “combinatory games with cubes” – with wave functions. Stop! And are they known to us in advance? No! We need to find it, but how (if only hydrogen is solved to the end, and then not exactly, but with a shift!)? Yes, take and measure these very desired properties! And why, then, after all these studies, also count functions (especially since even counting them very approximately — is a very time-consuming and simply task with fittings)? Thus, all the vaunted “positive content” is just a technique to “retroactively” direct research into a pre-selected track of quantum mechanics. And in general, the principle of superposition, which requires linear equations (linear World) is clearly a simplification of Nature (that is, a model).

For the resulting sometimes non-normalized (non-normalizable) functions, we are offered in quantum mechanics (probably as a half-measure) to determine the relative probability with respect to the values of $|\psi|^2$ at two points. But this is a pure hypothesis, which should at least be checked "for reasonableness" every time.

Thus, the main tool of quantum mechanics, to put it mildly, is not entirely reliable.

Chapter 3

Energy, momentum, force



Since quantum mechanics, like other new-fangled theories, is almost not engaged in substantiating the physical meaning and consistency of the concepts and quantities used, and often just pronounces the same verbal names as classical physics, then one should pay more attention to its verbal rhetoric and the concepts and quantities used. We start with the concept of energy.

In the paragraph "Atoms at rest; stationary states" [4, Chapter 7-1] there are too noticeable unfounded statements and postulates. Let's start with the following quote. "A single electron in empty space (1?!) can under some conditions (2?!) possess (3?) a well-defined (4?!) energy. For example, if it rests ...". Firstly, if it is alone and there is nothing else, then the question is, who and relative to what can measure his characteristics? Secondly, how does this "some" condition $\mathbf{v} = 0$ differ from any other specific "some" condition $\mathbf{v} = \mathbf{v}_1$? Thirdly, why on earth and since when has the word "possess" become associated with the word "forever"? If somebody had a car, then really after the accident, the culprit could claim that he did not possess it at all (a convenient excuse before the traffic police)? And at the time of the collision, did the car also not have a certain speed (and energy)? Let's see what the traffic cop and the second participant of the accident will say to this! And fourthly, when,statements are made about energy, you can laugh a lot: "By the energy of E_0 we mean the mass of all this multiplied by c^2" (did someone feel better from a false oath?!). How do you like these relativistic nonsense, given that at the end of the paragraph Feynman will say that in quantum mechanics "we have the right to shift our zero energy very, very much, and it still doesn't change anything". Is it really necessary to tie a web of quantum mechanics with the theory of relativity? But the web is ephemeral! Next, a **postulate** is introduced for the amplitude dependence $a\ exp[-i(E_0/\hbar)t]$: "just assume that this rule is always true". Of course, there is no justification for the postulate, since the dependence could well be as follows: $a\ exp[-iF(E_0, t, \ldots)]$ with an arbitrary function F, including a nonlinear one. And since the amplitude itself has no physical meaning, the quantities $\omega, E_0, \hbar$, etc., which are newly introduced into the amplitude, have the same "rich" physical meaning (in this particular definition $\hbar\omega = E_0 = Mc^2$).

With the involvement of the "uncertainty relation" there are also uncertainties: it turns out that a particle can rest only if it is the only one in the entire Universe ($\Delta x = \infty$). If we shield this particle (for example, for 1 second with a closed box the size of 1 light-year, or with a second particle), then this particle will not be able to remain at rest in any way, since then inequality should be $\Delta p \neq 0$. Thus, the particle **immediately** loses the ability to have **a certain velocity** $\mathbf{v} = 0$, or any other velocity $\mathbf{v} = \mathbf{v}_1$.

The paragraph "Uniform motion" [4, Chapter 7-2] causes nothing but laughter: "If we assume that the theory of relativity is correct (?!), then a particle resting in one inertial system in another inertial system may be in uniform motion". But is it from ordinary common sense (or from the Galilean principle) shouldn't this? What does fanatical belief in the theory of relativity have to do with it?! How is it possible to "cram" Lorentz transformations into **non-relativistic** quantum mechanics and talk further about points of equal phase, if in quantum mechanics the phase is arbitrary at all (and has no physical meaning)!

It would be possible to simply introduce a mathematical replacement of

$$\omega t \to \omega t - kx,$$

and from the dimensions to determine the meaning of k (since **is postulated** the need to introduce the value $\hbar$, then this can be done unambiguously). The introduction of a physical meaning for the velocity of a particle as a group velocity of wave beats (with superposition) is **postulative**. Feynman honestly admits at the end of the paragraph: “we secretly added **one more** hypothesis: in addition to the fact that

$$\exp[-(i/\hbar)(W_p t - \mathbf{px})]$$

there is a **possible** solution, we assume that the same system may have other solutions with all possible $\mathbf{p}$, and that different terms will interfere”. That is, quantum mechanics tries to create the **illusion of real motion** (observed!) as a composition of stationary states (at rest!). And even with such artificial “sucking out of the finger”, they secretly slip us postulate after postulate.

Now about the potential. Pseudo-arguments about a particle inside a box with a constant potential [4, Chapter 7-3] are clearly superfluous here, since in classical physics the potential does not have an independent physical meaning (recall that only the potential difference makes sense), as well as in quantum mechanics the addition of an arbitrary phase (which does not make sense) to all amplitudes does not lead to any observed changes. Thus, just **another postulate is introduced** that the amplitude is proportional to

$$\exp[-(i/\hbar)((E_p + U)t - \mathbf{px})]$$

with an arbitrary value U, which may depend on the coordinates.

The next **postulate** is taken "from the ceiling" that "changes in the amplitude (that is, its phase) have the same frequency everywhere". And the fact that this postulate leads, when equating frequencies to an expression that **by letters** coincides with the law of conservation of classical energy does not prove anything and can hardly delight, since earlier in quantum mechanics, the motion of particles was determined not through the characteristics of the **phase** velocity, but through **group** speed!

It is also necessary to understand the limitations of the model of quantum mechanical barrier itself (which is considered to be stationary), since in reality the interaction potential (barrier) is determined by the continuously moving particles themselves, therefore, it is "breathing" (oscillating). Consequently, resonant passing, energy and momentum transfer to a single particle (for example, in a uranium atom) are possible, which may well be observed in classical physics (thus, the opposition — what can be in classical physics, and what cannot be in it — again does not correspond to reality).

It is noteworthy that "high theorists" seem to constantly want to belittle the role of classical physics (such behavior can hardly elevate a real scientist). Feynman calls the approximate coincidence of the results of quantum mechanical motion **with a slowly and smoothly changing potential** the classical limit. But in the classics, the potential could be arbitrary! Where was the transition to such specific allegedly classical conditions carried out? There are only a formal approximate coincidence of the final results. But there are a lot of such supposedly "general theories" that can be invented.

There arises also a number of questions about the "precession" of the particle [4, chapter 7-5]. Since quantum mechanics claims a fundamentally different way of describing phenomena (different from the classical description), we *a priori* do not know at all what the quantum mechanical magnetic moment "μ" is. In fact, the situation is reversed and "translated into a normal language" would sound like this. The probability of detecting a particle in a certain state undergoes periodic changes and depends on the classical (!) magnetic field $\mathbf{B}$, so let's denote some quantity included in the product with $\mathbf{B}$ by the letter "μ" (in dimension, namely: $i\mu\mathbf{B}t/\hbar$).

Questions to the experience with muon decay itself (judging by the introductory word "let's assume" — it was another thought experiment):

1) How reliable is it that muons turn out to be polarized? It would be nice to add a description of experiments to verify this property (and investigate all possible causes of the phenomenon) and graphs of the probability distribution of a particular polarization.

2) Does the deceleration in the substance A not affect the orientation of muons at all?! This is obviously strange (even if there is an earth support, colliding cars turn around and turn over), and it would be worthwhile to give graphs of changes in the polarization of muons during inhibition.

3) Is it really only the field $\mathbf{B}$ affect the process and only affects muons, and there is no influence at all on the departing

electrons?! In what experiments have these facts been verified and with what probability?

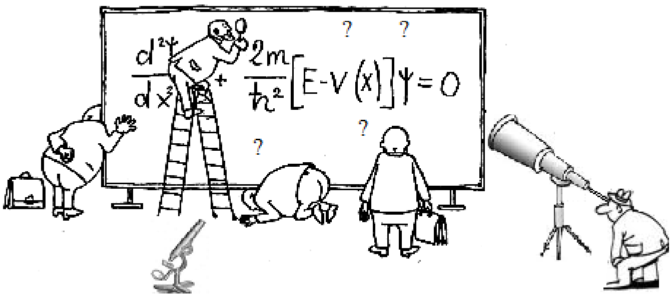
4) It is stated that "**at high energies** electrons are emitted predominantly in the direction opposite to the direction of the muon spin". Perhaps. But in this experiment, practically resting (!) particles emit, and the result of this really **another** experiment can be **different**. Where and by whom has the possible dependence on speed been verified?

5) In order to assert the influence of the value of the field **B** on a certain quantity μ, it is necessary that this last value **can be measured independently** from the studied influence of the **B**-field, but all the previous description of the textbook suggests that this value is also measured using the **B** fields themselves. Thus, there is no way to independently verify the proposed **regularity**, and it again turns out to be just a new postulative **definition** (remember A. Poincare's statement about the difference between a law and a definition).

So wherever you go in quantum mechanics, there is a lot of tension with the justification of physical concepts and quantities, and all quantum mechanical concepts and interpretations are "sewn with white threads".

Chapter 4

Schrödinger equation



Let us now turn to the basis of the fundamentals of quantum mechanics — to the Schrodinger equation. We can agree with Feynman's opinion regarding Bohr's explanation that "the reason why electrons move in this way remained a mystery" [5, Chapter 16-5]. But the Schrodinger equation also added only quantitative **calculation possibilities**, without eliminating all the contradictions and complex issues that existed (that is, without solving the fundamental problems of explanation). The "smearing" of an electron with some probability around the nucleus does not cancel the question: how can the average between states **with continuous energy loss** on radiation give a stationary state with constant energy?

In quantum mechanics, the question that was raised back to Bohr's "theory" has not been canceled: radiation with a given frequency lasts a finite time, and it turns out that **having started** the transition from one level, the electron must already know in advance **at which level it intends to stop**. We, for our part, can only "help" this electron in the freedom of its choice also by choosing in advance the transition from which level to which we will count (retroactively ensuring the coincidence of our "choices"). From the author's point of view, the whole depth of the voiced problem lies in the insufficiency of the state characteristics proposed by quantum mechanics. If, for example, an electron is in an atom at the hundredth level, then since the radiation (already with a specific frequency!) starts from the self of this level, therefore, already at this hundredth level, the electron has the opportunity to possess at least ninety-nine different distinguishable properties. Why isn't this proof of the existence of hidden parameters?

From a physical point of view, there is also the following problem: stationary solutions are obtained only with **strictly specific choice of energy**, which means that the resulting solution **is unstable** with respect **to any infinitesimal perturbations**. Why is this suddenly the main problem of all computational physics — the problem of stability of solutions — does not concern quantum mechanics at all? Apparently, it is incorrect to consider any calculations in quantum mechanics without a full balance of energies (radiations).

With the "introduction" of the Schrodinger equation in [3, p. 50], a solid comedy turns out. First, for a system of non-interacting particles, they write

$$\hat{H} = -\frac{\hbar^2}{2}\sum_a \frac{1}{m_a}\nabla_a,$$

where "∇_a is the Laplace operator in which differentiation is performed by **the coordinates of the a-th particle**". Just think about it! As stated in quantum mechanics, the coordinates and momentum of a particle are not measurable at the same time. But for a particle **with an already defined momentum** (hence, "smeared" in space!), how did you run around the Universe and look for these most accurate (!) to differentiate the **coordinates of the a-th particle**?! A contradiction with the very essence of quantum mechanics (if it has one, according to the textbooks). Secondly, for the Hamiltonian of interacting particles, the function of the pulses was added with the coordinate function $U(\mathbf{r})$, which again are not simultaneously measurable. Therefore, such a combined expression has no independent meaning (a game in mathematical coincidences?). Thirdly, even for free particles, the statement from page 71 has not been proved: "so that the relation $E = p^2/(2m)$ was the case for all eigenvalues of energy and momentum, the same relation **should be** right for their operators". Why exactly **should be** and not **maybe**? When the interaction is turned on, the preservation of the form of this "half" of the Hamiltonian is especially not proved.

The representation of the solution of the Schrodinger equation for a free particle in the form of de Broglie waves [3, p. 51] is very conditional. What kind of plane wave can we talk about? If we take an analogy with the classics, then there is nothing in common in physics, only mathematical similarity. Indeed, in classical physics, both the amplitude and its square have a physical meaning (directly measurable); we can actually see the wave, determine its characteristics and where it is moving. In the quantum mechanical case, the value of ψ itself has no independent physical meaning and is not measurable, therefore, we will not see the wave process, and in the square of the modulus of the wave function $|\psi|^2$, these supposedly wave movements are "eaten".

Why the wave function should be continuous — in [3, p. 53] is not explained at all. Even in classical physics, probability (similar to the quantum mechanical square of the modulus of the wave function) can be discrete both in space and in time, but quantum mechanics is declared a more general theory and therefore must include all the particulars of classical physics. As for the continuity of derivatives of ψ – even more so: once at $U = \infty$ they are "generously allowed" to have a gap (just the task dictates its own and "pushes" the mathematics of quantum mechanics "to the wall"), then nothing (neither mathematics nor physics) is prohibited in all other cases. Also, nothing prohibits the wave function from tending to infinity at somewhere (since this is possible with some problem statements). Just to $|\psi|^2$ had a physical meaning of probability density, this function should be normalized (otherwise everything is based on faith: maybe lucky in the sense of similarity to reality, or maybe not so much).

Since the kinetic and potential parts of the energy are not precisely measurable at the same time, the "proof" for the average energy $E_{av} > U_{min}$ is not proper quantum mechanical, but relies only on a plausible analogy from classical physics.

The pseudo-explanation of the possibility for a particle to be in those regions of space in which $E < U$ causes laughter: "if by the measurement process a particle is localized at a certain point in space, then as a result of …it ceases to possess any particular kinetic energy at all". This "nonsense" would make at least some sense if we were always obliged to take measurements **at the moment of passing through the barrier**. And if we put a trapping counter (with a wide aperture) very far away in the area behind a narrow barrier, where the particle has already passed through this barrier (the distance from the counter to the barrier is very large). Will he stop fixing particles (or does the particle sense the presence of a counter and its influence at a huge distance from the barrier, before it has reached it yet, and for some reason feels only from inside the barrier, but not in free flight)?! So the measurement process has nothing to do with it at all, and quantum mechanical pseudo-explanations are out of place.

The description in the paragraph "Quantized energy levels"

[5, Chapter 16-6] is not entirely correct. “By eye” graphically, different types of behavior of a particle in a potential well are plotted. For example, having “constructed” a solution [5, Fig. 16.6] with an amplitude increasing at $x > x_2$, Feynman writes “for the solution we have made, it is much more likely to meet an electron in $x = +\infty$ than anywhere else”. In fact, the resulting “solution” is not normalized and therefore has no physical meaning at all; but it turned out because of our choice to look for a solution in the form of

$$\varphi = a(x)e^{-iEt/\hbar}.$$

There is a big computational and conceptual “choice problem” here, because partial differential equations can have many different types of solutions. Why, for example, cannot other periodic functions ($\exp[-i\alpha\sin(\beta t)]$, etc.) be taken as a time dependence? In principle, such periodic solutions could correspond to some constantly occurring cyclic reactions, states that are stationary in the dynamic sense. Or, in general, with a different time dependence, the variables could not be separatable at all.

Feynman’s proof of the necessity of using a dynamic momentum (or “p-momentum”) in quantum mechanics [5, Chapter 21-3] is not at all rigorous. First, before discussing the possibility of a sudden (instantaneous) change (increase) of a certain quantity (in this case, the vector-potential $\mathbf{A}$), it is necessary to find out what Nature says about such a change, and whether it affects other measurable quantities (are these laws interrelated changes with other investigated values). Secondly, let’s translate “Feynman’s proof” into normal language (let’s turn it backwise: “from head on feet”). Feynman writes: “And what happens when I instantly turn on the vector potential? **According to the quantum mechanical equation** (!!!), a sudden change in $\mathbf{A}$ does not cause a sudden change in ψ; ... this means that the gradient has not changed." Thus, Feynman **begins with faith in** quantum mechanical **equations** (and not in experiment), naturally, he will end with the same faith. It's just a game with math hooks. And when it turned out that the momentum had changed in the experiment (by $-q\mathbf{A}$), in order to leave the equation unchanged, he simply had to substitute the difference between the momentum and the randomly detected increment everywhere instead of the momentum. The great goal of physics — is to artificially save equations and assert faith in them *a priori*!

When discussing a case with

$$U = -\frac{\alpha}{r^s}$$

in [3, p. 54] for $s > 2$, the textbook authors do not even notice all the nonsense of the situation: "... there are negative eigenvalues of energy, arbitrarily large in absolute magnitude". Just think about it! A particle falling on such a center could emit an infinite amount of energy. If this is not a perpetual motion machine (an act of creation, etc.), then correct me. Any normal physicist will immediately conclude that for such a situation (and for many others), particles cannot be considered as point particles at all (and the released energy during annihilation should be limited). The method of "proof" is also surprising: for small r_0 artificially (!) without solving the equation, a function equal to zero outside r_0 is chosen; for any s, they assume that

$$\langle \frac{1}{r^s} \rangle \sim \frac{1}{r_0^s}.$$

Consideration of the case of $r \to \infty$ is also not strict: where have you seen solutions (in quantum mechanics, not in classical physics) that are different from zero only in the layer? Moreover, this layer is chosen to increase with the increase of r_0 (in proportion to r_0, that is, the shares of "occupied" and free space are kept constant); again, they arbitrarily choose

$$\langle \frac{1}{r^s} \rangle \sim \frac{1}{r_0^s}.$$

The possibility of choosing ψ by a real function — this is just a mathematical game, because in the presence of a magnetic field it is impossible, and all real particles have magnetic moments.

It is significant (in terms of the lack of originality and novelty) that in quantum mechanics, the relationship of particle velocity with momentum and acceleration with potential gradient is given by the same classical relations. But this is exactly what can actually be measured (unlike the quantum mechanical "original speculations" on "four-story mathematical constructions").

Here again we meet an unsubstantiated statement [3, p. 56]: "the fact that velocity does not exist simultaneously with coordinates means that if a particle is located at a certain point in space at some point in time, then it will not have a certain position already at the next infinitely close time moment". This statement could relate directly only to the measurement process, and not to a particle moving under the action of the forces of Nature without the intervention of an observer. And such an ideology of quantum mechanics contradicts the ideology of using differential equations, and even the ideology of mathematics in general! After all, in mathematics we are simply talking about mathematical multiplication by infinitesimal quantities dt, using the fact of continuity of changes in the corresponding physical quantities in Nature.

The proof of the mutual orthogonality of wave functions for states with different energies is based (through the Gauss theorem) on the belief that wave functions and their derivatives are always continuous in Nature. But there is no such proof in the Landau-Lifshitz textbook! And can a classically understood proof exist at all due to the strange "flickering" (non-classical) nature of the behavior of quantum objects?

The very ideology of quantum mechanics is contradicted by attempts to reduce a real three-dimensional problem to a problem with fewer dimensions or to formulate the problem in two- or one-dimensional form at once! Statements in [3, p. 60] that "if the potential energy of a particle depends on only one coordinate (x), then the wave function can be searched for as a product of a function from the variables y, z to a function only from the variable x" and "of these, the first is determined by the Schrodinger equation of the free movements, and the second — by the one-dimensional Schrodinger equation", are not proved by anything (taken for granted). In fact, **it is possible to divide a single three-dimensional equation** into three one-dimensional ones **in an infinite number of ways**. For example, you can write the following system:

$$\begin{cases} \frac{\partial^2\psi}{\partial x^2} + \frac{2m}{\hbar^2}\left(E_1 - U(x)\right)\psi &= 0 \\ \frac{\partial^2\psi}{\partial y^2} + \frac{2m}{\hbar^2}E_2\psi &= 0 \\ \frac{\partial^2\psi}{\partial z^2} + \frac{2m}{\hbar^2}E_3\psi &= 0 \\ E_1 + E_2 + E_3 &= E, \end{cases}$$

where E_1, E_2 and E_3 are some functions of x, y, z, two of which are completely arbitrary, and the third is from the last equation of the system. Therefore, the possibility of considering purely one-dimensional motions in quantum mechanics (for the real three-dimensional world) — is one of the additional **hypotheses**. The same questions remain for the case of the representability of potential energy by the sum of individual terms, each depending only on its coordinate. Apparently, this problem becomes particularly acute when trying to account for the magnetic field (and after all, all particles have a magnetic moment) or when trying to include nonlinear processes in quantum mechanics.

The need for continuity conditions ψ and ψ' in quantum mechanics is not justified, because even in classical physics, probability does not have to be continuous in space in the presence of barriers, and there is no question at all about the continuity of its derivative (remember children's games with a shooting metal ball flying into holes). And quantum mechanics claims to be a more general theory, that is, to include particular classical cases. Therefore, finding solutions for a potential box in this way (see, for example, [3, p. 63]) is not justified (not the only possible one). For $U_0 = \infty$, this is obvious, since in this case they immediately forget about the need for continuity of ψ'. From the same point of view, it might be possible to understand that the following property of solutions in problem 2 is strange [3, p. 65]. For $U_1 = U_2$, there is always at least one energy level in a potential well, and for $U_1 \neq U_2$, there is a well width less than which there are no discrete energy levels. This is strange: if in a potential well where a particle is already "sitting" at the energy level, I will "raise" (not decrease!) the larger wall U_2, then the level will disappear. Maybe this is not due to reality, but to the choice of conditions imposed on ψ and ψ' when searching for possible solutions.

When determining the energy levels of a linear oscillator using the matrix method, it can be seen [3, p. 67] that the coordinates and velocities have certain values at the same time:

$$(\dot{x})_{mn} = i\omega_{mn}x_{mn},$$

which contradicts the very ideology of quantum mechanics. In particular, "all matrix elements are equal to zero, except for those for which $\omega_{mn} = \pm\omega$". And how, strictly speaking, does zero differ from any other **defined** value? The determination of the lower limit for the possible values of the oscillator energy using the uncertainty relation in problem 2 [3, p. 71] is not fully justified. In fact, the condition

$$\overline{E} \geq \frac{\hbar\omega}{2}$$

is obtained, but based **on the inequality for the mean value**, it is impossible to deduce the inequality for "**generally all possible values of energy**". The conclusion could be made in the reverse order only: from the condition for all E, it would be possible to obtain a condition for the average, but in the textbook the situation is the opposite.

It remains another mystery: what is the meaning of the found continuous non-normalizable wave functions (occupying the entire space) for a **continuous** spectrum, for example, for a homogeneous field [3, p. 73]? Is this a science-like game programmed according to a predetermined scheme with mathematical hooks to inflate self-importance in front of students?

But for the transmission coefficient, obvious "punctures" are obtained. So, in problem 1 [3, p. 77], the reflection coefficient of a particle from a rectangular potential wall is found at $E > U_0$; it turns out that it is impossible to go to the classical limit. In justification, the following pseudo-scientific phrase is pronounced: "the classical limit corresponds to the case when the de Broglie wavelength of a particle is small compared to ... with distances at which the field $U(x)$... noticeably changes, This distance is zero, so that the limit transition cannot be made". What nonsense! Firstly, the resulting solution is not approximate, but **is a strict consequence of** quantum mechanical **principles**, and if quantum mechanics really were a more general theory than classical physics, then it would cover all cases of classical physics as special cases. That's not so. Secondly, what does the de-Broglie wave have to do with it, if even the physical meaning for it is not coordinated (not generally accepted)? Thirdly, what does the characteristic dimensions of the problem have to do with it, if any case can be modeled in classical physics? If quantum mechanics has these problems and limitations, then they are — its own internal difficulties!

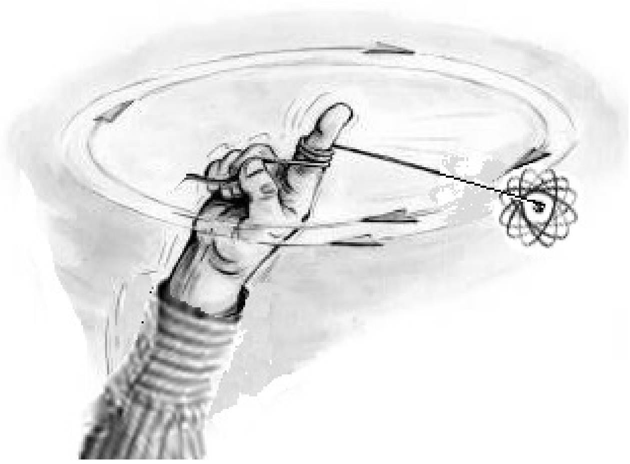
Fourth, what kind of "new fashion" has bred in physics: to make **dimensional constants** to be tending to zero, that is, quantities whose specific value **was fixed by Nature itself**?! The gentlemen-"limitmen" should study mathematics (limits) better and learn how to tends to zero (in fact, compare with a dimensionless unit) only dimensionless relations (so as not to assert that "the length of a boa, measured in [elephants], is negligible, but the length of the same boa, measured in [parrots], is infinitely large"). Apparently, obvious embarrassments in experiments can be avoided because in real situations they deal with limited barriers, and in this case, quantum mechanics gives reasonable results.

Unlike classical physics, the consideration of one-dimensional problems in quantum mechanics is completely conditional (rather, it is — one-parameter problems). After all, according to its own principles, if you fix exactly the coordinate, then the momentum will be infinite, and the particle flies out of the fixed plane; if in the perpendicular plane we assume $p_{\perp} = 0$, then the coordinate of the particle will be indeterminate in this plane (that is, it is a spatial motion, and the particle can be at an arbitrary distance from the initial fixation).

Thus, the justification of the Schrodinger equation is not perfect either from a physical, mathematical, or practical point of view. This equation did not solve the existing fundamental problems of explanation and, contrary to the supported opinion, did not lead to the construction of a strict algorithmic theory; its application in many cases raises great doubts (plausible corrections of the solution retroactively — knowing the answer from experiment — cannot be considered as a success!).

Chapter 5

Angular momentum



Although the concept of the "angular momentum" is well known to everyone from classical physics, but for quantum mechanics it is something completely different, coinciding with the former name only by the combination of words. Let's start at least with the fact that in classical mechanics, the moment of momentum, written as $m[\mathbf{r} \times \mathbf{v}]$, is in some way a combination of kinematic and dynamic quantities (as well as a combination of absolute and relative quantities). In quantum mechanics, the physical quantities of distance and velocity are considered not only immeasurable, but even not existing. How, then, can such a combined quantity be treated in the new physics? To begin with, it would be worthwhile to define a new physical meaning of such a quantity in quantum mechanics. For example, due to the simultaneous immeasurability of the quantities included in it (not the existence, "roughness" of the trajectory), the classical value could change arbitrarily ("fluctuation" from 0 to ∞), especially since it includes different components of quantities. Further, it is impossible to transfer automatically all the properties of the former classical angular momentum to a new quantum quantity, this issue requires additional theoretical and experimental justification. Independent experimental measurements of this new value for microobjects would be especially interesting (unless, of course, this value is not a fitting one).

When determining the eigenvalues of the angular momentum [3, p. 85], we again encounter a quantum mechanical comedy. Firstly, the conclusion about the integer value of the angular momentum projection is based only on faith (another postulate), since the function ψ itself has no physical meaning (only the square of the module $|\psi|^2$ possesses it) and **the pe-**

riodicity of φ of its phase multiplier (!) (ostensibly for the uniqueness of the function ψ) does not play any role for the actually observed quantities (at least, note that the period for even and odd functions may be different). Secondly, the integer components of the angular momentum **for any direction** — is an obvious contradiction. And the justification that different projections cannot have certain values at the same time is very weak: if there can be $L_x = L_y = L_z = 0$ at the same time (and it is difficult to imagine the science of mathematics itself without the existence of a "zero" element!), then how does the number zero (from the viewpoint of physical measurability and the uncertainty principle) differ from any other specific non-zero value? Thirdly, it is argued that the axes (for example, the Z axis) are not highlighted. In fact, each real particle has its intrinsic spin moment, which already allocates some direction for the particles interacting with it (therefore, the question on the possibility of degeneration of energy levels also requires additional research and justification).

In classical physics, changing the sign of all coordinates (inversion) does not change the energy of the system, which is obvious. But in quantum mechanics, they are beginning to invent ("suck out of the finger") a new conservation law for the value ψ (ψ has no independent meaning), therefore this "parity conservation law" also has no physical meaning, but is only a game of mathematical symbols in the artificially fixed framework of quantum mechanics. It is noteworthy that this "law" [3, p. 96] begins with the word "if", that is, we cannot know anything about the system in advance (maybe it has a certain parity, or maybe not). And will we need this "useful" information in hindsight?

In the absence of interaction between the parts of the system, the moments of its parts are preserved separately — this is in quantum mechanics, although it is not proved and is not obvious in advance, but it is stated by analogy with classical physics; and further in [3, p. 98] "it is deduced" the so-called "law of addition of moments" in the supposedly "next approximation". In what this approximation consists of quantitatively (by what parameter, and why it does not appear anywhere), and how to continuously move from one case to another is not mentioned at all (verbal **faith** and that's it!).

The "justification" of the impossibility of the decay of an atom with $L = 0$ into an ion and an electron, allegedly due to parity, causes only laughter — an atom cannot decay in the first and only turn **energetically**! If we will heat it up before the transition of the substance to the plasma state — and this will become possible in spite of everything else.

With respect to photons (for example, [5, Chapter 18-1]), Feynman, in fact, claims that they are only polarized in a circle: either right- or left-polarized to possess the one-unit angular moment, but they are not linearly polarized (that is, linearly polarized light — this is a statistical averaging of right- and left-polarized photons), but then how to understand the work of polarizers that distinguish (secrete) linearly polarized light (and how to relate to the numerous explanations and illustrations of the work of polarizers from physics courses?)? In addition, since photons have a "peculiar character" (see below) and they do not completely obey the "moment logic" (instead of the three projections there are only two), then where is the guarantee that the other plausible arguments about photon emission are true? For example, it is not clear why in the paragraph "Electric dipole radiation" [5, Chapter 18-1] for an **excited atom** with spin 1 and its projection $m_z = 0$ (that is, for the **third** possibility), we do not take into account the radiation of a photon in a plane perpendicular to the Z axis, because such radiation would also give a contribution for the direction of θ? Why should we assume that all possibilities **for one axis** (one-dimensional) exhaust **all spatial combinations**? Further — more: in a note to this paragraph, Feynman himself writes: "all these arguments are wrong, because our final states do not have a certain parity", that is, Nature does not want to play games with artificial quantum mechanical inventions in any way.

In Added note 2 [5, Chapter 18-8], to which the author refers for strictness, he writes: "What then will happen to our previous proof that an atom in a state with a certain energy must have a certain parity, and with our statement that parity in atomic processes is preserved? Shouldn't the final state in this problem ... have a certain parity? Yes, it should, if only we consider the complete final state, which includes the amplitudes of photon emission at all possible angles". That's great! It turns out that one atom emits not one photon, but an infinite number of them at all possible angles?! And what do experiments with individual (rare) excited atoms give (is it not the registration of individual rare specific photons)? Thus, Nature in the **physical** explanation here is not on the side of quantum mechanics (and does not limit the possibility of a classical **statistical** description in any way). In general, having not received a physically correct explanation, Feynman eventually turned the conversation to combinatorial mathematical games.

There are also questions with the description of the process of positronium annihilation [5, Chapter 18-3]. Let's start with the analogy of positronium and the hydrogen atom. For a hydrogen atom in the ground state, the probability for an electron to meet the nucleus (fall on the nucleus: fall into the center $\rho = 0$) is the greatest (see further on page 177). Why does not a neutron form from the nucleus? Just don't tell fairy tales about neutrinos-antineutrinos, because in other reactions they are capable of appearing for some reason "at the behest of the pike" (sorry, be captured) "out of thin air"!

Feynman raises questions about the structure of the photonic system in a note about the possibility of the orbital moment of momentum from the term $\mathbf{p} \times \mathbf{r}$: "like two lumps from the rim of a rotating wheel". Note that photons with such a device, in principle, can fly out not exactly along one straight line, but along parallel lines. Taking into account the fact that photons have a "peculiar charecter", arguments about the impossibility of two-photon annihilation from a spin-1 state may also turn out to be weak or even incorrect. Rather, the situation here was the opposite: since two-photon and three-photon annihilations are observed, then the task was artificially to fit this experimental fact "retroactively" into the framework of modern concepts of quantum mechanics.

Regarding three-photon annihilation, there arises another "naive" question: if positronium is only 1/4 of the time in a state with spin 0, and 3/4 of the time — in a state with spin 1, then why does three-photon annihilation occur 1000 times less often? Apparently, the "proofs" of theorists ("in a complicated way") that the ground state of positronium has negative parity are just as strict, and the only argument — that **such an assumption in the framework of quantum mechanics** agrees better with experiment than other hypotheses in the same limited framework of quantum mechanics.

This is followed by the "favorite hobby of theorists" — a thought experiment with non-existent polarizers of γ-quanta (another deception). Although in fact, **the result of simultaneous guaranteed measurement** of specific polarizations **of two spatially separated photons** in classical physics can be considered as **confirmation of the determinism in a separate act** (and probabilities are "appended" only as a result of statistics when observing an ensemble of realizations). The paradox of Einstein – Podolsky – Rosen, associated with the delusional theory of relativity, has nothing to do with nonrelativistic quantum mechanics.

The derivation of the formulas in the paragraph "Rotation matrix for any spin" [5, Chapter 18-4] assumes the possibility of dividing a single object into several smaller objects with spin 1/2 and complete independence from each other (uncorrelation) for any such different combinations. This, generally speaking, requires proof, but experimental verification for complex systems also becomes impossible (errors are too large) and remains at the mercy of faith. It is also strange to expect that the properties of all complex objects of the microcosm (atoms, molecules, nuclei, elementary particles) depend **only on such simple combinations** of the values of spins, orbital moments and their projections. Often, comparing an experimental dependence with a theoretical curve indicates only a strong belief in the theory (especially if the comparison area is limited, and there are many possibilities to choose from theoretical curves).

Thus, with the physical justification of the very concept of the angular momentum and its specific applications in quantum mechanics, too, not everything is smooth.

Chapter 6

Movement in a centrally symmetrical field

Let us now proceed to the analysis of the key models for describing the microcosm and solutions for such models, for which quantum mechanics was built (for example, models of a centrally symmetric field and solutions for the hydrogen atom, for other atoms and molecules).

In classical physics, the description of the motion of two interacting point particles can be reduced to the description of the motion of one particle with a reduced mass in a centrally symmetric field. But how could such a thing be possible conceptually in quantum mechanics [3, p. 101]? After all, each of the microparticles, as stated in the quantum mechanical theory, does not have exact values of coordinates and velocities at the same time, how then can the relative distance and relative velocity be determined, and even at the same time?

It is not proved the finiteness of the values of the function ψ at the finiteness of the magnitude of $U(r)$ in the textbook [3, p. 102] at all: can finite quantities really not enter into equations with infinite functions? Just another unfounded statement.

And what is this next fictional nonsense — **free** motion of a particle "with a certain absolute magnitude and projection of the angular moment" (not spin!) [3, p. 104], has anyone thought? If something "twists" the motion of a particle (deflects it from rectilinear motion), then it is no longer free! Or we could be talking about statistics for a large number of particles. For example, at $l = 0$, in fact, finite stationary flows are established to and from the center, which gives a standing wave (but this is not for a single particle!). The same situation will be with other l. (It is also interesting to note in parentheses that a certain multiplier $(-1)^l$ was introduced here for mythical reasons of "convenience", while in other previous cases, they "fight" in the wave function for the sign that has no physical meaning.) For some reason, they begin to remember explcitly about particle flows with a clever look only in obvious cases when the wave function turns out to be infinite at the origin (then it is called a source or a drain). That is, the interpretation of one and the same function can be near-scientifically adjusted to a specific task for plausibility (all theorists are strong "in hindsight"!). Thus, the paragraph "Free moution (spherical polar co-ordinates)" [3, p. 104] is nothing more than a game with mathematical equations and "hooks".

The same pseudo-mathematical game with equations is represented by the paragraph "Resolution of a plane wave" [3, p. 111]: it is quite clear that from the viewpoint of mathematics **any function** can be decomposed into some sets of functions that make up **the complete basis**, and the presence or absence of physical meaning in this arbitrarily taken the functions will have absolutely nothing to do with it. Therefore, choosing a fixed k is just a **arbitrary choice**. And the "justification" allegedly from a physical viewpoint, as if $m = 0$ (through the phase) is completely ridiculous, because the function ψ itself has no physical meaning, but only the square of the module $|\psi|^2$ has this meaning (so, at least some kind of visibility of a physical justification there could be only through the properties of the function $|\psi|^2$).

Paragraph " "Fall" of a particle to the centre" [3, p. 113] is again a (win-win for pseudo-scientists) game with mathematical equations without physical meaning (there is no such potential in Nature that is considered there!). In addition, this paragraph also represents "violence against mathematics". How can we neglect the value of E if this discarded value can be of the same order as the kept value of U? And what is this "perversion" with the artificial cutting out of U at $r = r_0$ and the further tending $r_0 \to 0$? After all, it would be correct (strictly) to make two transitions: first $r_0 \to r$, and then $r \to 0$! But then, despite the fact that the limit is $B/A \to 0$, in the solution

$$R = Ar^{S_1} + Br^{S_2}$$

both terms would have the same order of magnitude (the question remains even with the zeros of the function)! Naturally, in the case of $\gamma > 1/4$, such manipulations with mathematics lead to the fact that the equation at $r_0 \to 0$ does not tend to any limit at all. The physically correct decryption of the "stripped-down" the task is already quite obvious. As soon as you **fixed the final** E and began to increase the depth of the pit, it is natural that the number of this energy level may change (grow with the depth of the pit). But to prove the fall to the center (and determine the number of levels), it would be necessary to solve the complete problem strictly (for example, for a Coulomb field, the number of stationary levels is also infinite, but there is no fall to the center). In quantum mechanics, a small modification of conditions often leads to qualitatively new situations (for example, in a pit with equal edges, there is always a discrete level, and with unequal edges of the pit, even if these two cases differ by an extremely small amount, there may already be no stationary level). Therefore, "plausible" reasoning does not always work in quantum mechanics, and an exact problem should be solved (which, generally speaking, does not indicate in favor of quantum mechanics).

It is surprising that many physicists swear an oath to the "theory" of relativity even where nothing is demonstrated qualitatively, is not proved quantitatively, or is not required at all for the current description of the problem. So Feynman [5, Chapter 19-1] writes: "... let's do it ... approximately: we forget that the electron has spin, and that it should be described by the laws of relativistic mechanics". And then it follows a completely classical model description of a proton as a charge circulating in a circle. Note that in a similar model, both the spin and the magnetic moment of an electron can be understood (for believers in SRT, this is impossible due to speed limits). And the magnetic interaction is completely described by classical electrodynamics. The velocities of the electron in the atom are also clearly non-relativistic, so why was this curtsy needed? But what is most significant: even after all these plausible corrections (in the center of mass system), the energy levels still turn out to be shifted relative to the calculated values (an obvious "puncture" of quantum mechanics even for a system of two particles — the only solved exact model!)! The regular "darning of a hole" in quantum mechanics is carried out using the Casimir effect.

Even to the only "exact solution" obtained for the ground state of the hydrogen atom, questions remain. For example, using the resulting function

$$\varphi_1 = e^{-\rho},$$

if you calculate the average potential energy of an electron (in the atom), then it will be equal to minus infinity. Therefore, in order for the total energy of the atom to be finite, the kinetic energy of the electron must be infinite. It would be better not to say stupidity, as if it contradicts to SRT (which itself has nothing to do with the Nature of the microcosm), but simply to say that it contradicts observations, because in a hydrogen atom, no one has seen an electron moving at enormous speeds, higher or even comparable to the speed of light. The situation will be similar for other higher-energetic states at $l = 0$.

The opinion that the orbital motion in an atom can have only integer values of l is, in fact, postulated in quantum mechanics (or artificially taken to fit calculated quantities to experimental data in a fixed framework of quantum mechanics).

Even the only "exact" solution for the hydrogen atom is not so accurate: all levels are noticeably shifted from the calculated values. And for hydrogen-like energy levels (single-electron ions) [3, p. 236], inaccuracies are also presented (which cannot all be attributed to allegedly "relativistic" effects, both quantitatively and qualitatively — the velocities are far from c). Strongly excited states of an electron, considered "as motion in the Coulomb field of an "atomic remainder" with an effective charge equals to one", are also too inaccurate [3, p. 236]. From a predictive viewpoint, the introduction of the Rydberg correction "retroactively" has zero value for quantum mechanics, since it is not constant, but again is determined "post factum" — from experiments for each (!) level.

The possibility of obtaining solutions for a Coulomb field (for example, for a hydrogen atom) both in spherical coordinates (which is very familiar and habitual to everyone) and in more exotic parabolic coordinates [3, p. 125] actually indicates the ambiguity of solutions of quantum mechanics! Despite the coincidence of energy levels in these cases, the wave functions of these two types of solutions themselves differ significantly: in parabolic coordinates, the function ψ is not even symmetric with respect to the plane $z = 0$! But with the help of the wave function, it is possible to determine the experimentally measurable value — the distribution of particles (of electrons in an atom). How should Nature toss about to and fro choosing between these two essentially different cases of distributions?! Thus, this "puncture" of the quantum mechanics shows that not only the function ψ, but also $|\psi|^2$ has no physical meaning: all quantum mechanics is a game of "what looks like what" (with the sole purpose — to artificially make fittting of calculated energy levels to their pre-measured values).

When describing state changes in time [4, Chapter 8-4], Feynman uses the technique of transition to infinitesimal time intervals, that is, in fact, he carries out the process of linearization, and the question of the linearity or nonlinearity of the properties of our world remains "behind the scenes". To use the theory of relativity, Feynman frankly admits that there are difficulties: "it's not so easy to specify how **simultaneously** everything looks everywhere". Maybe a formal awareness of this fact saved science from complete stagnation caused by the demands "to cram" the theory of relativity to everywhere?

From a physical viewpoint, the demonstration paragraph "The ammonia molecule" [4, Chapter 8-6] looks rather comical. Let's start with the formulation of the problem: from a real molecule with an infinite number of states, they are trying to make a model system with two states (the model is very similar to reality!). Everything was rigidly fixed in the molecule: the momentum, the angular momentum, the axis of rotation. I wonder what to do with the notorious quantum mechanical uncertainty (or in the interests of theory, it should be forgotten for a while)? The differences between the two states were left only in the arrangement of the nitrogen atom relative to the plane of the hydrogen atoms. Since the system has only two states, then, contrary to the "words casually" about oscillations, oscillations in such a model system should be absent at all! But the book claims that in the next moment, the system "may not be in the same state anymore". What is an instant focus-hocus without intermediate states?! Next, Feynman tries to apply here an underdetermined system of two equations with two unknown functions and four more unknown coefficients. Immediately, he had to introduce a bunch of new postulates along with the old postulates (in order to get at least something definite). Firstly, the previously introduced postulate is used that if nothing in the system could change, then in one case it would be $H_{11} = E_1$, and in the other case it would be $H_{22} = E_2$. Secondly, he equates $E_1 = E_2$. Why on earth? Since we can somehow distinguish one state from another in our device, then they already have to differ in some way: for example, if the device detects (or polarizes) using a field, then one direction of the moment will be along the field, and the other — against the field, and they will differ in energy (of interactions). Thirdly, Feynman talks about "pushing" of nitrogen "through (past, beside) three hydrogens". Strange: there are no oscillations in this direction at all, that is, nitrogen should "stand still", but there is an instantaneous "pushing through" (that is, you can find yourself and here and there without speed?!). Fourth, again, the presence of a device (of an analyzer) makes **optional** the equality of the coefficients H_{12} and H_{21}. Fifth, with the "inclusion" of these new coefficients H_{12} and H_{21} (that is, **of the new physical process**), it is not at all necessary to maintain the coefficients H_{11} and H_{22} to be equal to **the previous** values (for the previous absence of any process). Further, Feynman honestly admits that "in quantum mechanics, the difficulty is not only to get a solution, but also to understand their meaning"! And then the game "what's like what" begins (to seek out a physical interpretation of a mathematical solution). As a result, he "found" that the probability of detecting a molecule in state 2 changes as the square of a sine. The question is: can we provide all the conditions of the "experiment" and monitor the isolated molecule to confirm this dependence? Of course not! Will we attract statistics ("the third kind of lie")? It will turn out a speculation on faith again. In addition, the value of A after all these frauds turned out to be uncertain. Will we again choose a value that is advantageous for quantum mechanics (for fitting)? Feynman further cites a well-known analogy with connected pendulums. And there would be need to tell him that with using oscillations, it would be worthwhile to set and solve the problem more correctly, but instead he says the coding phrase as a conclusion: "splitting the energy levels of an ammonia molecule — is strictly a quantum mechanical effect".

The contradictions of the obtained solution to the initial assumptions are clearly manifested in the following paragraph "The states of an ammonia molecule" [4, Chapter 9-1]. If it was previously assumed (and calculated) that there are only two states and the transition in time between them is carried out with a probability equal to the square of the sine:

$$\sin^2\left(\frac{At}{\hbar}\right),$$

then now it turns out that there are an infinite number of states: they change depending on the choice of arbitrary coefficients a and b and with any choice of the ratio a/b do not coincide with another solution for another choice of the ratio a/b for any t! Thus, in fact, it is not just about interpreting an unambiguous solution, but about choosing a "suitable solution" from an infinite number of them (that is, about mathematical adjustment to the desired!). In addition, it is claimed that the molecule can make transitions between levels with $\Delta E = 10^{-4}$eV ($\lambda = 1.25$cm)! This is a very small energy difference. It is strange that for any other changes, for example, in the energy level of the lightest electron, large energies are required by many orders of magnitude, and here in order to "push through" a whole nitrogen atom with all its electrons (more precisely, the transfer of the entire system — to preserve the position of the center of mass!) such a minimum is enough. Something here does not fit the model with reality!

Feynman in [4, Chapter 9-2] speaks about the "jumping" of nitrogen through the plane of hydrogen atoms (in an electric field) as a matter of course or a proven fact. But it's not like that! Even in conceptual (or qualitative) terms, everything is described here by a solid pile of untested hypotheses, and Feynman admits honestly about the quantitative verification of his hypothetical description: "In accordance with rigorous physical theory, it must be possible to calculate these constants (E_0, A) if the positions and movements of all nuclei and electrons are known. But **no one has ever** done this ... the task is too complicated ... no one knows more about this molecule than you and I do". Thus, **instead of comparing** the calculated and experimentally measured values, we are asked **to just be-**

lieve in a newfangled theory (that is, immediately take the values E_0, A required to fit experimental data). But there remain many questions about the very essence of this descriptive model. Firstly, how can the field be considered constant, because during the "transfer through" the mutual distance between nitrogen and hydrogen atoms should change, that is, the total field should change! This means that the model already does not correspond to reality. Secondly, in addition to a small difference in the energies of the states, the nitrogen atom with such a "jumping" would have to overcome much more significant electrical forces when approaching the plane of hydrogen atoms. Thirdly, why can't the ammonia molecule turn? After all, this effect must be taken into account (even incomplete turns — statistically!), since the potential energy changes. Fourth, where is the proof that the geometry of atoms and molecules does not change at all with such a "jump" (rather, the opposite is obvious for this model!). Further, during the (resonant) transition from state I to state II, the energy of the molecule decreases, and Feynman again without evidence (they simply do not exist in the model itself and must be searched artificially) utters a plausible phrase: "this loss of energy will not be able to go into anything else, but only into the mechanism that generates the field". But then the balance of the probability of transitions should take into account the balance of radiation in frequencies, taking into account the interaction of molecules with each other (including taking into account temperature characteristics) and with the walls of the chamber, as well as the characteristics of the chamber (its interaction with the surrounding space, because the radiated energy can partially leave the area with ammonia molecules).

Some dissatisfaction also arises due to the incoherency of quantum mechanics: somewhere it is proudly stated about its great discovery that light should be emitted and absorbed only in portions (each entirely), and in this (other) case, continuous field characteristics are calmly substituted (for example, frequency integration is carried out when calculating light absorption). But the greatest dissatisfaction with quantum mechanics arises from the scanty amount of comparison of theoretically calculated (completely "from first principles") physical quantities with their experimentally measured values.

Unfortunately, all the two-level systems considered by Feynman [4] have only the character of a qualitative demonstration model, since the following possibilities for the system are not taken into account: the presence of translational motions (non-zero temperatures), rotations, oscillations, torsions, excited states near the basic states, collective processes, etc. A scientific approach (for predictive power) would have to include an assessment of the role of unaccounted effects. Otherwise, it turns out that we are artificially looking for only those elements of a pre-selected theory (quantum mechanics) that somehow resemble real phenomena, without thinking at all about whether Nature really works like that (that is, without penetrating into the essence of the processes taking place). The constant "coding reminder" that in the classics an electron is not able to do something (for example, "penetrate through the barrier") is a manipulation of reality to the chosen rough model: in a multiparticle system (generally speaking, open) there are always many possibilities of resonant transitions. And quite strange in this regard is the statement that "in this space, an electron moves almost like a free particle in empty space, but at the same time possessing **negative** energy" (!) (see [4, Chapter 10-1]). Do not forget that all particles in a multiparticle system have energy and momentum (in our case — not only electrons, but also protons) and a resonant redistribution of energy may well occur between them. And in general, it is strange to invent some kind of "exchange process", if, quite possibly, due to the choice of configuration, the stability of the system (attraction) can be maintained. In the presented "theory" of two-level systems, it is obtained that the bond of **asymmetric** diatomic molecules should be very **weak**, but in fact, contrary to quantum mechanics, there are numerous examples of the opposite property (HF, HCl, etc.)!

The meaning of writing many paragraphs in textbooks on quantum mechanics is unclear. Take, for example, the paragraph "The hydrogen molecule" in [4, Chapter 10-3]. It does not look like a **demonstration of the fundamental principles** of quantum mechanics: instead of interference of electrons (fermi particles!) with a negative sign, it is required to obtain the addition (!) of amplitudes. And then they verbally (without any experimental or other justification) declare the antiparallel orientation of the electron spins (it remains unclear why in this case the spins were able to turn around like that, and in other cases they cannot turn around like that?). There are no **quantitative calculations** and comparisons with experiments either. Does a set of plausible verbal examples serve only to strengthen faith (since mentioning neither ionic nor covalent bond is binding to anything)?

Similarly, the phrase [4, Chapter 10-3] "spins tend to line up in an antiparallel position and . . . have the potential to release energy **not because there is a large magnetic force**, but because of the prohibition principle", apparently, also serves for a coding reinforcement of faith in quantum mechanics and promotes thoughtless rejection of elementary proven facts of classical physics.

The description of the "mysteries" of the benzene molecule in [4, Chapter 10-4], it would seem, should reflect another triumph of quantum mechanics. But is it so? The description of a stronger bond in the ring is quite understandable from the viewpoint of common sense: a unified molecule has an uniform bond in the entire ring, and not a static alternation of single and weaker double bonds. But this (classical) idea is fully consistent with the absence of the second form of dibromobenzene, unlike quantum mechanical "puncture": after all, quantum mechanics does not prohibit **different** systems to have **different** lowest energies and, as Feynman writes, "one of the two ... possibilities **more likely** than the other". But this "high probability" does not at all negate the very possibility of having a second form of the molecule, which, however, experiement does not give at all!

In general, such an artificial search for applications for the "ubiquitous" two-state model (for example, in the paragraph "Dyes" [4, Chapter 10-5]) looks ridiculous: otherwise, you can "draw" a non-existent molecule in exactly the same way, "finish" its second state by simply looking at it from the back of the sheet in the passing rays (to the lumen), and try to prove that such a system will have a non-zero binding energy and the lowest energy state. Or is it necessary to "get smart with applications of quantum mechanics" after learning the results of experiments in advance?

The statements of quantum problems are shocking in their naivety: "if l and m... are given". It is understandable when scientists are looking for a way from simpler knowledge to more complex. For example, there are quantities that are more easily (and obviously) observable and measurable, and you need to find another more complex quantity that is very necessary and useful for subsequent calculations and predictions, but much more difficult to measure (having a wider range of functional dependencies). Naturally, if someone finds an opportunity to express the functional dependence of this necessary quantity through simpler quantities, then honor and praise to him. But: "given l and m" ... Who and what directive from above gave them? Translating into understandable language: if some kind of functional dependence resembles what quantum mechanics received at specific values of l and m, then do not suffer from confirmation and essence, but consider l and m exactly like that (and continue to believe in quantum mechanics!). In fact, the choise that in the general solution for hydrogen, the separation constant of angular and radial variables K_l [5, Chapter 19-4] is considered to depend only on the orbital quantum number l — is a hypothesis. If there is a preferred direction in the atom (for example, by spin), then the rigorous solution may also depend on the projection of the angular momentum m on this preferred axis.

It is also interesting when in one place Feynman says that for two particles it is necessary to consider only a joint distribution function (somewhere he even talks about the distribution function of the entire Universe for calculating individual particles). But when describing the Periodic Table [5, Chapter 19-6], he easily "rolls down" from his "strict" quantum mechanical principles to the principle “at fishless place — cancer is also fish”, and, in fact, he unprovenly describes the movement of a single electron in the field of the total carcass (of nucleus and lower electrons).

Unconditionally, the fact that the Schrodinger equation does not take into account such a characteristic of electrons as spin at all indicates its approximate nature (not rigorous, limited). And it’s not worth “bending over” and pronouncing the word “relativistic” [3, p. 231] at every mention of spin interactions — magnetic interactions are “familiar” to people long before the “relativistic boom”.

Further, quantum mechanics had so many declarations that the states of the system should be considered as a whole — as a single wave function (however, due to the smearing of the electron throughout the universe, a natural question arises: shouldn’t there be only one ψ at all — for the entire Universe at once?). And suddenly it is said, contrary to its own ideology, that it is possible to “introduce the concept of the states of each electron separately” [3, p. 232]. And it is simply impossible to look at the subsequent “tricks” without a smile: “since the self-consistent field is centrally symmetric” [3, p. 232] . . . Even attempts to prove it are not made. What could this mean in theory? In classical physics, for example, if one of the electrons moved significantly slower than the others, then averaging could be carried out for fast movements and only for this “turtle electron” to get some averaged field. However, the velocities of all electrons in an atom have the same order of magnitude, which means that it won’t succeed to reduce the “carcass” to a static state. But even if it were possible, there would be no question of any central symmetry. Let's remember high school: p-orbitals have the shape of an eight, etc. And these are experimental (!) results, so in practice there can be no question of any central symmetry. What, then, is this method that does not rely on reality? We translate its essence "into understandable language": knowing in advance the results of the experiments, "post factum", we will make a fitting to these results with the help of such a postulated centrally symmetric field (this can be done with some accuracy). Thus, this is not a scientific, not a research, not a calculation method, but only a fitting and **demonstration** method (strengthening faith). The absence of quantum mechanical explanations for the relative positions of different levels only confirms such an assessment. Recall that in this case, the **empirical** Hund rule is used. From the same viewpoint, it is not proven at all that the electron moments of the inner filled shells are mutually compensated, as indicated in [3, p. 235].

The self-consistent field method (see, for example, [3, p. 237]) relies on a bunch of arbitrary hypotheses. First, **separate** wave functions are introduced for each of the electrons. At the same time, even their states are described as if each of the electrons is in a separate hydrogen atom. But the wave function must be unified and it is unknown (not proven) whether it can even be approximated as a combination of individual wave functions. Secondly, achieving the orthogonality of ψ_1 and $\psi_2' = \psi_2 + const \cdot \psi_1$, for some reason they forget that the physical meaning of the function ψ_2' is lost, since the normalization condition is violated for it. Thirdly, the proximity of the energy level to the real level (even with a complete coincidence — this is one point) does not guarantee the proximity of the resulting wave function to the true wave function (a continuum in the whole space). But the distribution of electron density in an atom (depending precisely on the entire wave function) is an experimentally measured quantity. And no one even tries to prove the statement made in [3, p. 238], as if "this is the best function of all functions of this kind" (the question is — by which parameter is the best?).

The "evaluation estimates" are also ridiculous (see [3, p. 246] "Wave functions of the outer electrons near the nucleus"), ending with the phrase: "the formulas determine only the systematic course of changes in quantities with an increase in z, without taking into account non-systematic changes during the transition from one element to the next". It sounds allegedly scientific-like and outwardly solid, but, most importantly, it is not verifiable, that is, not provable. Since **any other** arbitrary dependence can be separated from any dependence, and the difference can be attributed to anything else ("non-systematic dependence"; theories, approximations, calculations). I would like to ask an "adequate" question: "in what kind of universe did you find it?" In our World, there are only (!) a little more than a hundred elements that all obey to the **periodic law**. In general, it is dishonest to pretend that something has been explained and dependencies have been obtained if the predictions are made "plus or minus a tram stop": with such accuracy, you can arbitrarily "draw" an infinite number of dependencies and even an infinite number that coincide in form (!) with the required one. Agree that statements like: "the formula is true for those cases for which it turns out to be approximately true, and for other cases it does not fit" look ridiculous.

The same situation exists for the so-called "bond types" [3, p. 251] (or approximations). For a little more than a hundred elements, there are (or rather, artificially distinguished): the "Russell-Soundor case" ("LS-type of bond"), "jj-type of bond", about the latter they immediately say that it "does not occur in its pure form", and also "various intermediate types of bond between LS- and jj-types of bond". And all this sciencelikeness, of course, is "post factum" — after experimental results, that is, obtained by a theory with an obvious zero predictive power.

In quantum mechanics [3, p. 252], "explanation" of the Mendeleev's periodic system of elements again has a superficially qualitative character of stating facts "post factum": there are too many individual "features" in Nature (filling states, for example, or changes in ionization potentials, etc.). Judging by the ionization potentials, the sizes of alkali metal atoms should practically coincide. Judging by the ionization potentials for the corresponding previous elements — inert gases, their outer electron shell should monotonically grow with the element number. But the filled shell of the alkali metal corresponds to the shell of the previous inert gas. What could be the reason that an increase in the core charge by one in each of these cases immediately leads to a "collapse" of each substantially new electron shell of the next inert gas under to the "almost the same" size (constant) of an alkali metal atom?

It looks cynical in quantum mechanics, according to its own ideology, the use of "effective potential energy" taking into account centrifugal energy [3, p. 257]. After all, the latter value depends on the velocity of the particle (electron), and the potential $\psi(\mathbf{r})$ depends on the coordinates of the particle (electron), but $\mathbf{r}$ and $\mathbf{v}$ cannot simultaneously, according to quantum mechanics, have certain values, so $\psi(\mathbf{r})$ and mv^2/r do not exist at the same time. And the very concept of the angular momentum is also contradictory, and the fact that l is written instead of $m[\mathbf{v} \times \mathbf{r}]$ does not change the matter! Who are we deceiving? Not yourself?

It is funny when quantum scientists "salute" relativists [3, p. 260], calling pairs of levels of X-ray terms correct (or relativistic). What does relativism have to do with non-relativistic quantum mechanics? Is it really impossible to do without encodings?

The statement in the paragraph "Stark effect" [3, p. 265] that "in an atom placed in a homogeneous external electric field, we are dealing with a system of electrons located in an axially symmetric field (the field of the nucleus together with the external field)" — is incorrect in the general case, since the alternating fields of all remaining electrons (except an allocated electron) act on any allocated electron. The exception is the hydrogen atom (which was not considered in this quoted paragraph).

Further in the book it is stated that "the diagonal matrix elements of the dipole moment **identically vanish**" [3, p. 265], and they refer to the rigorous (!) result of the previous paragraph [3, p. 261] for "**any system** of particles in stationary states". Allegedly just by this reason, the effect should be proportional **to the square of the field**. And suddenly all these **general** results stop working for the simplest hydrogen atom (of course, the scientific-like phrase "to strengthen faith" is pronounced — "about random degeneracy"). The following phrase is noteworthy [3, p. 269]: "it is **inconvenient** to use the usual perturbation theory to calculate the quadratic effect". Is it inconvenient, or is it not possible to hide the deception? Anyway, at the end of paragraph [3, p. 274] in a note, they have to admit: "the total series of perturbation theory for the Stark splitting of levels cannot be convergent in the rigorous sense of the word, but is only **asymptotically** (?!): starting from a certain place of the series . . . , its further terms increase, and they are not decreasing". How so?! In the non-existent (divergent) "solution" does not take into account the most significant terms, but only the smallest ones that can be summed up (as in the joke about a drunkard looking for the keys to an apartment under a street lamp, because it is lighter there).

The very formulation of the problem for electronic terms causes bewilderment: it is assumed that "electronic terms are not numbers, but functions of the parameters — of distances between nuclei in a molecule" [3, p. 277]. Does humanity already have a way to artificially change this very distance (like as balls moved with fingers) without introducing "external" fields in "surroundings"? After all, we are talking about an isolated molecule. Whether the molecule itself determines at what single, stable distance its nuclei will be located, or not? If we had a medium together with an isolated molecule, then the change in the distance between the nuclei would also occur (together with changes of **all** types of movements) **in concordance** with changes in all external and internal conditions and **quite unambiguously**. Further in this paragraph, they are not talking about "how it works in Nature", but about how to "classify" everything according to the already familiar (advertised) "shelves". Reasoning about the classification of terms in the process of reflections is simply "ridiculously pompous", because with a double reflection of any most complex object relative to any arbitrary plane, the object always turns into itself (the momentum after a single reflection relative to the plane passing through the axis always changes to the opposite). Then, according to scientific-like reasoning [3, p. 278], it would always follow that the wave function would transform into itself at each reflection with a coefficient of ± 1. Note also that the presence of empirical rules (in this case, about the normal state), which are not explained by theory, indicates the weakness of the theory.

The problem of the intersection of terms is somewhat far-fetched. So, for example, the nucleus of any atom could then be represented as consisting of hydrogen and helium atoms and consider how the orbits of electrons of such "'parts" of the atom change, when they approach each other and form the nucleus of a composite atom (and also enter a certain distance parameter — the size of the composite nucleus). In fact, in a molecule, as in an atom, under each specific external conditions, the electronic configuration turns out to be quite definite (unique) and should be determined **for the entire molecule as a whole**! Don't you think pompous statements about the rigor of the results look ridiculous if there are exceptions to them? For example, [3, p. 281]: "if ... we would get two intersecting terms of the same symmetry, then when calculating the next approximation they will be pushed apart ... We emphasize that this result applies not only to a diatomic molecule, but is in fact a general quantum mechanical theorem. . . ". But in the note: "there is a kind of exception — ion H_2^+". So much for the "'general theorem"! Apparently, we are supposed idiots, considering the connection of molecular terms with atomic ones as "children's puzzles" (as combinatorics). After all, the state of each particular molecule is stable (equilibrium), and to change $r \to \infty$ or $r \to 0$, you need to apply force! Therefore, in the general case, the moment of momentum changes, and even the angular moment of each electron changes in different ways!

The desire of "adult pundits" to make pompous scientific-like-sounding statements is simply ridiculous. So, in [3, p. 287] it says: "the ability of atoms to connect to each other is related to their spin . . . the connection occurs so that the spins of the atoms are mutually compensated". And this statement was made **contrary to** the previously identified "exceptions": O_2, NO, NO_2, $Cl\,O_2$ (as well as the subsequent exceptions, among which: Xe, Rn, elements of intermediate groups). It is further stated that the doubled spin coincides with the chemical valence. But many elements exhibit several valences! After that, it becomes possible to make any number of scientific-like speculations "with the hindsight" of the type "the excited state is comparatively close", or "about inclinations" and "affections" (it remains to introduce more gods with their own "characters" for each element!).

It is also obvious that quantum mechanics cannot do without classical mechanics in any way. And not only as a semi-mythical classical device and an artificial limiting case (another big question — which of the theories is more general!), but as purely classical exact mathematical expressions for physical quantities, rigorous (accurate, not probabilistic) methods, and for approximate calculations, obvious in the classics by physical meaning and not justified in any way in the "fundamentally new" quantum mechanics. For example, this is when splitting **a single spectrum problem** into artificial **stationary** subtasks in [3, p. 293] "Vibrational and rotational structures of singlet terms in the diatomic molecule".

For purely **non-relativistic** quantum mechanics, both in physical essence, and in terms of values of quantities, and in terms of equations, there are no limitations on the rate of transmission of interactions (the Schrödinger equation is a parabolic equation). For example, sub-barrier tunneling (the passage of a quantum mechanical barrier) occurs instantly, with infinite speed. Therefore, it remains a complete "mystery" to identify certain "relativistic" interactions and the use of phrases like: "when taking into account relativistic interactions, degenerate energy levels are split" [3, p. 300]. Apparently, this is a "ritual magical sacrifice for fanatical believers" in such nonsense.

Long-term scientific-like-sounding "justifications" of the types of spectra are also ridiculous if, as a result, some unknown constants (functions) appear in the formula, different "for different terms" (overtheoretized phenomenology). So it would be possible to write an arbitrary function with a number of coefficients, and then get the required values by fitting the coefficients. Where is the physical theory here?

Many "plausible" the reasoning immediately turns out to be "questionable" if you look at the problem in more detail. So, in the paragraph "Pre-dissociation" [3, p. 323] in Fig. 30 (see Figure 1.3 below), potential energy curves are depicted, but they do not depict a zero energy level, and at the same time they talk about the decay of a molecule. Can the potential energy at infinity tend to a finite positive or negative value (and most of the solved problems "bound" to zero value)?! Won't curves 1 and 2 have **a common zero limit** at the same infinity (I apologize for the obvious naive question)? And how, from the viewpoint of quantum mechanics itself, to evaluate equality (90,2) in collisions of two atoms?! Contrary to the ideology of quantum mechanics, we have clear values of the coordinates and impulses of the nuclei at the moment of collision (and where did the uncertainty relation "hide"?). A legitimate question arises, how can equality be differentiated [3, p. 326]:

$$\frac{p_1^2}{2\mu} + u_1 = \frac{p_2^2}{2\mu} + u_2,$$

if the coordinates and momentums are not determined at the same time (again, the uncertainty relation has "disappeared": "here we read... here we don't read... here is a fat spot ..."). Even more "remarkable" is Fig. 31 [3, p. 327], which turns out to be the same as Figure 30, but in the next approximation (see Figure 1.2 below). Here a piece of curve 2 became a continuation of curve 1', and a piece of curve 1 — a continuation of curve 2'. It would be ridiculous in the classics if someone "mixed up" the results of the zero and first approximations: a comet was flying (and not necessarily a big one), its orbit crossed the Earth's orbit (even without a collision), and then the Earth flew along the comet's orbit (flew away), and the comet "settled" instead of the Earth as the third planet. And in quantum mechanics, it is also impossible to confuse the electronic terms of atoms if they differ, for example, in the orbital moment. Such mathematical "tricks with hooks" can only cause laughter.

Note that even the operator for the energy of the rotational

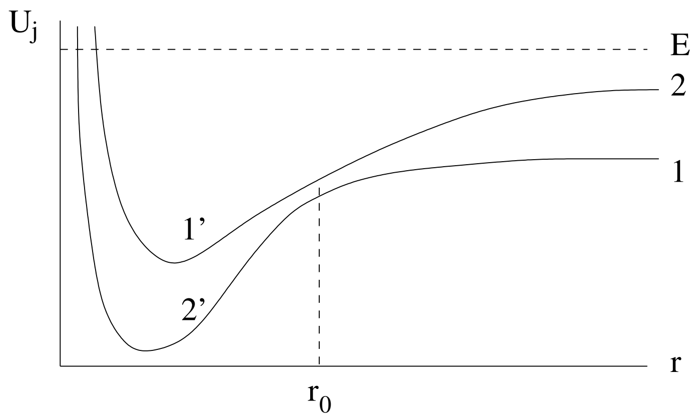


Figure 2: Figure 31 from the textbook [3]

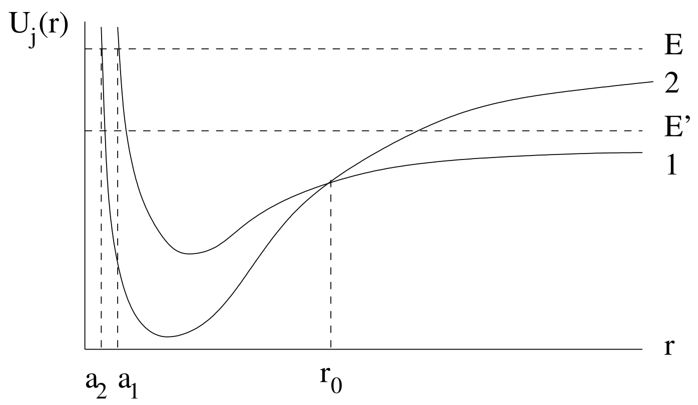


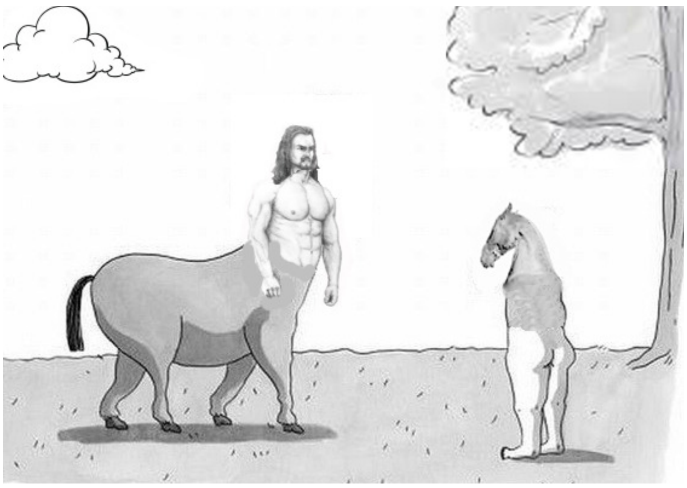
Figure 3: Figure 30 from the textbook [3]

motion of the molecule [3, p. 392] is not found at all **independently**, but **through the difference** with an oscillatory component. How then can the wave function be considered the product of an electronic wave function, a wave function of oscillatory motion and a rotational wave function [3, p. 394], that is, as a product of **independent** parts? It turns out inconsistently!

Thus, even the key models and demonstration solutions of quantum mechanics turned out to be flawed, too.

Chapter 7

Apparatus of quantum mechanics



Let us now turn to the "exclusive mathematics" of quantum mechanics and the quantum mechanical interpretations invented for this mathematics.

All "new-introducted" theories (for example, quantum mechanics) use classical verbal "rhetoric" to weaken the attentiveness of physicists, because in the classics everything is clear and verifiable. Even the concept of an isolated system in quantum mechanics strictly mathematically (not verbal) can be introduced only using really non-existent infinite potentials or in the presence of infinite masses for the absence of momentum transfer. That is, sequential quantum mechanics — is the mechanics of an unified wave function for the entire Universe (completely speculative and useless for practical science), where everyone interacts with everyone. After all, wave functions and actions with them are extended to the entire space $-\infty < \mathbf{r} < +\infty$. From this viewpoint, in quantum mechanics, for example, the equivalence of the Hamiltonian of a system for its "parallel transfer to an arbitrary distance" is far from obvious (what to do with an observer or measuring device?). Maybe we should talk about moving the reference frame (origin) without touching the objects of the Universe at all?

Amazing science — quantum mechanics: it turns out that so many mathematical games can be invented with quantities that do not have their own physical meaning. So, "applying" a phase multiplier that has no independent physical meaning, with the help of an obviously classical operation of inversion, the concept of parity is introduced. However, the expected thing immediately turns out: there are states (not having a certain parity) for which this concept is not defined, for example, degenerate states, or states that do not correspond to the certain energy. It is obvious that such "concepts" cannot be included as the laws of Nature, but relate only to the model quantum mechanical method of description and can only help demonstration calculations in it. And even then, provided that we know in advance (apparently from experiment) the properties of the system and its symmetry. So, factually by chance, it was discovered that parity is not always preserved.

Regarding the derivation of conservation laws from the properties of symmetry, it should also be recalled that the additivity of infinitesimal changes does not have to lead to a linear functional dependence of finite quantities. That is, from $\delta\varphi + \delta\varphi = 2\delta\varphi$, the function φ does not have to follow, but it can be $\sin\varphi$, and other dependencies.

Feynman's remark [5, Chapter 17-2] beginning with the phrase "with some symmetries, what is true at first is always true" and the subsequent paragraph — are legitimate for model systems. But if we do not know the internal structure of the system, then we do not know its Hamiltonian and, therefore, we cannot be a priori sure of the symmetries of the system as a whole.

Regarding polarized light [5, Chapter 17-4], it should be noted that no complete proof of the equivalence for the angular momentum in classical and quantum mechanics turned out: there is no proof that during the acceleration of the electron, the phase shift φ_0 remains constant (in general, this value is also determined by the properties of the substance absorbing light). That is, only r and v change proportionally, which means that the value of ω can also change, and not coincide with the frequency of the "stimulating" light; then

$$\frac{dJ_z}{dW} = \frac{1}{\omega}$$

is not necessarily equivalent to

$$J_z = \frac{W}{\omega}.$$

Contrary to all previous predictions of quantum mechanics, that "a particle with spin 1 can have three values J_z : $+1, 0, -1$", Feynman admits that nature does not always obey theory (quantum mechanics): "light has its own temper: it has only two states" [5, Chapter 17-4]. But then he makes even more recognition "aside": "particles with zero rest mass can do with only one of two spin states" (for example, neutrinos). In general, Nature always has a richer choice than the "sour menu of theorists".

And the subsequent application contains an even stricter verdict on the rigor of quantum mechanics (p. 122). "At the very least, we tried to prove that the component of the moment of the momentum along the direction of motion of a particle with zero mass should be, for example, a multiple of $\hbar/2$, and not $\hbar/3$. But even by activating all sorts of properties of Lorentz transformations (and much more), we could not cope with this". Something in Nature there are big disagreements with the theory of relativity.

Regarding the paragraph "The disintegration of the Λ°" [5, Chapter 17-5], a wonderful "picture" also turns out: "we can't say anything about the amplitudes of a and b... no one knows how to count them yet. You have to get them from experiment". In general, it turns out that the theory exists only in order to confirm it by experiment and to assert faith in it by fitting coefficients.

The idea of decomposition of an arbitrary function by basic (stationary, normalized) states is understandable for the study of stationary processes or physical properties of systems (ensembles) in such states. And if the system makes a transition (which lasts a finite time!) from one state to another, is there really no probability of interaction in an intermediate state (and measurements of energy and other quantities at the time of transition)? In general, how in Nature (and not in a mathematical model) can you know in advance the full set of basic states (it turns out that you already had to investigate everything, and often this is — infinitely much!)? We are not talking about the mathematical aspects of the decomposition of a function by some complete set of other basic functions, but about the physics of the phenomenon and the process of its measurement. After all, for example, the same intermediate energy can often be represented in several different ways as the average of a set of other fixed energies. Does the equality of such averages for different sub-ensembles necessarily imply the sameness of states (for example, for simultaneous measurements of a quantity other than energy)? Some strange **additional law** turns out (does it have experimental confirmations or at least theoretical model proofs?).

The following landmark recognition of Feynman [5, Chapter 20-3] is: "no differential equations were solved (for calculating the helium atom), but special functions **with a set of**

adjustable parameters were compiled, which were selected so as to give the average energy the lowest value". In general, **faith** in quantum mechanics **is already strong** and you can no longer test anything, but simply satisfy the experiment with a simple fittting! Is this now a new goal of science — to strengthen faith with hindsight?

The designations and approach chosen by Feynman are not always convenient: although simple, but voluminous calculations are often dragged out in order to eventually come to an obvious result. Many operators in quantum mechanics are connected by relations that completely copy well-known classical results (for some reason this does not inspire Feynman). For example, the relation of the total energy operator to the momentum operator

$$\hat{\mathcal{H}} = \frac{1}{2m}\hat{\mathcal{P}} \cdot \hat{\mathcal{P}} + V(\mathbf{r}),$$

of the total momentum of the system with the momentums of its parts, of the expression of the angular momentum operator through the momentum component operators and others. Even as an example of the discrepancy between the results of quantum mechanics and classical physics, Feynman [5, Chapter 20-6] for some reason does not cite a quantity that has an independent physical meaning, but, in fact, a postulate of quantum mechanics — the Heisenberg uncertainty principle. On the other hand, Feynman himself cites as an example of non-commuting operators from classical physics — rotation to finite angles, which negates the "extraordinariness" of the differences between classical and quantum mechanics. Deducing the classical Newton's law, Feynman for some reason "calls" it "for average values" for no reason (otherwise this result would simply contradict the unfounded statements of quantum mechanics about the impossibility of the existence of trajectories in the microcosm). Note that when deriving an expression to change an arbitrary operator over time, Feynman writes full derivatives in time, and substitutes expressions for partial derivatives in time, that is, again admits inaccuracies in the calculations.

Feynman often admits statements of the following form: "to make it clear that it [the equation] is correct, I want to illustrate it with a simple example" (or he reduces one problem with some **specific** parameter setting to an already known problem). In fact, coincidences of this type — are just coincidences of particular solutions for this choice of specific parameters, and no finite number of particular examples **proves a more general statement**.

The fact that a system free from external influences can be in a stationary or quasi-stationary state (possess a certain set of physical quantities) does not guarantee that during the measurement itself (that is, forced interaction with the device), the spectrum of measured values of a certain physical quantity will remain the same as in if there is no interaction. Thus, from a **physical** (but not a mathematical model) viewpoint, the possibility of decomposition of **any state** by a set of quasi-stationary states remains a hypothesis (most likely incorrect, since the real possibility of measuring a quantity during the transition between quasi-stationary states is not taken into account at all). From the same point of view, determining the average value of a quantity only through a set of eigenfunctions of stationary states may also be non-rigorous (it is possible that the difference will increase as we move away from the ground state, when the number of variants of time-finite transitions to the underlying quasi-stationary states increases).

Often, eigenfunctions, contrary to the proof, are not mutually orthogonal (that is, additional mathematical actions are required). Introduction to quantum mechanics of operators for physical quantities — is another postulate of quantum mechanics. The subsequent definition of Hermitian, or self-adjoint operators — is only a consequence of this postulate and the real nature for all physical quantities (however, for linear equations it is sometimes convenient to use complex quantities, then purely mathematical games begin with using other operators, symmetrization, alternation, etc.).

For some reason, when determining the sum of operators, the possibility of a new operator having new eigenfunctions other than the eigenfunctions of each operator separately and eigenvalues other than the sum of eigenvalues is unproven excluded. This possibility can be easily seen from the following identical entry:

$$(\hat{f}+\hat{g})\psi \equiv (\hat{f}+\hat{z})\psi + (\hat{g}-\hat{z})\psi,$$

where $\hat{z}$ can be a completely arbitrary operator. For the same reason, even if the values $\hat{f}$ and $\hat{g}$ cannot have certain values at the same time (apparently, this is expressed in the difference in their set of proper functions $\psi_i^{(f)}$ and $\psi_i^{(g)}$), this does not mean that the summary operator has no eigenvalues and eigenfunctions. Apparently, for the product of operators, the situation may also be somewhat broader than it is used in quantum mechanics, for example:

$$\hat{f}\hat{g}\psi_n \equiv \hat{f}(\hat{g}\psi_n) = \hat{f}g_n\psi_k \equiv g_n(\hat{f}\psi_k) = g_n f_n \psi_n.$$

The question of commutativity for such a product of operators, or, as it is interpreted in quantum mechanics, about the simultaneous measurability of the quantities f and g may also have different answers. It is very strange that the property of commutativity (interpreted in quantum mechanics as simultaneous measurability) is not associative. That is, it does not follow from $\{\hat{f}, \hat{h}\} = 0$ and $\{\hat{g}, \hat{h}\} = 0$ that $\{\hat{f}, \hat{g}\} = 0$. Apparently, this means the biased nature of quantum mechanics' interpretation of the concept of simultaneous immeasurability for quantities as the simultaneous non-existence of such quantities.

Even the linear dependence on ψ is not a consequence of some obvious properties of Nature, but a consequence of the postulate of quantum mechanics (the principle of superposition); and the fact that the dependence is expressed by the first derivative in time is not justified at all. The definition of differentiation of operators in time is introduced formally mathematically (though strictly within the framework of the model). Therefore, the following phrase remains only faith, a verbal "justification" supporting the modern interpretation of quantum mechanics [3, p. 26]: "the concept of the derivative of a physical quantity in time cannot be defined in quantum mechanics in the sense that it has in classical mechanics", since allegedly the quantity "does not have any definite value at all in the following moments".

With the density matrix [3, p. 38], a solid comedy comes out. Are we going to do school substitutions instead of quantum mechanics? If the function $\psi(q, x)$ is known, then, according to quantum mechanics, everything is known to the maximum, and then why else define a certain density matrix (less informative)? If $\psi(q, x)$ is unknown (which in reality it is!), then the definition of $\rho(x, x')$ is made "on the sand"! It is also not proven that a subsystem that is part of a larger closed system can have independent (!) sets of physical quantities (allegedly, the operator does not act at all on the remaining, except for the selected subsystem, coordinates of the closed system): do you want to "simultaneously sit on two chairs located in different apartments" (in classical and quantum physics)? Isn't it funny for you to look at the derivation of the equation for the density matrix? Imagine if someone would say something like this: "some phenomenon, essentially determined by the action of, for example, electromagnetic forces, must obey the same equation as in the absence of these electromagnetic forces". But isn't the same thing done in [3, p. 39]: "the desired linear (?) differential(?) equation for $\rho(x, x')$ must also be satisfied **in the special case** when the system has a wave function". There is a little deception here — instead of the word "system" there should be the word "subsystem", since it is such a wave function that is substituted (and not the wave function of the entire system!). Further, having obtained an equation for such a special case, they call it the "desired equation". Maybe it's worth to turn back "from head down on legs": from the general equation, one could get an equation for a particular case, but by deducing the equation only **for a particular case**, it is not possible to guarantee the form of the equation **in the general case** at all! For some reason, no mathematical requirements are imposed on the decomposition of functions over a certain set (for example, in wave functions of eigenstates) at all, but in mathematics it is always required to prove that a series generally has a certain limit, that is, it converges.

The introduction of the momentum operator in [3, p. 42] is actually done postulatively (simply, a certain quantity was "called" by this name), in particular, the original function (for example, having jumps) can not always be restored by a linearized function which is Taylor series expansion up to the 2nd order. To what belongs, on the physical meaning, the proper functions of the momentum operator? They belong to a particle that is alone in the entire Universe and flies at a constant speed is unknown relative to what and even unknown where, since it is "smeared" throughout the Universe. A remarkably capacious concept! Generally speaking, quantum mechanics can be defined as an attempt to give a physical meaning instead of measurable quantities to some mathematical actions (operators). However, non-commuting operators obviously cannot have a physical meaning in classical physics, which means that for quantum mechanics it had to be "exclusively sucked out of the finger": to postulate the simultaneous immeasurability, for example, of coordinates and momentum. It is strange when in quantum mechanics we see the following statement of problems: "a certain wave function (or set of states) is given"' ...; even an example of the uncertainty relation in [3, p. 47] is given "by eye". And then what prevents you from setting the initial state in the same spirit – as completely deterministic? What prevents you from choosing a generalized function or a function with a break, a jump as a wave function (that could, for example, "compensate" for noncommutativity and eliminate the supposedly simultaneous immeasurability of quantities)?

A solid comedy comes out with H. Weyl's proof [3, p. 48] about the minimum product of fluctuations equal to $\hbar/2$. He starts with an integral, which **is automatically positive for any choice of** α, but gets a square trinomial, which is positive already not for all values of α. Now only when a certain condition is met, this becomes so again. But it is clear from the initial integral that by putting $x = 0$ and $p_x = 0$, we would also get a true equality for any α! Math game!

It is touching in quantum mechanics – to write integrals containing both momentum and coordinates at the same time. Sometimes, however, the limits of integration are "thievishly" not written — maybe they will not notice and will not remember that in quantum mechanics the momentum and coordinates are declared simultaneously non-existent. Fans of the quantum paradigm want to sit on two chairs located on different floors at the same time: how can an integral be strictly considered if the quantities included in it do not exist simultaneously with each other? They would have chosen one thing: either to use the proposed mathematics, or to trumpet about "uncertainties" and "novelty".

In quantum mechanics, there is always fun for hookmakers — right "field of miracles" some kind. We read in [3, p. 492]: "A number of important properties of the scattering amplitude can be established by studying it as a function of energy $\ldots E$, formally considered as a complex variable". We translate the "recipe" into an understandable language: a quantity is taken that does not have an independent meaning, in it the energy E loses its physical meaning and is replaced by the letter E with complex values (after r also will be complex value). "Pound water in a mortar" – this is "necessary to discover important properties of Nature"!

The following "masterpiece" ([3, p. 493]): "an expression as the asymptotic form ... may turn out to be illegal — a small term in it against the background of a large one may turn out to be an unacceptable excess of accuracy". What kind of nonsense (and the "severity" of the tone — for greater importance, so that you don't even doubt)?! What does "illegal" mean?! If they wanted to say approximate, then this is already obvious (from the word "asymptotic"), but how, for example, taking into account the addition of 10^{-10} can make **"illegal"** an approximate value of 1?! Even a terrible phrase was invented: "unacceptable excess of accuracy"! Unacceptable for what? For approximate calculations, any **improvement** is acceptable! Apparently, it interferes with globalist conclusions that claim to be strict. Further ([3, p. 493]), "self-cutting" is done – the requirement of exponential decrease of $U(r)$ is inserted, that is, all real potentials of finite powers are immediately "cut off" (it is not known whether Nature has anything left for such cases?).

So the apparatus of quantum mechanics — is not mathematics in the proper sense of the word (in terms of rigor and algorithmicity), and not physics (in terms of justification, measurement, connecting of results), but a kind of hybrid that lives off faith and fittings to in advance known results.

Chapter 8

Spin

Now let's go to the discussion of the concept of "spin" — allegedly a specific quantum "invention". Let's start with an experimental "justification".

When describing the filtering of atoms using the Stern-Gerlach device [4, Chapter 5-1], Feynman honestly says with regard to thought experiments that "no one has ever put all these experiments exactly in this way" (there are too many other "degrees of freedom" in the system), that is, the description is given for the purpose of **illustration** of the theory for trainees, which they should no longer doubt. But in fact, a number of questions immediately arise, for example, did the "three varieties" of atoms already exist **before** the experiment, or did they arise as a "response" to the effects as a result of the experiment itself? What properties were induced by the influence itself, or what changes in properties occurred at each stage: during acceleration, when entering an inhomogeneous magnetic field, during braking? How in general is it possible to "talk about something new in a new language" without a relationship with what was previously known and can be verified and recreated (for example, in mechanical analogies)? The next paragraph "Experiments with filtered atoms" shows only that the state of the atoms changes as a result of the influence of the device itself; and after then for sequentially connected filters, another conclusion can be drawn that the state of the atoms is induced by the experimental setup itself.

There is not a single "purely quantum" effect that does not have classical analogues, and "amplitudes" have nothing to do with it at all. For example, falling charged particles will either repel or attract, and the probability of hitting a given place will vary for them compared to the case of non-interacting uncharged particles. We can also recall the property of polarization of light and its passage through successive polarizers. Therefore, Feynman's statement looks very strange [4, Chapter 6-1]: "where our logic is most abstract, it always gives the right results there", but "when we try to build concrete models, we are unable to find a theory that agrees with the experiment". And the fact that all amplitudes can be multiplied by the same phase multiplier at the same time suggests that we are "playing mathematical games" with quantities that have no independent meaning (that is, this is not physics, but a set of mnemonic rules). In the next paragraph, in order not to think more about the phase multiplier, Feynman arbitrarily suggests taking "as a rule to bring the rotation matrix to the standard form" by dividing it by the root of the determinant. The remark about the independence of the results of the experiment from the orientation of the entire installation in space (with unknown accuracy and on unknown scales) is taken on faith as desirable (and it is unlikely that the uniformity and isotropy of space can ever be proved, but whether it will be refuted — is really a question of experience).

When describing turns around the Z axis in [4, Chapter 6-3], many questions remain unclear. It is stated that when the spin is oriented along the Z axis, such a rotation does not change the probability amplitude, but other experimental conditions are not specified. But in order for the beam to turn, it had to be **influenced by**. What is this impact? It turns out that "the amplitude does not change with this type of influence, in which the amplitude does not change". Very specific! We need links to real experiments (with a description of the device). Further, during the discussion, they "mentally" put **an additional** device that creates an x state. But then it is necessary to prove (experimentally) that this new addition does not affect everything else (for example, the z-state). Again, in the representation, this is still only a new assumption. But the "standard" way of constructing a "modern" physical theory ("with the help of a child's constructor") requires proof of the independence of the "components" (the absence of their mutual significant influence), otherwise some given "firmly established fact" cannot be used as a component in more complex mental and experimental constructions. Let's say we believe that only the probability phase changes. But after all, from the property of turns at small angles, it is impossible to draw a conclusion about turns at large angles in any way! From the only requirement that the physical result (probability) does not change when turning by 360°, there is no way to extract the dependence $\frac{1}{2}\varphi$ (for example, from an infinite number of possibilities it could be the dependence $\pi \sin\varphi$). With a description of the turns by 180° and by 90° around the Y axis – a solid "comedy". It was said about **a fundamentally new** language and suddenly: "in an inverted device T, both the **field** and the direction of its **gradient** are inverted; for a particle with a given direction of **magnetic moment**, **force** does not change". That is, after all, without classical concepts, models and interpretations of "neither here nor there"? The following "sucked out of the finger conclusion" also causes laughter: "the result of any rotation by 360° should be the same as when turning by 360° around the Z axis – all amplitudes should simply change the sign" (if when turning at 180°, a person stands on his head, then the blood flows to the head, but any turns around the vertical axis do not produce anything like that). It is completely strange: if a 360° turn in quantum mechanics does not lead to the same obviously previous state, but to a change in the signs of the amplitudes, then from where does the "additivity of turns" came (as if two turns by 180° give the same thing as the rotation by 360°; or the change of properties when turning by 180° equal to result with turn twice by 90°)? Therefore, all the conclusions of quantum mechanics based on the composition of turns are not proven, and even more so are not unambiguous. Generally speaking, "common sense" in physics should be used for quantities that have at least some physical meaning! Maybe it was worth looking for functions right away for probabilities?

Interestingly, has anyone checked for stability both states of orientation of spins in a magnetic field (for example, in classical physics they would have different stability and would not be completely equivalent). Also in the classics, the linear dependence of the Hamiltonian on the field $\mathbf{B}$ is only the first approximation. A general remark to quantum mechanics: the stationarity of the **state as a whole** (for example, energy levels) does not at all imply the stationarity of all parts of the system (for example, the orientation of spins, or some elements of motion), since dynamic equilibrium is also possible. Using the linear approximation of the Hamiltonian in combination with complex quantities gives more room for maneuver (fitting to a known answer), however, the presence of nonlinearities can cancel the use of similar mathematical techniques using the superposition principle.

The description of a rotating electron in a magnetic field is quite similar to the classical precession. The Pauli spin matrices in [4, Chapter 11-1] are introduced **purely formally** as a purely mathematical object. It is hardly possible to agree with Feynman's opinion [4, Chapter 11-1] that "nature knows quantum mechanics, classical mechanics is just an approximation, so there is nothing mysterious in the fact that because of classical mechanics, shadows of quantum mechanical laws that actually represent their background look out here and there". Rather, on the contrary — classical mechanics is "probed from-and-to" (and recreated), but quantum mechanics — is one of the possible types of interpretation adopted based on the observation of the "shadow theater" (that is, the "eternal hypothesis"). Unlike mathematics, games with "mathematical hooks" and deepening into abstraction cannot bring new results for physical science, since the "physical roots" (experimentally verified) are not added from mathematical games and remain the same.

Regarding the statement about spin, as if "this property of elementary particles is specifically quantum (disappearing when passing to the limit $\hbar \to 0$)" [3, p. 188]. It expresses only the personal **faith** of the authors. What kind of nonsense follows next: S_{mol} is a "given number" (in what units is it set, who checked and with what accuracy?), and when passing to the classical limit there should be $\hbar \to 0$ and $\hbar S \to 0$! How can a dimensional quantity (!) tied (in the model of quantum mechanics) to absolutely specific (!) properties of our World tend to zero?! Is it possible to remake the Universe?! A game of pseudo-mathematics! Therefore, there is no evidence that the existence of particle spin is impossible in classical mechanics (apparently, the phrase about the meaninglessness of studying the rotation of particles around its axis is also deliberately trying to consolidate the belief in the uniqueness of interpretations of quantum mechanics). If spin (as rotation around its own axis) is considered an analogue of the orbital moment (also rotational motion in space), then it is clear that in the model of quantum mechanics its projections on the chosen direction would also take a discrete value. But if we do not consider this (as it is now accepted), then the discreteness of its projections is another **additional hypothesis** of quantum mechanics (is not a lot of artificial postulates there?). Similarly, the rules of commutation of spin operators are also a hypothesis. The phrase [3, p. 189] is simply ridiculous: "the spin operator acts on the "spin variable", and not on the . . . coordinates, therefore, to obtain the desired commutation relations, we must consider the operation of an infinitesimal rotation in general, as a rotation of the coordinate system". Where is the logic?! If they say that spin has nothing to do with rotation, then what does the (infinitesimal) rotation operators have to do with it?! If we believe in such a postulation, then questions arise "from the other side": since the possibility of adding (in full moment) the orbital moment and spin is postulated, since they obey the same commutation rules (obtained with the help of rotations), and since for rotations in quantum mechanics these are — ALL properties, then, how can one arrogantly claim that spin is not related to the proper rotation of particles? Rather, on the contrary, this proves a similar connection!

In paragraph 55 "Spinors" [3, p. 191], the change in the wave function of a particle with spin σ during rotations is considered and the coordinate dependence is omitted. But this means, in fact, that we are considering a **point** particle — the first hypothesis (not proven); and the second hypothesis from here (and again not proven) — mathematical — that the wave function is representable as a product of the coordinate part by the spin part. After that again, the hypothesis follows (if we do not consider spin as the rotation of a particle around its axis, then this is precisely the hypothesis) that the infinitesimal rotation operator is expressed using the spin operator. The linearity of the operators — is another postulated hypothesis. Further, in paragraph 57 "The wave functions of particles with arbitrary spin" [3, p. 198] we read: " "The spin" properties of the wave equations, being essentially their properties with respect to the rotations of the coordinate system, are obviously identical for a particle with spin S and for a system of $n = 2S$ particles with spin 1/2 directed so that the total spin of the system is S". Why on earth?! I am translating into understandable language this next, unconfirmed hypothesis (more precisely, a postulate). From what was considered earlier (independence from coordinates; particle — in the center), $2S$ of particles must be **point** and fit all **into one point of space**, forming a new **point** particle. I don't remember such a case in Nature! ...Maybe I missed something?

In completely **classical** statistical physics, we also do not follow individual particles (this is its essence) and the density (probability) of the distribution will also be preserved with permutations of identical particles. And for technical reasons (our choice) this is happening, or supposedly fundamentally (which is unprovable) — it doesn't matter. However, in paragraph 61 "The principle of indistinguishability of similar particles" [3, p. 209], we again meet another "scientific-sounding-like activity" from scratch. Indeed, only the square of the modulus of the wave function has a physical meaning, and not the wave function itself. Therefore, with a twofold transposition of a pair of particles, the wave function does not have to pass into itself, but can contain an arbitrary phase multiplier $e^{i\alpha}$. Thus, the wave function can be not only 1) symmetric or 2) antisymmetric, but also 3) not symmetric and not antisymmetric. In mathematical terms, quantum mechanics significantly restricts the choice of possible functions to only the first two cases. It is necessary then to prove that, firstly, the restriction made allows us to consider any cases (which is postulated, but not proved even by examples of exact calculations — everything is based on “faith and an approximate hydrogen atom”), and, secondly, the discarded possible functions physically do not give new solutions (which is also not proven). There are more problems and contradictions in relativistic physics than there are any strict solutions, therefore, it is simply incorrect to refer to another (relativistic) theory that does not fit in with ordinary quantum mechanics — as an alleged proof. The mention of the Pauli principle in this paragraph is also absolutely unclear. So, the values χ_i “denote the totality of three coordinates and the projection of the spin of each of the particles”. But then, in classical physics also, we will not be able to place two (conditionally) balls at one point in space, because the place will already be occupied by one of them. On the other hand, according to quantum mechanics, the probability of the location of each of the particles is smeared throughout space. Therefore, in principle, at one point in space there may well become two electrons with the same spins, but in different energy states, which from a physical viewpoint raises great doubts (the point of space is one!).

How, after all, “adherents of the newfangled revolutions of the XX century” like to “turn everything upside down” and confuse the matter! “The electrical interaction of particles does not depend on their spins” [3, p. 212] (and then — another “oath to relativism” in a footnote). What does the word “electric” have to do with it? After all, in addition to the electric interaction, two **real** electrons participate in the magnetic interaction. And two magnets in classical physics interact differently depending on the mutual orientation of their poles and without any relativism. And if quantum mechanics claims to be able to describe the phenomena of the microcosm, then it is necessary to take into account the real, not fictional properties of microcosm. But in reality, the energy of the interaction of electrons (and the nucleus, whose properties are not yet taken into account either in the “proofs” or in calculations) depends on their mutual motion and on the mutual orientation of their spins. Solutions will also depend on this same configuration and will differ for this reason. And the principle of indistinguishability of particles (supposedly purely quantum mechanical) turns out to have nothing to do with this issue at all: no special “exchange” interaction will have to be invented if the real properties of the particles are immediately taken into account, and artificial “circumcision techniques” are not carried out through a known place. Since the “proofs” do not take into account the properties of the nucleus at all (and the characteristics of the motion of real electrons), then further reasoning in this paragraph about what moments and spins can be and what functions should be at the same time — is only a way to make a choice that does not contradict the previously postulated definition of the symmetry properties of the wave functions.

Many moments remain “behind the scenes”. For example, why are there only two possible values of the total spin for a system consisting of an arbitrary number of electrons: 0 and 1 (and is this so, or is it again — a way to be within the framework of the selected additional postulate about the properties of symmetry?), but for particles with spin greater than 1/2, there may not be two possible values, and more? It is known that the moment of momentum may depend on the point relative to which it is determined, and in an atom, when determining the angular moment, the motion of the nucleus is not taken into account at all. Jungian schemes — are also just a scientific-sounding-like way to assert faith in symmetry schemes. But, in general, why is the wave function considered **as the product** of the coordinate part by the spin part? The need for this has not been proven by anyone! But then even these fictitious symmetry properties should belong to the total function only, and nothing can be said separately about the distribution of its spin part. And how is it possible **according to the very ideology of quantum mechanics** to attribute certain coordinates to particles when determining which spin can be in a particular configuration — because electrons are distributed throughout space! One might as well forbid an arbitrary electron in an atom to have any spin projection, since surely somewhere in the Universe there is an atom with the same electron energy and the same spin projection. A "smeared" electron, being at any point in space, can, it turns out, have any energy (belong to any level) — and we can neither verify nor refute this. Yes, many mnemonic (fitting) rules are invented in quantum mechanics in order to artificially complicate the theory and discourage researchers from any desire to look for an alternative solution and explanation.

If, according to Dirac, there is an infinite sea of electrons that has filled all negative energy values, then the mass of this "formation" must be infinite; what about such a "new" gravitational paradox sucked out of a finger? Since it follows from the Dirac equation that particles can equally have negative mass and negative energy (the sum of their own and kinetic energies!), then **the only** case of having a physical meaning for such an equation — this is $m \equiv 0$. That is, this equation is for empty space (without particles and measuring instruments), and the void (0) is invariant with respect to any transformations.

Thus, "peculiar quantum" inventions also do not pull on logic and validity.

Chapter 9

Perturbation theory

Every competent physicist understands that some hypothetical absolutely strict quantum mechanics (with a single stationary wave function for the entire Universe) could not have any practical use. This means that it is necessary to pronounce some plausible spells and, in the image of well-developed classical physics, create methods for constructing approximate solutions using a limited set of parameters. For transitions, the perturbation theory works as a such addition, which we will begin to discuss.

The possibility to choose, instead of the wave functions ψ_n^i themselves, linear combinations of them as solutions (for example, in the case of degeneracy) indicates the ambiguity of quantum mechanics, because in these cases the experimentally observed electronic distributions in the atom will differ! And there is no answer to the question, what and how (on what principle) does Nature choose?! The situation is aggravated by taking into account possible transitions into a continuous spectrum (and this possibility is constantly present in Nature, and only model consideration can limit or exclude it): degeneracy is always present here (and energy alone is not enough to determine the state). As a result, there are always more possibilities than it takes to describe Nature in a predictable way, and then what is the value of such a theory (recall the objections to theories explaining the patterns of distribution for the orbits of the planets in the Solar System, but with the larger number of possible unobservable orbits)?

The "moment of truth" comes when considering time-dependent perturbations [3, p. 136], that is, for the only real formulation of the problem realized by Nature itself. In this case, it turns out that stationary states do not exist at all, that is, there is no such thing for which it was "invented" quantum mechanics as a separate science. Does the wave function itself even make sense in such cases — this refers to questions of faith (verification of this "hypothesis" can be carried out experimentally, but separately in each specific case; then what is the meaning of such a theory?).

The possibility of finding a convergent solution by successive approximations is not justified at all and, apparently, **cannot be justified in the general case**. For example, the smallness of perturbations does not guarantee this. Therefore, the phrase [3, p. 137] "in fact, in most cases the first approximation is sufficient" should be translated into honest language as follows: "further terms are often not tried to look for, so as not to accidentally run into a situation where one of the subsequent terms is larger than the previous ones and spoils the fitting". So, in the subsequent presentation, for a time-periodic perturbation, we see that additional conditions have to be introduced so that the denominators in the decomposition are not small, and for the case of the presence of a continuous spectrum (the only real case), a new additional condition — for frequency is obtained again.

The problem [3, p. 139] with a perturbation, the frequency of which is close to the frequency of the transition between the n and m levels, is also doubtful. It turns out in the answer that the probability of the state changes periodically for these levels. It is strange! Firstly, does the strictly deterministic (!) behavior of the system (when we know exactly the moment in time at which the system will be in a strictly defined state) contradict the ideology of quantum mechanics itself? And, secondly, it turned out that the behavior under strict resonance does not depend at all on the presence of other energy levels! Therefore, if we maintain an atom excited to the 100th level in a weak periodic field with a frequency (small) corresponding to the transition between the 100th and 101st levels, then the atom **will never go into a stationary state**. Of course, this is not the case. Thus, the rigor of quantum mechanics and its methods (and the practical **predictive** benefits) are in doubt.

The very ideology of the decomposition of a state that has arisen in new conditions, according to undisturbed states, is in some cases flawed (even in the classics, fundamentally new states may appear; recall the rotating whirligig, the Kapitsa pendulum, the Dzhanibekov effect, etc.). So, if a new stationary state arises, then it will not be possible to "catch" it in this way (recall for example, noise-induced transitions — see the book of the same name by Lefebvre and Horsthemke).

It is also interesting to note that in the absence of mathematical problems (for example, divergence), the rule is immediately declared "general": "according to the general rules, the square of the modulus of the coefficient $a_{kn}(\infty)$ determines the probability of ... being in the k-th stationary state" (see [3, pp. 141]). But as soon as numerically (**accidentally**) a situation is discovered in which the "general rule" stops, they immediately forget about its (imaginary) generality. For example, if the perturbation acts indefinitely long, then the integral diverges, and the probability of transition turns out to be infinite. In this case, the divergence is immediately declared insignificant, they discard one of the terms inside the module square (!) and now take the module square from the remaining term (of course, with the utterance of some plausible phrase-spell). That's cheating! After all, the probability can go beyond 1 (that is, lose its meaning) for not only infinite, but also finite time t! In such a case, if we want to determine the probability of a transition during this time t, then we, it turns out, should use a "truncated formula" (with one term thrown out)?! And for a slightly different time $t - \delta t$, it turns out that you can still use the "full formula"?! That is, in an infinitesimal amount of time, there is a causeless qualitative leap? But how could we know in advance in the experiment at what point the effect will be stopped (or is the choice of the "correct" formula carried out only "in hindsight"?)? Sheer cheating!

The "theater of the absurd" continues in the next paragraph, where transitions to continuous spectrum states occurring under the influence of periodic perturbation are considered [3, p. 146]. With "academic ease" Landau throws the phrase: "it is obvious from the results of paragraph 40 ...". How can anything be obvious from a paragraph where nothing has been considered and proved strictly?! For the function

$$|a_{\nu n}|^2 = |F_{\nu n}|^2 \frac{4\sin^2 \frac{\omega_{\nu n} - \omega}{2} t}{\hbar^2 (\omega_{\nu n} - \omega)^2}$$

he writes: "it is easy (?!) to see that for large t the function standing here can be represented as proportional to t". What nonsense! If $\omega \neq \omega_{\nu n}$, then it will be a periodically changing function **for any** t. If $\omega \to \omega_{\nu n}$, then any student will open the limit according to Lopital's rule and get (again **for any** t):

$$|a_{\nu n}|^2 = \frac{|F_{\nu n}|^2}{\hbar^2} t^2$$

(the same result would be obtained with $t \to 0$ and an arbitrary ω). It is also strange when in the formula (42,6):

$$w_{nE} = \frac{2\pi}{\hbar} \left| F_{E_n} \right|^2 ,$$

expressing the **dimensionless** value of the transition probability w_{nE}, the dimension F_{E_n} must correspond to the dimension $\sqrt{\hbar}$!

A solid "scientific-sounding-like verbiage" is also present in paragraph 43 "Transitions in the continuous spectrum" [3, p. 147]. The continuity of the spectrum is meant in energy, but it does not follow from anywhere that there is a continuous set of states with the same (!) energy from ν to $\nu + d\nu$. But the integral is taken by value of ν (just a "random" deceptive designation: in this paragraph, ν has nothing to do with frequency, that is, with energy!). Further. On the one hand, it is said that under the influence of a constant perturbation, the probability of transition is different from zero only when (!) $E_\nu = E_{\nu_0}$. But, on the other hand (even if we forget about the difficulties of **experimental distinction** of energy-degenerate states without external influence), after formula (43,2), "mathematical trickery" begins — "extraction the differential dE_ν of energy from $d\nu$". How did they manage to extract a continuous "piece of dE_ν" from one "point E_{ν_0}"?!

Section "The uncertainty relation for energy" [3, p. 150] represents the pinnacle of absurdity. Firstly, the system is divided into "two weakly interacting parts", and for each of them the "probability of transition under the influence of a **time-independent disturbance**" is considered. What nonsense!

It is impossible to imagine such a situation even in statistical physics (even in the classics with balls) as real: where did the fluctuations go?! The interaction simply cannot remain constant, but it will always contain the widest range of disturbances (especially when taking into account the smallness of the quantum of action). Secondly, even if we accidentally believe in the adequacy of the model constant perturbation, it was previously stated that with such a perturbation, the system makes transitions without changing energy (that is, between degenerate states). Hence, strict use of the formulas obtained would give $E' = E$! Thirdly, only a part of the formula is used in the output: the coefficient determining the magnitude of the disturbance is thrown out. For example, it is obvious for classical physics that the change in energy may depend on the magnitude of the disturbance. Fourth, the strange thing is the **sign-definite character** of the energy change (always growing!): it turns out that the more often we make measurements in the system, the more the energy grows. And this happens deterministically in a regular way (and not fluctuationally)! As a result, even for a closed system, we would get an increase in energy (violation of the conservation law). If we split the system into a very large number of subsystems, we can get **arbitrary energy growth** depending on the number of mentally allocated parts (we have uncertainty).

It is also interesting that from two records of the uncertainty ratio, which are completely identical from a mathematical point of view, namely, for energy-time and for momentums-coordinates, different conclusions have been drawn about the measurability and immeasurability of the corresponding physical quantities (and this is already a sign of Faith!).

The approach of the type "we read here, but we don't read there" (or of the type "don't ask unnecessary questions") can also be traced in the note [3, p. 151]: "it doesn't matter how the energy of the "measuring" particle becomes known". Similar statements [3, p. 154] about the energy of a particle in a well are also anecdotical: "...this is the order of magnitude for the kinetic energy, of which could a particle possess, if it enclosed in a volume with linear dimensions a (since, according to the uncertainty relation, its momentum would be of the order of $\hbar/a$)". Well, what does this uncertainty relation, associated **with the process of simultaneous measurement** of quantities, have to do with the **existence** of arbitrary **exact** values of a physical quantity without external influence (without this very measurement process)?!

Generally speaking, the mathematical ideology of quantum mechanics is also flawed, because **it is looking for a solution in all space**, and establishment of such a solution in infinite space (due to the finiteness of the speed of interactions) would take infinite time (and during such a time so many changes occur in the world, not even to mention the constantly present fluctuations). Perhaps that is why (mathematically) we have such a strange phenomenon, when for a three-dimensional case in a shallow potential well there may be no levels of negative energy, and for a one-dimensional and two-dimensional well, which are, in fact, special cases of a three-dimensional problem, such levels always exist; that is, paradoxically, with an increase in the number of dimensions, the number of possibilities decreases.

For the Coulomb field, it is obvious, contrary to everything written in [3, p. 155], that the potential energy cannot be considered as a perturbation, because the perturbed wave function diverges (!), and all the assumptions made earlier in the derivation of formulas turn out to be erroneous (again — "we count some part, but we don't count other part").

Generally speaking, the applicability of certain approximations in quantum mechanics (both methods and results) should be justified every time. And this is not at all due to the smallness of a particular magnitude or impact (as in classical physics), but from the analysis of "changes in all resonant conditions". After all, quantum mechanics — is, in fact, a description of some stationary interference patterns that are being realized, but they can significantly (!) change even with a slight change in conditions, impacts (or fluctuations, etc.).

Quantum mechanics fundamentally (!) does not satisfy some limit transitions. All arguments about the identity of particles "crumble to dust" if electrons can have individual characteristics (and it would be very strange if this were not the case!).

So, we see that such an important section of quantum mechanics for practice as perturbation theory is written with a "pitchfork on water" (it is not strictly justified and non-algorithmic).

Chapter 10

Quasiclassics. Limiting transition

It is obvious that different fields of knowledge and different scientific theories should fit together. As declared in textbooks, quantum mechanics satisfies the correspondence principle, that is, in the limit, a transition to the results of classical physics can be made. Is this so (and how rigorous)?

It is interesting to note that during the transition to quasiclassics [5, Chapter 19-4], replacement is necessary (we do not write dimensional coefficients)

$$l(l+1) \to L^2,$$

that is, the principle of conformity is not fully observed. And the apology that l should be large is not strict, because the transition should give a correspondence of $l \to L$, and the value of L is macroscopically measurable (there are no problems to distinguish experimentally L^2 from $L^2 + L$).

The analogy of the transition from wave to geometric optics and from quantum to classical mechanics [3, p. 20] cannot be complete. After all, in an electromagnetic wave, there is a **characteristic spatial dimension** of the order of the wavelength only along the wave, but the perpendicular dimensions of the beam are generally indeterminate (since electric and magnetic fields "oscillate", not spatial coordinates). In quantum mechanics, on the other hand, it is generally stated that there is no concept of a trajectory, and in principle, the transition could not be made at all: even for a point particle, the "perpendicular dimensions of the trajectory" are indefinite (finite) and, in general, they blur. Otherwise, it is not clear at all: if by formally tending $\hbar \to 0$ and the size of the wave packet to zero, it is possible from a less accurate quantum mechanical description to obtain a more accurate (in terms of the number of quantities and their accuracy) classical description, then why was it worth "fence round a new vegetable garden"? Note also that the operators themselves, when tending $\hbar \to 0$, do not turn into classical quantities, but only when using additional hypotheses of the limiting transition of equations and wave functions. Thus, a very strange (flawed) correspondence principle was obtained, artificially adjusted to the results of classical physics known in advance.

The description of the measurement process itself (which should also be connected with the classics) in [3, p. 21] leaves a painful impression: it seems as if they want to confuse the process, and not clarify it. The declaration that the system at some point can be divided into completely independent subsystems (with a wave function equal to the product of the wave functions of the parts) is a hypothesis, especially for charged particles with a long-range nature of interaction. The non-reproducibility of the measurement results for all quantities with the supposedly single exception — of the coordinate (and even then not for internal physical reasons of quantum mechanics, but because of love for the theory of relativity — allegedly infinite speed is unacceptable) — would mean the complete non-physicality of the proposed theory (but, apparently, the real situation is still different, and rather resembles the definition of the average over an ensemble of realizations from statistical physics). And one can simply laugh at the "fundamental significance of the deep irreversibility of the measurement process": how does this irreversibility differ from the simplest classical irreversibility of open systems?

When passing from the Schrodinger equation to the case of classical mechanics in [3, p. 52], the following points are striking. First: the probability density is always "moving" according to the laws of classical mechanics with the classical velocity $\mathbf{v}$ at each point and does not depend at all on any specific quantum mechanical effects (there are strictly no terms containing $\hbar$). Second: it just "hits the ears" when they talk about some transitions for physical dimensional (!) quantities, for example, $\hbar \to 0$. Why, $\hbar$ has a specific value (in what units is it small: in elephants? in monkeys? but in parrots it's much longer!)! So just to discard the whole term

$$\frac{\hbar^2}{2ma}\nabla a$$

is not allowed! It should be compared **with other dimensional terms** of the real part of the equation (at least some **dimensionless parameter** was singled out, otherwise it's just some kind of fraud).

A natural comedy is the description of the wave function in the quasi-classical case [3, p. 158]. Such decompositions of the type (46,3) by degrees of $\hbar$ can be introduced an infinite (!) set, because $\hbar$ is a dimensional quantity, and what is "hidden" in σ_n is complete uncertainty (at least all σ_n must have values corresponding to the denominator $\hbar^n$ by dimension, so that after reduction with the corresponding multipliers, the dimensions of each term coincide with the dimensions of σ_0 and σ). Consequently, there is no question of any decreasing series in this formula. Indeed, any dimensional quantity, depending on what it is compared with, can be both large and small. Therefore, decomposition by a small parameter always assumes that this parameter is dimensionless and much less than one. This means that there is no guarantee that some 101st term of the series (46,3) will not be greater than the "zero" term of the series, and that, in general, the series can be terminated on some term. For the same reason, the condition of "quasi-classicity" (46,6):

$$\left|\frac{d\lambda}{dx}\right| \ll 1$$

is not strictly justified, although the previous expression

$$\left|\frac{d}{dx}\left(\frac{\hbar}{\sigma'}\right)\right| \ll 1$$

is true! The inequality (46,7) is not strictly justified either:

$$\frac{m\hbar\,|F|}{p^3} \ll 1.$$

Thus, the "found" forms of approximate wave functions in the quasi-classical case are only **hypotheses**, which in each case must still be checked "for reasonableness" and compliance with experience. In what consists their valueness then?! Consequently, the limit expressions for quasi-classical functions — are also just (unfounded) assumptions.

Note that the Bohr-Sommerfeld quantization rule (48.2) [3, p. 163] includes only the region of classical particle motion! Therefore, if this part of the well is left unchanged, then, no matter how we deform the area that is not accessible to classical motion, this will not affect the solution at all (then what does quantum mechanics have to do with it, in general, with its "singularities", for example, under-barrier penetrations?).

When approximate calculations are used in classical physics — this is completely natural and does not contradict its paradigm, because the solutions are "local" (individual, independent of the rest of the world). Another thing is — in quantum mechanics: the wave function is distributed throughout space, and before getting its exact value at a specific point, it must be normalized throughout infinite space. Therefore, obtaining an approximate solution for a bounded domain makes questionable sense. In the best case, it gives only a form of dependence, without the exact value of the function even in this area (but we could find out this after comparing it with the exact solution or with the results of experiments *post factum* only; so what is the meaning of such pseudo-knowledge?).

Scientific-like estimates of the type [3, p. 170] are also questionable: "of the entire field of motion, it is the most significant (why is it significant, by what parameter, who calculated it and how?!) the part corresponding to the distances r at which $|U| \sim |E|$".

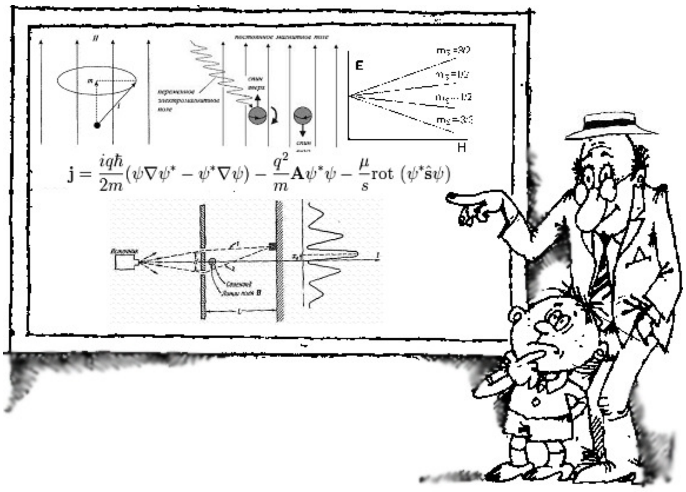
Reading the paragraph "Penetration through a potential barrier" [3, p. 171] one can understand why mathematicians do not like physicists: often the latter use mathematics only as demonstrative hooks for (completely lax) "proofs" of preplanned conclusions. So, neither rigorous solutions nor strict approximations are used in this section. For example, to the left of the barrier, the wave **is completely reflected**, that is, the barrier is considered completely impenetrable, and suddenly to the right — there is a penetrated wave. Inside the barrier, too, only one of the two functions is used; the accuracy of approximate functions is not evaluated at all, etc. As a result, we are told: "the formula is applicable when the expression standing in the exponent is large, so that the value of D itself is small" — this, apparently, to hide a possible question: how many times does this approximate D differ from its true value? Further, when they say that with a "sharp" barrier, the coefficient β^2 appears in the formula, they do not indicate that it contains not just a constant number, but, like the exponent, a functional dependence. (The first two "school" tasks for this paragraph on substitutions into the formula look just as funny.)

We read [3, p. 456]: "In quantum mechanics, the concept of rotation /for particles in a spherically symmetric field/ does not make any sense at all... The separation of the energy of a system into "internal" and "rotational" parts in quantum mechanics does not make strict sense at all". That's great! But what about the claims that classical physics is the limiting case of quantum mechanics? After all, in the classics, all this had a clear meaning (for example, the rotation of a completely homogeneous ball); when is this sense "born" in the process of passage to the limit? "The rotational structure of the levels appears ... as a result of ... rotation ... of the field with respect to a fixed coordinate system" [ibid.]. But if you think about it, in quantum mechanics, the coordinate system cannot be fixed (what should it be tied to and how?) — the relation of uncertainties hinders (and the question about the existence of integral objects suggests itself).

So, contrary to attempts to present the wishful thing as a reality, quantum mechanics with its passage to the limit of classical proven results is not so rosy.

Chapter 11

Movement in a magnetic field



The influence of the magnetic field on particles is well studied in classical electrodynamics; let's now move on to discussing the behavior of microparticles in a magnetic field from the viewpoint of quantum theory.

Pompous adherents of "new-fashionable physics" like to spit so much into the well of classical physics, from which they themselves constantly drink, that it just becomes disgusting. So, in [3, p. 421] it is written: "...spin directly interacts with the magnetic field. In the classical Hamilton function, this interaction is fully absent ..." And then the most usual **classical** expression (110,3) is obtained for a particle having a **magnetic moment**. And what was there to inflate the cheeks, maybe it would be better to note this coincidence (and use the analogy further)?!

We also read: "A particle with spin can also be attributed its own magnetic moment". Why turn everything upside down? The reality was historically different: **an own magnetic moment was experimentally registered** for particles, which allowed us to express the hypothesis about the presence of spin in them (again, it would be worth emphasizing the analogy with classical models, and not disavowing it).

We read further: "$\mathbf{p}$ and $\mathbf{A}$ are commutative if $\mathrm{div}\mathbf{A} = 0$. This, in particular, is the case for a homogeneous field if we select its vector potential in the form

$$\mathbf{A} = \frac{1}{2}[\mathbf{H} \times \mathbf{r}].''$$

And **IF** you choose the same vector potential in a completely different form, you will get something completely different (that's "great" – what I want, I turn)?! Then, let's pay attention to the following fact: so many "fabulous" properties were previously "deduced" based on the uniqueness of the wave function (and the "importance" of its symmetries), and suddenly, it turns out that the gradient invariance of the vector potential $\mathbf{A}$ requires a coordinated change in the wave function ψ (see (110.8)).

It also turns out that at one and the same time, a particle in the presence of a magnetic field cannot possesses even two definite velocity components. And this is fundamental! Again, it turns out "great" in quantum mechanics: influence (essence) — the magnetic field — has been added, and the possibility (certainty) has decreased. Otherwise, apparently, it will be impossible to adjust at least something to the answer known in advance from the experiment! And since **strict** equality of the magnetic field to zero is possible only in mathematics, and not in real Nature, then all previous quantum descriptions without a magnetic field generally lose their "great meaning".

Note also about the next "great accuracy" of quantum mechanics — the experimentally measured magnetic moments of **all** particles are not expressed in integers from the corresponding magnetons (ideally calculated theoretical quantities).

"On the other hand" in theory, a strict "fitting" has been performed in relation to the reversibility of time; only to whom and why is it necessary in practice (or a scientific-like game with unverifiable principles – is always a win-win, isn't it?!)?

In that time, when quantum mechanics received a discrete set of energies comparable to real spectra (for example, for a hydrogen atom at least somehow), it was evaluated as its triumph. But it remains a mystery what is the practical meaning of the expression for energy in paragraph 111 "Motion in a uniform magnetic field" [3, p. 424], if it turns out that "motion along the z axis is not quantized" and that energy levels are degenerate with continuous multiplicity (p_x runs through a continuous series of values)? Is it in an "exact-by-theory" scientific-like "solution" and in a large number of mathematical hooks?!

Let's consider the "explanation" of the Zeeman effect from the textbook [3, p. 427]. In its original form (the initial unnumbered formulas), the Hamiltonian is expressed in terms of a vector potential, which, as is known, is defined up to the gradient of an arbitrary function, that is, it can take **arbitrary values**. How, in the following formulas, can we talk about the smallness of a particular term and draw any neglect and conclusions from this? Just because we chosen the **particular** expression (110.6) as the mathematical expression for the quantity $\mathbf{A}$? And if you choose another expression equivalent in physical meaning for the quantity $\mathbf{A}$, can the final result be completely different?

As a lyrical digression: it would also be interesting to experimentally check what relation all the invented forms of vector potential recording convenient for calculations (and different invariances) have to the movement of microparticles in our only Universe, taking into account the well-known Aharonov-Bohm effect (the dependence of the effect on the magnitude of the potential itself).

Further, the textbook arbitrarily talks about a certain smallness of the field (without even specifying in what units we measure this field, that is, in comparison with what the smallness is manifested: for example, a meter compared to a kilometer is a small value, but compared to a millimeter is large). So, even the weakness of the field itself does not allow us to neglect the quadratic term in the field. It would be necessary to allocate **a small dimensionless parameter** to compare the ratio of quantities **of the same dimension**! Textbook does not explain why the external magnetic field has the ability to remove degeneracy, and the magnetic field of the nucleus and the electrons themselves — does not have such an ability (the nature of the field is one and the same!).

Then, the average values are mathematically handled carelessly. So, instead of (112,4) it should be

$$\overline{S}_z = M_J \frac{(J\overline{S})}{J^2},$$

but in general case

$$(J\overline{S}) \neq (\overline{J}\ \overline{S}) \neq (\overline{JS}) \neq (JS),$$

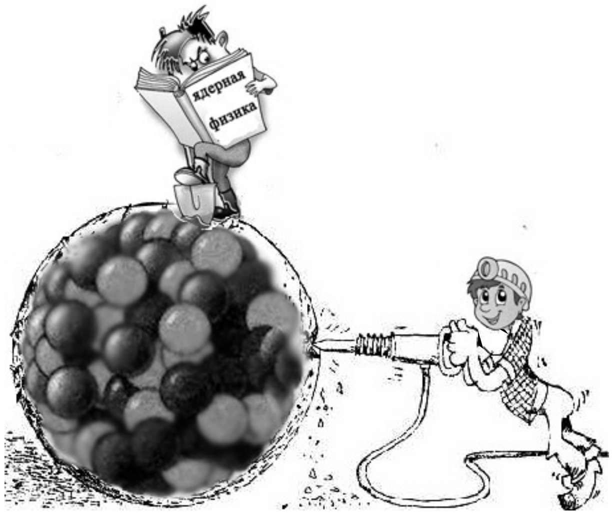
therefore, omitting the sign of the averaging is not strictly proved even with a "spell" of Russell-Saunders approximation.

The paragraph does not make a quantitative comparison of the expressions used with the discarded terms of the perturbation theory, but they turn out to prevail in some cases, that is, the experimental values differ from the theoretical dependencies. But at the same time, only a (favorable) special case was considered (in general case, as it is claimed, calculation is impossible at all).

Thus, the only real case in Nature is the movement of microparticles in a magnetic field, taking into account the presence of its own magnetic moments, is presented in quantum theory rather faintly (with poor physical justification, insufficient mathematical rigor and doubtful interpretations and conclusions).

Chapter 12

Atomic nucleus



Let's "dive" deeper now for research, to the very nucleus. After all, the number of nuclei is limited, which means that the expected successes of quantum mechanics should be more noticeable. Maybe everything is transparent there (there is no "muddy theoretical water")?

Question to nuclear forces: exist there "neutron" nuclei and why do heavy isotopes become unstable (after all, electromagnetic forces decrease, but, according to the alleged isotopic invariance, nuclear attracting forces remain)? Similarly, the key question of electromagnetism remains unresolved — about the size of the electron (not in theory, but in Nature!) and its internal characteristics.

An amazing "invention" is after all — quantum mechanics! Somewhere people fought in principle and are still fighting for the laws of conservation of energy and momentum, but here — please: **negative energy of a free particle**, or **imaginary momentum** (that is, negative kinetic energy of "motion"!). Interestingly, has anyone tried to reconcile with the law of conservation of momentum the force of attraction allegedly arising from the exchange of particles? If you try to attract a neighbor by throwing sandbags to each other, then you obviously won't get anything out of it (only solid repulsion). However in quantum mechanics, for some reason, this "trick" is used [4, Chapter 10-2] both to "explain" nuclear forces (using πmesons) and even to "explain" Coulomb interaction (using a virtual photon). Does no one really feel **complete nonsense of such pseudo-explanations** for fundamental interactions? Take, for example, an analogy. For nuclear forces, this would mean that a "soccer ball" (the first nucleon) begins to attract with another “soccer ball” (the second nucleon) at a distance of several sizes of the “ball”. Therefore, π-mesons (each of which is a sixth part of the nucleon by mass) from the 1st “ball” should fly in all directions so as not to “accidentally miss” the 2nd “ball” (after all, how can the 1st “ball” know in advance where will fly 2nd “ball”?). How is it that the nucleons “don’t wash off” from smearing yourself into particles like that? And why does the “delight” from such a pseudo-explanation of the attraction of **two nucleons** overshadow the legitimately arising questions about the mechanism of attraction for now **a much larger number** of π-mesons, which are “constantly shot” by a nucleon into the void, because all those, who did not collide with the 2nd nucleon, π-mesons must return to the 1st nucleon again, otherwise the latter would not have a certain mass (which is measured in experiments!)? And for the analogy of the Coulomb interaction with the football, in general, huge scales of attraction are obtained (clearly exceeding the size of the “football field”). Again, the same questions arise about the “flushing”, about the mechanism of attraction of virtual photons that did not hit the “target” to the 1st particle and their **exact return** to the “original owner” regardless of the nature of its subsequent movement!

Unfortunately, the disadvantage of “description models” (models that do not touch the physical essence of the process) is common — they are close to “mnemonic rules”, that is, they “work” only where it has already been verified that they work (and therefore their predictive power is zero for new situations and phenomena). I wonder if it is possible to specify a specific frequency for this hypothetical virtual photon (after all, the binding energy is certain)? And what happens if a plate is placed between two charged bodies that is opaque for such a frequency: will Coulomb attraction be completely shielded?

Feynman's statement is surprising that the masses of a proton and a neutron are almost equal for nuclear forces, but, on the contrary, a hydrogen atom is lighter (!) than a proton (the energy of an atom is H^0 less than the energy of a proton by mc^2 [4, Chapter 10-2]). Further, it is strange to transfer the process of the birth of a neutral meson π^0, observed **in high-energy collisions**, one-to-one to a "calm" nuclear attraction. It's like after a collision of trains to conclude that also from two trains standing side by side in rest, for no reason at all, will "splashing" fragments of glass and metal structures towards each other. It is also strange to talk about the emission of a photon by an electron, if for the photoelectric effect (the "triumph" of Einstein and quantum mechanics) such **was declared impossible**. Is it really now that everything that is born during high-energy collisions should be considered "flying out" of particles and contained inside them at rest?!

Let's move on to the chapter "The hyperfine splitting in hydrogen" [4, Chapter 12-1]. It seems to me that it is not entirely correct to state what spin can be responsible for without giving its definition and working model. The next "naive" question to the phrase "the spin of an electron can be directed either up, or down": where does the atom have the "top" and where is the "bottom"? It would be possible to talk about four (and not two) states if we turn on the magnetic field (then the highlighted direction appears). Since we are talking about "about a ten-millionth part of an electron volt", even minuscule thermal fluctuations ($T > 0$) will affect the system, that is, even in classical physics, we would be talking about dynamic equilibrium and about a statistical approach to the concept of energy levels in a real experiment. Note that according to Feynman, for a more rigorous description, in addition to spins, it would be necessary to take into account the momentums of both an electron and a proton and their difference for different "split" levels.

How to interpret the following landmark recognition of Feynman: "there is no general rule on how to write the Hamiltonian of an atomic system, and finding the right formula requires ... art" — as recognition of the fitting nature of quantum mechanics (or maybe not to "put on airs" and use proven classical results and methods)? Let's also think about the universally recognized (and seemingly true) phrase of Feynman: "if there is no external disturbance — something like a magnetic field that allocates some direction in space, then the Hamiltonian cannot depend on our choice of the direction of the axes x, y and z". Firstly, why does the disturbance (more precisely, the field) have to be external? Does any emitter (for example, an asymmetric one) have a spherically symmetrical radiation character (let's recall the radiation pattern of the emitter or antenna, the complex spectrum and directivity of synchrotron radiation, etc.)? After all, the very presence of a spin (more precisely, a magnetic moment) in a particle indicates its "incomplete" spherical symmetry and already highlights a certain direction in space. Secondly, what does the choice of axes x, y, z mean for a given orientation of the atom? If we calculate the integral radiation from an atom (for example, during transitions) along a full spherical angle, then, indeed, such characteristics cannot depend on the choice of a coordinate system. But if we are interested in the characteristics of frequency and direction, then almost always there will be a dependence on the direction and frequency (for example, an atom may not emit in a certain direction at all during transitions, and we will not fix them at a certain location of the detectors). Generally speaking, it is impossible to consider the mechanics of the microcosm in isolation from the detailed mechanisms of radiation! But quantum mechanics does not deal with mechanisms (taboo!). Thirdly, even the independence of some integral (or average) value from the choice of axes does not prove at all that all the terms in it must have the analogous property (which, in principle, may affect other measurable characteristics). But this is all a lyrical digression.

It would seem that Feynman is saying the right things [4, Chapter 12-1]: “it is best to start with the basis that **is physically most obvious**”; let’s remember this statement. And suddenly, after calculations, it turns out that states III and IV are not at all related to the choice made (see [4, Chapter 12-3]), but represent a linear combination of multidirectional spins (respectively, the sum and the difference). Thus, the spins are not directed at all as intended, but in a different way. How, then, does this differ from the various resonant states of systems of many bodies (for example, planetary rotation around its axis and the Sun), or from classical precession? So, the spins do not have to be directed **strictly** on the field or against the field?!

For Zeeman splitting, the Hamiltonian is simply postulated, or rather, written off “gritting its teeth” from classical physics (it is also assumed that the magnetic field does not change the interaction of an electron and a proton). The field $\mathbf{B}$ is simply obliged (and not as it is written in Feynman's "accept") to be directed along the z axis, since all previous and subsequent calculations (and the choice of "top" and "bottom") rely on this. The fact that with a large magnetic field, states III and IV tend to states $|+- >$ and $|-+ >$, respectively, also resembles the classical precession of a spinning top or a magnetic rotator in a magnetic field (with strong fields, the deflection angle is small).

One more serious problem is connected with the description of the splitting of levels in a magnetic field. In the hydrogen atom, the lowest level has split into four, that is, the number of levels is greater than the number of participating particles! If this description were correct, then, since nothing would fundamentally change for multielectronic systems, therefore, each level would be split into several levels in the magnetic field. And this would mean that matter would "collapse" in the magnetic field: all electrons from high levels would strive to occupy the new lower levels that appeared with **different** energy (with the release of radiation energy!). But no one is discovering this. Perhaps the situation is different: depending on the statistical orientation of a particular molecule, the steady (consistent, resonant, etc.) level in the magnetic field changes in a certain way. That is, transitions will not be in the same molecule between different levels, but **different levels** will be observed **in molecules of different orientation**.

The fact that there is still no complete theory of nuclear forces, there is a huge share of the responsibility of fanatical formalists trying to cram everything into the "Procrustean bed" of relativity theory and quantum mechanics. But quantum mechanics was originally created for rather narrow and specific purposes — describing the stability of atoms and obtaining their spectra (that is, only for electromagnetic interactions on microscales). Why did they decide to “inflate” it in such a way (to hyperbolize the scope of its applicability)? The myth “about the charge symmetry of nuclear forces” (which, in fact, is not always fulfilled, but only for light nuclei, and then very **approximately**) forces, firstly, to strongly limit the choice of solutions, for example, by **strictly** symmetric or antisymmetric wave functions [3, p. 438]; but in reality, they can be and neither the one, and nor the other. And, secondly, instead of searching for the essence of the phenomenon, studying specific mechanisms of forces and their manifestations (properties), it forces to engage in formalism — artificial introduction of isotopic invariance and the new quantum number (well, you just can’t live without it!). They completely overplayed — just nonsense: “approximate invariance”! And a number for which “the physical meaning remains unclear” [3, p. 439] — with further coding — to find out the meaning of this fictional “unknown what” and games with the introduction of new operators, laying out on familiar shelves (states).

The categoricalness of the claims of theorists is simply incomprehensible! So, in [3, p. 442] we read: “the velocities of nucleons in the nucleus are about 1/4 of the speed of light”. Firstly, who measured such speeds at such a distance (or again fitting on the tip of the pen to the theory)? Secondly, has anyone already proved that the nucleons in the nucleus are individualized (exist separately)? Thirdly, the temperature inside the nucleus should then be hundreds of millions of degrees, and the nucleons in the nuclei with the nuclei themselves would have to participate in establishing thermodynamic equilibrium with the environment, both through fields and during collisions (recall the objection to the existence of Lissagens). Fourth, that **inside the nucleus** it is possible to introduce the concept of the distance between nucleons (as points) and their spins — one can only **believe**. Therefore, the "determining" the nature of the dependence of potential energy on spins — is just an attempt to decompose the "unknown" into the usual "shelves" (an unsuccessful attempt, since the theory gives bad predictions even for such a finite number of objects). But if we approach strictly, the isotopic spin was introduced only approximately. This means that there may not exist its **discrete projections** at all, but some smoother continuous curves. In the absence of individualization of nucleons in the nucleus, the spins can also give not a discrete set of values, but other characteristics.

The goal of the theorists in this case is unclear: why classify by states that do not have quantitative predictive power, but need to "peek" into the experimental answer? Yes, knowing in advance the answer from the experiment, it is possible to approximate any curve with some accuracy by **fitting** coefficients using several given functions; but where is the science of physics here? And the fact that even with such a "fitting" mathematical technique, the results are obtained "not so much", makes an eloquent verdict on the methods of quantum mechanics in the field of nuclear physics.

In the shell model [3, p. 447], a self-consistent field is considered spherically symmetric. But this is not a proven hypothesis by anyone (it is especially difficult to imagine this, for example, for a deuteron)! In reality, the core field may even be unsteady (despite all the cries about wave radiation). An obvious puncture of the self-consistent field method is the fact that the wave function (constructed as a symmetrized sum of the products of the wave functions for individual particles) leads "to finite probabilities of velocity values other than zero" [3, p. 447]. They get out of a delicate situation as always — by deception, with the help of mathematical tricks: they do not change the argument in ψ, but they make changes in the desired quantities! But this technique contradicts all the previous mathematics of quantum mechanics. And physical quantities are obtained by "mutants". So, the dipole momentum operator has a funny look

$$\mathbf{d} = e\left(1 - \frac{z}{A}\right)\sum_p \mathbf{r}_p - e\frac{z}{A}\sum_n \mathbf{r}_n,$$

that is, it turns out that neutrons also "have" a charge! It's especially fun to look at special cases: for a **single** proton, it turns out that the dipole moment is determined by a **nonexistent** neutron (also for a ^2He nucleus — and it doesn't matter that the lifetime of such nuclei is finite!). By the way, the question is in the spirit of isotopic invariance: where did the nuclei with neutrons alone go, because nothing pushes them apart (there are no Coulomb forces) and any **nonzero** attractive force would be enough for the existence of stable (or, at least, quasi-stable) nuclei? So, for such objects, their dipole moment would be determined by non-existent protons (fortunately it is zero, but this is not essential). And for a deuteron, the contribution of a proton and a neutron is the same! After such "inventions of the charge of neutral particles", you can tell "any fairy tales" and pass them off as reality.

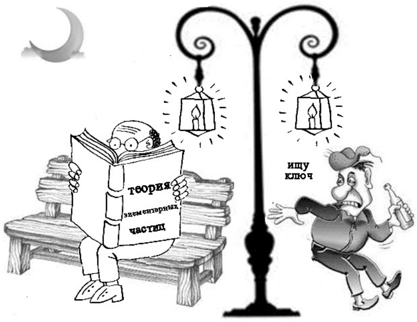
Attempts to determine the type of dependencies "for general reasons" using only the **the first** degrees of magnitude cause a smile. And the phrase "about decomposition" there will be no place here: you can compare either dimensionless quantities (with each other and a unit), or quantities of the same dimension (with each other). Otherwise, the coefficients for quantities of different dimensions can differ from each other in any way. Therefore, the phrases that this is a "pseudo-scalar" (for example, when determining the interaction of a nucleon with a self-consistent field) or about the absence of a preferred direction do not justify the restriction of the **reality** of choice in any way, but are only self-limitations of the **model** of a spherically symmetric field. When nuclei are classified by states, it seems as if someone could experimentally **simultaneously measure** all quantum numbers and energy. But in fact, the inverse problem is being "solved": somehow decompose the available experimental data for energies according to "theoretical quantum-mechanical shelves" (according to the principle: "if this — is close, then this is the same", **with attribution** of all other untested properties).

The determination of the spectra of nuclei, as always, is done "in hindsight" — what experience says, we will adjust the theory to that (we "will sort out on shelves" of either spherically symmetric solutions or of rotational spectra, for example, of an ellipsoid of rotation). Where is the predictive power of theory? Another "cheating" is observed when calculating the contribution to energy from the interaction of an odd nucleon with the centrifugal field of a rotating nucleus: although "in reality, the angular moment vector of the nucleon does not exist in the axial field of the nucleus" [3, p. 459], it is formally substituted into an expression, which is re-written from classical mechanics. Well, it's really necessary to "patch up Trishkin's caftan", otherwise it doesn't add up!

Thus, in the field of the nucleus, quantum mechanics is more like a collection of fairy tales than a strict algorithmic theory.

Chapter 13

Collision theory. Elementary particles. Photon



It would seem that since quantum mechanics began with the explanation of the photoelectric effect by introducing the concept of quanta, the whole triumph of this revolutionary theory should be highlighted most vividly when describing photons. Let's see if that's the case.

On the example of the paragraph "The polarization states of the photon" [4, Chapter 11-4], the "power of faith" in quantum mechanics is clearly visible (a scientist should differ from a "believer" in that he does not set himself a goal in advance to save his favorite theory at any cost, and when faced with contradictions in the theory, he will not hide them behind plausible phrases). So, Feynman himself writes: "there exists no three quarters of a photon. Either he is **entire** here, or he is not at all". And then follows the believer's passage: "And quantum mechanics also tells us that he is **entire** here, but 3/4 **of time**". Here's to you and hello! And what after the polaroid, this "one single photon" will still fly as a "3/4 part"? Or as a tracer bullet it will be "then we see, then we don't see"? Or it will 3/4 of the time fly, and then 1/4 of the time be absent? To save quantum mechanics, an unobserved kind of photon was invented!

The following remark refers to the note at the end of the same paragraph: "A photon is a particle with spin 1, which, however, does not have a 'zero'–state". That's great! Again — in the theory there should be such a spin projection, but there isn't one. Big deal! Let's assume that this is how it should be (in the theory, where there are more exceptions than rules, one more puncture, one less — nothing will change!), the main thing is — Faith!

And finally, at the end of the same paragraph, "...the phase (the phase relationship between states with the right and left circular polarizations) remembers the direction of x". Yes, such a situation was in classical physics, but after all, the phase multiplier (wave function or state function) in quantum mechanics has no independent physical meaning and is immeasurable!

Quantum mechanics could not come to terms with the status of a theory created to explain electromagnetic properties on atomic scales, it wanted to expand the scope of applicability to all phenomena of the microcosm. But are such claims adequate?

Unfortunately, several fields that can hardly be called scientific have "stuck" to the science of physics (both theories of relativity — SRT and GRT — together with the modern cosmology based on them can be attributed to unscientific fiction; it seems that the modern "theory" of "elementary" particles should also be counted as pseudo-scientific activity). In the paragraph "The neutral K-meson" [4, Chapter 11-5], Feynman talks about the "new law of conservation of strangeness" (and how many such pseudo-laws have been invented since then for non-physical quantities with a "smell", then with other "glucks"!). The whole "strangeness" lies in the fact that this **number is attributed to the particles after** passing the reaction (that is, simply **is introduced for systematization**) and cannot be confirmed by anything else (in independent way). In addition, reactions involving neutral particles (in this case $K^\circ, \overline{K}^\circ, \Lambda^\circ$, etc.) **are not visible** and they are judged again indirectly (already at some distance!), **after** some other reactions (before which a lot of things could happen "in a dark room"). Further, it turns out that the strangeness is not always preserved (for example, for Λ-particles in a weak interaction), that is, a neutral particle may not "give a report" on what type of interaction it decayed (for example, the final products of weak decays K° and $\overline{K}^{\circ}$ mesons are the same). In general, the law acts where it acts and when it acts, and where it does not act, it does not act there. Remarkably informative!

Let's give prizes for all such pseudo-laws (millions and more will be quickly made up), we just need to decide who will be allowed to be considered honorary authors of such "great inventions". In general, starting with the fact that K°-meson is not capable of generating Λ°-particles, suddenly after the work of Gellman and Paice, it turns out that it is still capable (preliminary transition to $\overline{K}^{\circ}$ does not change the essence of the "law" of preserving strangeness). It is not at all surprising that for its "complex and amazing result" they were unable to calculate the theoretical values of the quantities α and β included in the probability amplitude

$$C_1(t) = C_1(0)e^{-\beta t}e^{-i\alpha t}.$$

This is always the case with pseudo-theories: when it comes to science-sounding-likeness or the game "what looks like what", they cope, but when it comes to quantitative verification, they say to you: "(why check?) believe in theory, and take numerical values from experiment, that is, fit *post factum*".

Feynman's remark also makes one think: "since two π-mesons have infinitely many states depending on their impulses, integration over all possibilities leads to a value of α equal to infinity. But the natural value of α not infinite". Again, a remarkable property of quantum mechanics: if we take almost nothing into account, we get something similar to reality, but if we try to take into account all **observed** effects (that is, to improve the approximation), we often get an discomfiture of quantum mechanics.

For a system with n states, it also turns out "I turn what I want" (not the rigor of the method) [4, Chapter 11-6]. Starting with unconditional faith in the equations, they get a set of eigenenergies E_n with a corresponding set of eigenvalues for each energy $C_i(n)$, but if multiples of the energy $E_i = E_j$ with non-orthogonal eigenstates are suddenly found, then they immediately **forget** about the rigor of the equations and **change the coefficients found** so that the eigenstates turn out to be orthogonal. However, games using linear combinations work only under the assumption of strict correctness to the principle of superposition, that is, faith in the strict linearity of the World.

One more remark. When the Lesage hypothesis was discussed at the time, there was the following objection to it: particles should fly at a very high speed and, as a result, the established temperature in the Universe would be enormous. But after all, electrons in atoms fly at speeds only hundreds of times less than c, the speeds are chaotically distributed (at least due to the arbitrary orientation of the atoms relative to each other). What about the steady-state temperature of matter to say (or is it necessary to postulate that electrons, even in outer orbits, do not participate in establishing a balance?)? The presence of free electrons in plasma and in metals also raises the question of establishing a large equilibrium temperature (or are they also obliged not to interact with the external electrons of atoms?).

Quantum mechanics cannot do without imitation of classical physics! In [3, p. 469] we read: "The problem of elastic collision, like any problem of two bodies, is reduced to the problem of scattering of one particle with a reduced mass in the field $U(r)$ of a stationary force center". We translate into understandable language: real bodies are replaced by material points and all their coordinates (and the distances between them) are determined **absolutely exactly** (see the argument in $U(r)$). Therefore, according to quantum mechanics, there should be **complete uncertainty** in determining the velocities of all particles (before and after the collision), and, therefore, their directions, including the directions of scattering (for "chewing lovers", we can say a little longer: the exact fixation of r makes the value of v_r uncertain, but with simultaneous fixation of the value of E, the scattering angle also becomes indeterminate).

Another point arises in connection with the "requirements" of another fantastic theory — special relativity: we observe any process for a finite time Δt, which means there is uncertainty in the definition of energy ΔE, which means there should (supposedly) be uncertainty in the definition of masses $\Delta m = \Delta E/c^2$ (after all, c is a great constant —taboo!). In quantum mechanics, it is impossible to prove that the mass of the same particle, determined in different experiments, remains the same! Again, this gives rise to uncertainty in the scattering angle θ. But, of course, if you act as usual in pseudo-theories: here they "didn't notice", there they "greased", here they "covered up", here they "adjusted", then you can "hold a false smile" for a long time, as if everything is in order.

The phrase about limiting the particle flow with a wide but finite diaphragm (ostensibly to avoid interference effects [3, p. 470]) is also a fitting to classical physics. The solution in quantum mechanics essentially depends on the boundaries and should be defined **as an unified whole** according to its own principles of quantum mechanics (recall, for example, resonant tunneling through a barrier system, when the probability of passing one barrier can be made arbitrarily small, but the particle beam will pass through the entire barrier system completely). Further, having obtained a formula linking one (!) an unknown quantity (section) with an infinite number (!) of other unknown quantities of phases (δ_l), the comedy begins: they calculate **the order of magnitude (!)** of phases δ_l. And this is made for a quantity standing under the sine and, therefore, changing its value from 1 to 0 when changing this very δ_l by only $\pi/2$! Yes, it also turns out with this "great" quantum mechanical achievement: if the phases turn out to be finite — but the integral can still diverge! However false theories never lose heart and do not admit their mistakes ("Tell more lies, something will remain" – Goebbels): in this case, a non-existent meaning is also invented.

Interestingly, the scattering amplitude at the zero angle will be infinite when the field decays slower than

$$U \sim r^{-n}, \qquad n \leq 3,$$

that is, in all real cases. And then why all this hook-making?! In quantum mechanics, also some of its theorems represent games with hooks, for example, the reciprocity theorem: states are not exactly known, but there is a certain "mathematical hook" that allows **exactly (!)** to reverse the movements of particles (but what about the lack of exact coordinates and velocities; with a probabilistic description; with the uncertainty relation?).

"Proof" of the transition of the quasi-classical case into classical expressions for the scattering angle [3, p. 486] represents just some kind of anecdote! The **scattering amplitude** is taken, that is, a quantity that does not have an independent physical meaning in quantum mechanics, the part corresponding to the zero angle is thrown out. Then, under the pretext that the phases δ_l should be taken large, for all expressions their asymptotics are taken (**approximations**) and again **are additionally discarded** terms with small l (naturally, without specifying what "large" or "small" means). Of course, such manipulations with a quantity without physical meaning cannot lead to the acquisition of physical meaning by "its remainder". Then, from the resulting "trim", a "condition" is obtained, in which an even smaller "**separate** trim" leads to an extremum of one of the exponents. Substituting δ_l (**already** obtained from quasi-classical ideas) into this condition, they obtain a classical equation linking the scattering angle with the aiming distance, in which **all quantities** have a classical (measurably) physical meaning (there are no claims to mathematics in the last substitutions). There is "no smell of physics" here, but for pseudoscientists this is not the main thing — the hooks converge!

When we consider the motion of electrons in an atom, the introduction of an angular momentum is quite justified, but when, during scattering **of an external flow** of particles at the force center $U(r)$, in addition to fixing the distance r, the scattering angle θ and the energy E (or k), they introduce the parameter

l, the question naturally arises about the physical meaning of such an artificial concept. Interestingly, even the conditions for the applicability of solutions for different "partial" amplitudes are different [3, p. 502]: the larger l, the faster the same potential $U(r)$ should decrease ($< 1/r^{2l+3}$)! But this means that for some large l, the result (and the neglect of f_l) is incorrect! In addition, "abnormal" contributions may be present [3, p. 503]. Another "interesting" result (puncture): scattering on an impenetrable sphere gives for the section [3, p. 504]: $\sigma = 4\pi a^2$, that is, 4 times more than the classical expression. But the result does not contain the quantum constant $\hbar$ at all, so there is no limit transition to the classical result, obvious, for example, for macroscopic balls!

It is strange that at zero energy, most approximate expressions for scattering cross sections turn to infinity (it would seem that if there is no directional motion, then there should be no scattering).

Further [3, p. 505] paragraph 131 is called "**resonance** scattering at low energies", but in fact $E > 0$, and the level of $\varepsilon < 0$, that is, $E \neq \varepsilon$. Do not cost to distort clearly defined concepts! The methods of this paragraph for obtaining approximate expressions cause only a smile, especially at first, a completely arbitrary choice of $|\varepsilon|$ as a characteristic energy is made, then — virtual level (why not, for example, choose $\kappa = 0$ – after all, the effect will be even greater?). Have these solutions been tested for cases allowing a strict solution? Or is it all — "games of the order of magnitude"?

There are many **"formal"** (*ad hoc* — fitting) methods in quantum mechanics. Thus, in [3, p. 511], for a quasi-stationary state, an unnormalized wave function is considered in the form of a divergent spherical wave; it is stated that "this corresponds to a particle that eventually flies out of the system during its decay". Deception of workers! This formulation of the problem (with divergent waves) corresponds to a constant (stationary!) the flow of scattered particles, not the output of a single particle. And in order to make the resulting solution look like the truth, instead of real energies (they make the second **formal** step), they substitute non-existent complex energy values and compose a plausible meaning for all this. In general, another game of "what it looks like"! The forgery is immediately obvious, since by definition $|\psi|^2 \leq 1$, but in this case, the non-infinite probability increases with time (and is not normalized!).

In the Coulomb field, the collision problem can be solved exactly [3, p. 516]. However what do we see in this case? The solution is sought not as spherically symmetric (!), but in parabolic coordinates. In justification, a scientific-sounding phrase is written: "in the presence of a selected direction (in this case — directions of the selected particle)"' ... And what, in other cases it was not so?! And the energy turns out to be a positive quantity (and not, for example, a complex function!). That's just a plane wave at $z \to -\infty$ does not work — it is always distorted, and the diverging waves are also not spherical, but distorted. But, despite such a "monstrous" difference from the entire previous the "general" quantum mechanical theory of elastic scattering, the classical Rutherford formula is obtained for the cross section.

The ambiguity of the quantum-mechanical approach to the scattering process is evident from the fact that in a number of problems converging spherical waves are used instead of divergent spherical waves [3, p. 522]. In fact, from a mathematical viewpoint, **both** these functions can always be used equally, since they are both solutions of the Schrodinger equation. That is, the existing **reality of the experiment** is "driven" into the framework of the quantum mechanical apparatus, only the interpretative "**explanation of the choice with hindsight**" is changed.

When describing the collision of identical particles and determining the scattering cross section in [3, p. 524], they write: "Averaging is necessary over all possible spin states, considering them all equally probable". Recall that, according to quantum mechanics itself, the spins of particles in a field have not arbitrary directions, but a discrete set of projections onto this field (with an unproven "equal probability"). But will the situation correspond to the case of the absence of fields when real charged particles with their own magnetic moments collide? But are, for example, all projections of spins equally probable in an atom? Then there would not be a certain order when filling the atomic shells. Who and for which particles experimentally proved the **hypothesis** about an equally probable distribution of spin states? It is noteworthy that not only in the classical limit ($l^2 \gg v\hbar$), but also in the opposite case ($l^2 \ll v\hbar$), the cross section does not depend on $\hbar$ at all! At the same time, it is even strange that in the classical limit, this "independence" is **only statistical** [3, p. 525]: "the transition occurs in a very peculiar way ... **when averaging** over a small range of values of θ, the oscillating term ... disappears, and we come to the classical formula". I wonder if anyone in the experiment analyzed statistically this scattering cross section — not only the average, but also deviations from the average: do the points with the right density fit on the resulting quantum mechanical curve (or it is enough faith and fitting the hooks to a pre-known average result)?

Coincidences of approximate calculations by the order of magnitude or by the type of curve cannot be considered in favor of quantum mechanics, since often the order of magnitude or the type of curve can be obtained from elementary considerations (common sense). The confirmations of the theory can be attributed only to those cases when, within the limits of experimental errors, the calculations coincide with the experimental curve and the statistical spread of the data (and not just the average) it also fits into the theory (and how many such analyses have been done?). Often the choice in approximate calculations is made completely arbitrarily. So, for example, in [3, p. 529] for resonant scattering of charged particles, in view of the logarithmic divergence of the derivative χ'/χ, the point ρ is chosen arbitrarily. Elastic scattering in the presence of inelastic processes [3, p. 539] is also introduced not from a strict theory, but formally: using common sense and estimates, the result is "driven" into the framework of a quantum mechanical description (with obvious results).

The following statement looks strange [3, p. 548]: "with a decrease in velocity, the role of inelastic processes increases in comparison with elastic scattering". Moreover, the same statements are made for all partial sections of the reaction! (is it that as the lower the temperature, then the more nuclear and thermonuclear reactions occur?!) But after all, in Nature, almost all inelastic reaction channels have an energy threshold, while elastic scattering is — threshold-free. This means that the role of inelastic scattering could increase with a decrease in velocity only to a certain threshold value, and then with a decrease in velocity it should begin to decrease. It is also strange that the conditions of the "legality" of quantum mechanical conclusions become **more rigid** (for decrease of the potential) for the expansion terms (in l), declared as **playing a small role**. Such inconsistency immediately calls into question the correctness of the general conclusions drawn.

The following statement causes a smile [3, p. 560]: "...the detailed energy course of the cross sections ...is very complicated. This complexity makes it difficult ...to detect any systematic changes in the properties of sections ...In this regard, it makes sense to consider the course of sections ..., averaged over energy intervals ...". That's great, we translate into understandable language: "we do not know and do not want to understand the causes of patients' diseases and methods of their research — it's very difficult; let's make it easier —we find the average temperature in the hospital (and give everyone either temperature-lowering pills or temperature-raising pills) and brilliantly confirm our theory and high academic rank".

In [3, p. 565] we read: "Near the threshold, the relative velocity v' of the formed particles is small. Such a reaction is the reverse of a reaction in which the velocity of colliding particles is small". From the fact that the driver gets out of the car at a low speed, it does not follow in any way that a pedestrian, whom the car will collide with at the same low speed, will be sitting behind the wheel of this car. Glasses with drinks sometimes fall and break, but it is unlikely that with equal probability someone saw that glass fragments and a puddle from a drink would combine into a full glass and jump back on the table. The hypothesis of the existence and equal probability of a direct and reverse reaction can be believed for **true** elementary particles. In the presence of the internal structure of elementary particles (for example, neutrons) this is equivalent to the hypothesis of the absence of irreversibility on such a scale — and does it have convincing evidence?

The phrase from [3, p. 571]: “we can always consider the one of both electrons that has the highest velocity after the collision to be scattered”, — simply contradicts the quantum mechanical principle of particle identity. It should, according to quantum mechanics itself, sound like this: “Due to the principle of particle identity, we cannot distinguish which of the electrons is scattered and which — flew out of the atom as a result of ionization, and therefore **both equal options** should be taken into account **simultaneously**”. (Which is not done.)

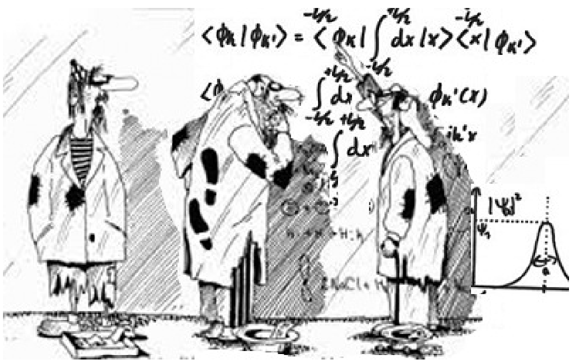
The following statement [3, p. 572] is not at all strict from a mathematical viewpoint: “Due to the orthogonality of the functions ψ_n and ψ_0, the term in U containing the interaction of Ze^2/r with the kernel disappears when integrated by $d\tau$”. The transition from a truly stationary center of inertia to a supposedly stationary kernel is a transition from an inertial reference system to a non-inertial system and is therefore illegal (quantum mechanical formulas are not intended for non-inertial reference systems). For example, in a hydrogen atom, both a proton and an electron can rotate around a common center of inertia **only with the same frequency** (otherwise the center of inertia will cease to be the center of inertia by definition).

Any harmonics that will be inherent in the movement of the electron, immediately by virtue of the principle of interaction will be reflected on the proton and will be inherent in it (the difference is only in the "amplitudes" of motion). In principle, the situation will not change for heavier nuclei: the harmonics of electrons are associated with the corresponding harmonics of the nucleus (only with an increase in the mass of the nucleus, their amplitude will decrease). Thus, there will be no strict "disappearance" of the contribution from the kernel. Further in the same place, the formal replacement of the integral by the Fourier component of the potential assumes that all charges are strictly point-like and the potential retains its form up to the singularity. In practice, this is not the case, all particles have a finite size (and even structure). Further, the substitution of approximate expressions with subsequent integration may give an inaccurate result, since the "quadratic effect of oscillations" may be lost. Also, the alleged writing of dependencies is not impressive, but with unknown constants (even in the order of magnitude it is not known whether they hit?). And finally, there is no comparison with experimental data anywhere in the textbook [3]. What are the real values, dependencies, deviations on average and statistical spread?

Thus, the too broad claims of quantum mechanics to cover all the phenomena of the microcosm clearly do not correspond to its real capabilities.

Chapter 14

Applications of Quantum Physics



Let us now analyze more closely the attempts of real practical applications of the theoretical achievements of the "omnipresent" quantum mechanics to the surrounding reality.

Nature has many examples of riddles and oddities in store for us. Why and by what mechanism do charged particles interact with each other (the possibility of attraction is especially unclear)? How can small electrons bind together molecules and matter as a whole? Why can a substance be solid if, according to modern views, there is more emptiness there than any "substance"?

As for Feynman's opinion [5, Chapter 13-1] that "one should expect an electron to fly into one or another atom almost immediately" — this is not obvious due to the ratio of the sizes of microparticles and the surrounding "emptiness". But what is really strange: why in some cases an electron is able to pass through matter (for a conductor), and in other cases — not (similarly, there are differences for different wavelengths of light and other waves). Feynman begins to describe conductivity as a natural state (assuming its presence in advance!) in a regular crystal, but after all, amorphous bodies, alloys, and liquids (mercury, for example, or electrolytes) can have conductivity.

Note the following. When considering the process of electron propagation in a crystal lattice [5, Chapter 13-1], since all neighboring states in the lattice are the same for it, then it can only be an external (arrived, or in Feynman's words **extra**) electron, that is, the lattice becomes charged. And how does this **additional** electron change the very levels of the atom where it is located (can this be neglected)?

The question of whether solutions have additional meaning at $|k| > \pi/b$ (b is the distance between atoms) remains open, contrary to Feynman's opinion [5, Chapter 13-2]. First, for amplitudes

$$C_n = \exp[ikx_n - (i/\hbar)Et]$$

you can determine the speed of the wave

$$\frac{x}{t} = \frac{E}{\hbar k},$$

and it will change not purely periodically with the growth of k. Secondly, do amplitudes at a point make sense at all? If we consider the amplitudes in the entire crystal region (and not only coinciding at the nodes), then, of course, they will differ at such k from those obtained for the interval $|k| \leq \pi/b$.

Further [5, Chapter 13-3] we read: "We have just revealed an amazing secret — how an electron in a crystal ... can sweep through the entire crystal, can fly through it completely freely, even if it has to collide with all the atoms". About the "sweep" — there are big doubts, since the directional velocities of conduction electrons in the metal are completely minuscule (about millimeters per second), at huge disordered speeds. About "revealed the secret" — it is also doubtful: after all, from the resulting formula

$$v = \frac{2Ab^2}{\hbar}k,$$

it follows, in fact, that all crystals must conduct current, and this is far from the case. Just as the speed of molecules in the air has absolutely nothing to do with the speed of atmospheric wind, so the speed of independent movement of electrons does not have to be related to the speed of their directional flow (electric current).

The following, as always, Feynman's honest confession [5, Chapter 13-5]: "only a little can be said about various coefficients, such as the emerging amplitude A, ... their value have to take from experience", — speaks for itself about the "practical benefits" of quantum mechanics in these matters (fitting is carried out retroactively).

In the section "Scattering from imperfections in the lattice" [5, Chapter 13-6], in fact, not the motion of a single electron is considered, but a stationary process: a constantly incoming stream of non-interacting particles and steady flows of particles that have passed through the "pollution atom"' and have reflected from this isolated atom.

To check (and confirm) the formula for conductivity in semiconductors [5, Chapter 14-2]

$$\sigma = \frac{N_n q_n^2 \tau_n}{m_n},$$

it would be necessary to be able to directly independently measure **all** quantities $(\sigma, N_n, q_n, \tau_n, m_n)$. By and large, there is such possibility not for all quantities, especially given the incomprehensible, rather fitting status of the effective charge q_n and the effective mass m_n.

A remark can also be made to the description of the Hall effect [5, Chapter 14-3]: the potential difference seems to be created mainly by **free excess charges**, and for some reason the theory assumes in advance that the velocity of directional drift in a semiconductor for positive and negative charged particles is the same (which may be not so, then the interpretation of the Hall coefficient, more precisely, its sign may change).

It is strange that for spin waves the effective mass of a magnon

$$m_{eff} = \frac{\hbar^2}{2Ab^2}$$

turned out to be the same as for an electron propagating in a lattice (compare [5, Chapter 13-3 and Chapter 15-1]). It turns out a strange picture: that the whole electron is flying through the lattice, that it is standing still, but only its spin has turned over — the same energy expenditure!

If for the summary energy of two spin waves, they can be considered independent [5, Chapter 15-2], then, despite the plausible statement that "there are countless terms in the Hamiltonian", nevertheless, different expressions are obtained for the amplitudes (and for the probability itself). Thus, not for all physical quantities, the approximations made give **equally correct** observed numerical results. The "arguments" from [5, Chapter 15-3] that consider magnons to be Bose particles are very poorly substantiated, because in reality there is still the same electron. It is his spin that can be directed either up or down in this case, and this state of affairs is no different from a free electron, which also orients itself either along the field or against the field; when the spin changes in both such cases, "the spin changes by one". Apparently, after all, the need to consider magnon as a boson is taken "in hindsight" (to coordinate the calculations of quantum mechanics with the results of experiments when using these objects).

Feynman's confessions are always landmark [5, Chapter 15-4]: "the reason why a physicist manages to deduce something from the basic principles is that he chooses only simple tasks. So far, he has been able to calculate with decent accuracy only a hydrogen atom and a helium atom". These are great "breakthroughs" of quantum mechanics (and even they — with "patches": even for the simplest hydrogen atom, the calculated values of the energies for the levels are shifted relative to the experimental values)!

About chemical applications in the same place, Feynman says that for the same molecule, depending on the type of reaction, it is necessary to consider the value of A different (that is, to engage in fitting). Experiment in real life is always ahead of theory (only in textbooks of theoretical physics — on the contrary): both in detecting the stability of chemical bonds, and in detecting "magic" numbers in nuclear physics, etc. With regard to chemical molecules, it is not at all obvious that the imposition of periodic conditions on an infinite linear crystal chain should lead to the same results as for finite molecules (for example, closed ones [5, Chapter 15-4]).

For the study of superconductivity, they have already given more than one Nobel prize, but nothing has changed (except for a few successful random experimental discoveries). None of the newly minted "theories" could answer the most important questions: how to calculate (based on simpler or known data) for a given substance the transition temperature to the superconducting state T_s, the critical field B_c and other measurable and important physical quantities? How to find the composition of a substance with predefined parameters: T_s, B_c and other properties? Instead, the theories are based on the principle of some similarity of individual qualities of the phenomenon (for example, they try to "explain" superconductivity with an equally "understood" superfluidity).

The interaction of electrons with the lattice would be more understandable from the viewpoint of ordinary classical resonances than mystical pairing when "the average distance between pairs is less (!) than the size of a single pair" [5, Chapter 21-5]. How do they bind "over the heads" of several neighbors, and why do pairs need to be considered here at all, and not a single wave function of the entire system (where did the fundamental requirements of the new physics evaporate?)? Simply again, the start was taken with the **belief** in the quantum mechanical model, and the **size was adjusted** to the one required for the theory (it is not possible to confirm or refute the value of this pair size by any direct experiments).

And, in general, what does bosons have to do with it?! Even the most nimble of them — photons — **and then not all pass** through crystals (there are areas of opacity, for example, in the same superconducting metals, attenuation, scattering, etc.); for crystals, the passage of photons may well be understood from the point of view of ordinary classical resonant frequencies. And among the particles, other bosons are well-known, which do not necessarily all pass through matter unhindered. The existing theory does not address the main question at all: why do electrons cease to be scattered by densely arranged atoms of matter at all (in fact, they cease to participate in the irreversible temperature exchange of energy — remember the problem that once arose against the idea of Lissagens).

We read on. A calculated explanation of many effects in superconductivity (ideal diamagnetism, Meissner phenomenon, etc.) was proposed "long before people understood the quantum mechanical origin of the effect" [5, Chapter 21-6], that is, it means that there was no special need for such a quantum mechanical "explanation".

When calculating the depth of penetration of a magnetic field into a superconductor [5, Chapter 21-6], Feynman arbitrarily takes only a solution decreasing in depth for **A** from the two resulting solutions ($exp[-\lambda x]$ and $exp[+\lambda x]$): "it cannot increase — there will be an explosion". Firstly, the quantity of **A** itself does not have an independent physical meaning (otherwise many substitutions and invariances "fly") and therefore can take arbitrary values (and for an "explosion", it would be worth giving more physical arguments). Secondly, there must be an argument why the second type of solution is rejected for conductors of **finite size** (recall, for example, potential barriers: if they are infinite, then only a damped solution is taken; if they are finite, then both types of solutions are taken into account and adjusting is performed at the boundaries). The argument that the current density is zero in the depth of the ring is not strict either (at least in relation to the magnitude of those effects under consideration — quantization of the flow, etc.); and mathematicians could laugh enough at the "argument" with a difference from zero for gradient along a closed trajectory. In essence, if you believe in the rigor of the equations and in derived "physical" arguments, then you will not get the result of quantization of the flow; but from a simple expression for ψ with a phase multiplier, such a plausible statement could be uttered.

Generally speaking, oriented electron spins (and not only and not necessarily the superconductivity current) can contribute to the conservation of the field inside the ring (or to pushing it beyond the surface and to other effects), then the quantities can also occur quantized in the meaning of multiplicity to electronic characteristics. And this is more understandable also from the classical viewpoint. It is also interesting that in the equations of superconductivity dynamics [5, Chapter 21-8], "quantum mechanical energy" "in all practical applications can be neglected" for one superconducting region, and again classical equations are obtained (even simplified, that is, model ones!). The dependence of current on frequency in the Josephson transition theory and various resonant effects may well have a classical origin (at least, there is nothing unusual here).

Quantum mechanics is full of statements like "something, they say, is insignificant", for example, [3, p. 380]: "we will completely neglect the influence of spin, since in polyatomic molecules this influence is, generally speaking, negligible". How can you say in advance that something is insignificant without research? For example, according to one theory, it is believed that spin is responsible for magnetism and there are already magnetic chemical materials made of complex molecules; also, high-temperature superconductivity has been found in substances with very complex molecules, and spin also plays, as it is believed, "not the least role" there.

Quantum mechanics cannot do without declarative pro-relativistic statements: "the magnetic interaction of particles with each other is a relativistic effect" [3, p. 421]. Why on earth would that be? What is moving there at near-light speeds (and how did they not notice this until the XX century, and how did they do without this?!)? And further, quantum mechanics simply “writes off” the known classical results and expressions without any additional justification of the expressions, and the physical meaning of the quantities used (for example, for generalized momentum and vector potential).

Surprisingly accurately and humorously about the problems of quantum mechanics is written in the article by O.H. Derevensky “Hocus-pocus of quantum theory” http://newfiz.narod.ru/qua-opus.htm . This article is worth reading to every physicist “to remove the blinders from the eyes”! And also in order not to repeat the “parrot spell”: “this is a purely quantum mechanical effect that has no classical analogues”. Here is a summary of some key points and questions from this article.

1. So, Planck’s semi-empirical (fitting) formula for the spectral energy density of equilibrium radiation has maxima at different points in units of kT for the frequency form of recording and for the wavelength form of recording (respectively $4.97kT$ and $2.82kT$), which, of course, excludes the alleged “exact experimental confirmation”, at least for one of these forms of recording.

2. Radio waves tens of thousands of kilometers long, since they are photons, are emitted and absorbed instantly — according to the resolution of the Ist Solvay Congress (from the series “obvious punctures of the modern interpretation of quantum mechanics”).

3. With what amplitude and how many times should the

oscillations occur for the emission of one quantum? The question is not idle. There observes no stable (static) interference pattern from different lasers with the same frequency ω (also a tricky question!), therefore, interference is possible only for the same quantum. But interference is possible with differences of millions of λ, which means that the quantum length must be very large. And since the resolution of the telescope improves with increasing aperture, then the width of the quantum should also be very large. Then how does the eye see, or how does radiation-absorption by a small atom occur (what is the real physical mechanism?)?

4. What is the physical mechanism of the occurrence of oscillations with a specific frequency ω_{mn} during the transition from one fixed orbit m to another given orbit n?

5. What keeps the atom from collapsing if there is no classical motion (centrifugal force, etc.)? That is, the problem of the stability of the atom has not been solved, because of which quantum mechanics was allegedly needed.

6. What forces ensure the stability of atoms and their spectra and restore the orbits of electrons after constant collisions of atoms with each other in matter?

7. It is failed to split the electron beam in two (as with heavier atoms!) — does an electron have a spin?!

8. How is the energy of a quantum (and an electron) smeared in accordance with its wave function (the localization issue is also far from trivial)?

9. How is the $NaCl$ molecule formed? The first. Thermal energy is not enough to ionize the Na atom. The second. In the

Cl^- ion, the new electron will be bound weaker than in Na^0. The third. According to the well-known Irnshaw theorem, a static system of electric charges cannot be stable ...

10. The whole band theory is a one-electron approximation (i.e. pure deception): an electron interacts only with a static ion backbone and does not interact with other electrons; the band theory does not predict the magnitude of the electrical conductivity of specific metals.

11. The declaration in the textbooks of quantum mechanics even about the diffraction of electrons at two slits is pure deception, since the de Broglie wavelength of an electron is of the order of the distance between atoms, which means that it is impossible to create and overlap such slits.

12. If atoms radiated and absorbed only exactly at resonant energies (frequencies), then, firstly, they would not participate in the equilibrium radiation exchange (!), and, secondly, there would be no molecules, since there is no exact coincidence of these resonant energies.

13. Experiments show that photoelectrons fly out **towards** quanta! Is there a phenomenon of light pressure in this case: Lebedev's experiments give the force for a mirror reflector a value 1.2-1.3 times greater than that for a black reflector (and not 2 times, as it should be according to theory!), so these are radiometric forces. Are you saying that photons tend to occupy one level? Then, why did the experimenters have been trying to achieve a single-mode laser regime for so long?

14. Regarding the propagation of electromagnetic fields in a vacuum: for oscillatory movements, a force returning to equi-

librium is needed — what can oscillate in the void? And in general, the field, as a continuous medium, has an infinite number of degrees of freedom, each of which accounts for kT, that is, the energy of the field would have to be infinite for any $T \neq 0$! The problem with infinite divergences, for example, with the self-action of the field, is also not solved. In the quantized form, the problems of the field concept have not gone away either!

15. The Mossbauer effect cannot be associated with interaction with the crystal **as a whole** (recoil), since there is anisotropy in crystals for this effect. For iron, the Mossbauer absorption is observed up to $1046°K$, although the Debye temperature for iron is much lower and equal to $467°K$!

In general, be sure to read the original article, you will not regret it! There this and much more is written in more detail and fascinating.

Thus, no matter how you hold an “important face”, and the recipes for the practical application of quantum mechanics to surrounding phenomena give a solid “sour” in the dry residue.

Conclusion

So, having analyzed the current state of quantum mechanics and realistically assessing its "successes", we see that neither the theoretical foundation, nor the mathematical implementation of the theory, nor practical methods and results clearly shine with physical, logical or mathematical validity, with rigor or algorithmicity. In the most optimistic case, quantum mechanics could be given the status of an approximate probability theory in some areas. But most of it is more like a solid pile of mnemonic rules, *ad hoc* hypotheses (for a specific special case) and retroactive fittings for a result known in advance from experience.

In the paragraph "What are the base states of the world" [4, Chapter 8-3] Feynman tries to predict "what the general quantum mechanical description of nature will turn out to be" and honestly admits that we do not know this yet. I also agree that it is not known whether the current generally accepted approach related to the separation of phenomena (by scale, time, energy, etc.) is correct. In fact, if we know (know how to describe) some phenomenon, then, based on quantum mechanical concepts, we can assume how to describe similar phenomena, but nothing we cannot say with certainty about the description of more general phenomena. In particular, it may turn out that taking into account the internal structure of particles and movements may lead to the fact that the description (on other principles) will be more accurate than allowed by quantum mechanics (peculiar hidden parameters). After all, it has now been discovered that it is possible to circumvent the prohibitions imposed by the wave theory on localization, focusing, etc. (nanotechnology is developing). And since quantum mechanics was based on an analogy with wave mechanics, it is possible that many prohibitions of quantum mechanics will also be overcome (for example, the uncertainty principle). I think the future will prove it. For me, at least, it is obvious that in its current form, quantum mechanics cannot be considered a model of a strict scientific theory and in the future will be replaced by more advanced theories.

Appendix:

Brief remarks on related and alternative theories

At the present time, the spectrum of physical theories is extremely extensive: from highly scientific to highly absurd (pseudoscientific), and a significant part of highly absurd theories belongs, as it does not sound regrettable, to academic science. One of the formal auxiliary criteria for separating highly scientific and highly absurd theories can be considered the ratio of the number of hook-making, namely the number of artificial principles, auxiliary concepts, mathematical calculations and immeasurable quantities, to the number of experimentally verified results. For experimental results, good phenomenology and highly scientific theories, this indicator ranges from one (experiment) to a dozen, and for highly absurd false theories it amounts to many tens, hundreds and thousands of units (there is no limit to stupidity; and there are no moral brakes on untruth).

In the age of the free Internet and a huge information flow, it is an impossible task to make any serious review of all alternative ideas in the field of the microcosm, and the author is not an expert on alternative theories (if someone wants to seriously deal with them, it is better to read the primary sources). Although many alternative proposals are at the initial stage of development and are being developed only by individual researchers, there are quite a lot of well-developed ideas and entire research directions. Therefore, here only some ideas concerning the microcosm will be briefly mentioned for completeness, some very superficial comments and assessments are given (the author apologizes in advance for not being able to analyze all alternative theories, even with which he is familiar).

Let's start with an obvious remark. If some theory relies on another false theory (for example, special or general relativity, relativistic cosmology, the Big Bang theory, etc.) or includes it, then it is immediately obvious that the result can only be another new false theory. An example here is string theory (and superstrings), trying to synthesize relativity theory (false theory) and quantum mechanics (temporary construction) into a single monster — to cross a hedgehog with a snake and get barbed wire to control physicists. Naturally, in order to en masse enlist some suckers with a fairy tale and receive funding from other suckers (without fear of exposure in the next hundred years), the string level is assigned to the deepest — under the subatomic level. Well, of course: after all, we have studied all the previous levels "very thoroughly" (the end of physics is coming!)! False scientists have few fairy tales about four-dimensional space, black holes and wormholes, dark energy and dark matter! Give them a 10- or even 26-dimensional space-time out of greed! There is no physics in this, but what poetry and what scope for mathematical games! These "theories" and similar ones (M-theory, loop quantum gravity) do not even want to mention, let alone analyze.

The following remark. If a new theory includes the theory considered in this book unchanged and tries to expand it, then it automatically transfers to itself all the "childhood diseases" of this theory found in the book (unreasonableness, contradictions, problems, shortcomings). Such theories include, for example, quantum electrodynamics, quantum chromodynamics. Naturally, their own specific problems will be added to the existing problems (infinite vacuum energy and its gravitational field, dubious renormalization, divergence of series and integrals, fundamentally unrecoverable particles, fantastic colors and bad smells of other invented supposedly quantum numbers, etc.), and the total number of problems can only increase. The results of all these super-matematized theories can be counted on the fingers, and the noise around them in the media has been artificially raised so much (and during quite a long period of time) that one might think that they are a panacea for Humanity. I don't want to attract additional attention to these mathematical toys, they have enough already.

Also, we will not discuss idealistic theories (the dream of God, information, software and projection theories, etc.) and theories of the universe (polarization, lepton, wave, unified, etc.), since we are interested in experimentally verifiable results specifically in those areas that have been studied in this book (it is impossible to embrace the immensity).

Of course, it is unreasonable and inadequate to demand from alternative theories that their authors explain and describe completely all the phenomena that were considered before them by hundreds of thousands of previous researchers. It is possible to evaluate only what was specifically done by the authors, and compare with similar results of their predecessors on the subject under consideration. Conventionally, all theories can be divided into two groups: 1) theories describing only the observed phenomena and not going beyond the level that can now be experimentally investigated, and 2) theories trying not only to detect patterns, but also to look inside the described phenomena and find their cause.

The following remark concerns theories that generally reject the existence of particles, and everything in the world is considered by them as the generation of wave structures (waves, vortices, solitons, etc.). In addition to the natural refutation associated with the limited stability of such formations and their inability to self-repair after interactions (particles retain their identifiably discrete properties), it should be recalled that wave formations pass through each other, and particles collide and even bounce off each other (are reflected). Take, for example, the Fourier decomposition over the entire space: harmonics do not interact without a medium, and how to determine which harmonic belongs to what in our vast Universe?

The Copenhagen interpretation of quantum mechanics did not suit very many researchers, who continued to search for "hidden parameters" (D. Bohm), refutation or alternative explanations. One of the alternatives to modern quantum mechanics is an attempt to describe the classical phenomena of the microcosm using the theory of fluctuations (according to the type of work "Noise-Induced Transitions", Horsthemke W., Lefever R.). Indeed, theoretically, discrete levels can arise under the influence of noise, and transitions can occur between them. But these phenomena can only lie at a deeper level than the experimentally studied phenomena. Therefore, in addition to complicated mathematics, we will have to go not from cause to effect, but in reverse order: according to the consequence, based on faith in equations and invisible processes, to deduce (ambiguously) the causes. And wait for science to descend to the next deep level and confirm our conclusions. It is possible that we will live to see the result.

Another alternative to quantum mechanics can be called the pilot wave theory (D. Bohm). New experiments (A. Steinberg) using the approach of the so-called "weak measurements", in fact, proved that, contrary to quantum mechanics, particles have a certain trajectory, for example, they pass through one specific slit. This is to be expected. What do you say — well done (honest experiments should always be welcomed)!

It is obvious that all ethereal theories are precisely physical theories that try to penetrate deep into things and understand the causes and mechanisms of phenomena (in contrast to the pseudo-mathematical nature of many modern theories), that is, they belong to the second group. Ethereal theories have the most enemies (both among highly educated semi-mathematicians and semi-physicists and among specialists who thoughtlessly believe in near-scientific advertising), demanding the impossible from these theories: to immediately explain all the phenomena existing in the world (turning a blind eye to the fact that modern theories have not only failed in explanation of all the phenomena, but also have many problems and internal contradictions). The theories of the ether are very diverse, even it would be difficult to list all the authors, so let's just give some characteristic examples. For example, it is also a gas ether (V.A. Atsyukovsky; P.D. Prussov), and electron-positron or photonic ether (A.V. Rykov), and granular ether (A.I. Zakazchikov), and domain ether (K.A. Haidarov), and variously charged ether (F.F. Gorbatsevich), and ether having a charge of one sign (V.I. Mirkin), and solid ether (E.V. Gusev), and liquid ether (V.M. Antonov), and many others. The particles of the ether itself can also be isotropic, and anisotropic, and of several varieties, and have a number of complex properties, and transform, etc. Some theories are quite well developed; which directions can be seriously analyzed? It is obvious that only a set of experimentally confirmed new predictions could confirm or refute this or that theory, or force them to abandon all (it is clear that the experiments advertised by universally recognized science cannot be considered critical). In the meantime, we can make the following comments on the "internal" problems of such theories. If ether particles are able to transform, then what is the mechanism of self-restoration and maintenance of experimentally verified identity and discreteness of many objects of our world? For ether particles with complex properties, problems arise again to explain these properties (their causes and mechanisms of occurrence and action). For example, if we consider ether particles with charges of both signs, then the previous unresolved questions remain: what forces hold each charge as a whole, what is the mechanism of attraction of charges of the opposite sign (that is, the questions are again transferred to a deeper level)? Why aren't the charges neutralized? and others . If the ether consists of repelling particles of the same sign, then why is our world not purely gaseous (but it is also condensed into solid and liquid objects)? For the solid ether, the main "internal" questions are: what holds this solid formation together, and how explain the mechanism of movement through it without braking for objects of completely different sizes and energies from galaxies to elementary particles (yes, photons can pass through a crystal, and electrons move in a metal, but this happens in a solid body only for some objects and in a limited range of energies).

What would we like to expect from any theory? At least: 1) an internally consistent, consequtive approach to phenomena; 2) algorithmic description of the entire complex of phenomena under consideration in a single way (without particular hypotheses for each particular case, without peeping in response); 3) obtaining all experimentally measured quantities from the first principles, and not mathematized games with artificially invented hooks; 4) new experimentally verifiable predictions; 5) if possible, explanations of the causes and mechanisms of phenomena.

Afterword

I have no doubt that if the truth that
three angles of a triangle are equal to two angles
of a square contradicted someone's right to power
or the interests of those who already have authority,
then the teaching of geometry would be, if not disputed,
then supplanted by the burning of all books on geometry.
(Thomas Hobbes)

Someone may think: "Why do we need such critical books at all, especially since a ready-made theory is not offered instead?" I will answer. All work must be done in the right quality and quantity, in the right place and at the right time, otherwise it is — "Sisyphean labor". Currently, as the attitude of researchers to new fundamental ideas shows, the academic scientific community is not yet ready to accept any new theories, even in the field of "controlled" by quantum mechanics.

Of course, many have already encountered some particular inconsistencies and problems of the theory under consideration, but they were hardly familiar with the whole system of fittings, frauds, inconsistencies, artificial hypotheses and internal problems. Therefore, the task of this Part I of the book was to "take the blinders off your eyes", to help researchers think independently about existing problems and attitude to them. And for this it is necessary not to take out of memory the "expromptu" preparations once learned, but to learn to look at everything consciously, "with open eyes"; To know why certain physical definitions, ideas, laws, and methods were once adopted "at the fork of the road"; Be able to evaluate the ideas of the past from the perspective of the facts and experiences accumulated to date and, if necessary, return to the same "fork in the road" and make a more correct choice.

Unfortunately, since the so-called "great revolutions in physics" there have been certain changes for the worse. The scientific community has gradually turned from bright independent personalities honestly interested in the Truth into an extremely inert gray colossus, where the proportion of true scientists is relatively small (although they determine what remains in science for centuries). The process of self-purification and self-organization has practically stopped working. Currently, several groups can be conditionally distinguished in the scientific community: 1) true scientists, 2) simply paid scientific workers, 3) science officials, 4) false scientists.

Although the number of pseudo-scientists (who are ready to say "white" for "black" in their selfish interests) is relatively small, they own almost all the "advertising time" (modern cosmology and both theories of relativity should be attributed to false theories in the area of responsibility of official science).

Science officials only maintain a strict bureaucratic order, they "keep their nose to the wind", ready turn out to be "with a flag ahead" of the prevailing opinion.

The overwhelming part of the scientific community is simply paid scientific workers. Many of them directly say that science — is what money is paid for, and they are ready to do any monetary work anywhere and as the conjuncture requires. Many are ready to work honestly, but within the limits of the "red flags" placed by someone. And a significant part of scientists do not even think about what science is and about the moral aspects of a scientist's activity (it seems that they have a stereotype of an ever-hurrying digger embedded in their subconscious, ready to pass off any find as a treasure and eager for recognition as the ultimate goal).

The position of a True scientist is perfectly highlighted in the following statement. **Who wants to reveal the truth, he no less diligently searches for it and in the beliefs or assumptions of the opponent ... He tries to help the opponent find words for his thought that would most accurately express it. He tries to understand the opponent better than the latter understands himself. Instead of using every weak point of the opponent's argument to overthrow, debunk and destroy the cause that he defends, the participant in the substantive discussion makes efforts to extract from the statements of the opponent all that is valuable that will help to reveal the truth.** (T. Kotarbinsky)

How many people treat the search for Truth and the methods of discussion like it is done by True scientists? It is not necessary to perceive the discussion of scientific theories in the spirit of animal instincts of competition "for a place in the Sun"! Let's finally move away from the vicious practice of "sweeping problems under the carpet", and, on the contrary, let's honestly report inconsistencies in physical theories, contradictions with other facts or proven theories, non-algorithmic techniques, additional *ad hoc* hypotheses, unsolved physical, philosophical, methodological or mathematical problems. When these problems are honestly highlighted, any researcher will be able to try to solve them; and if our generation cannot do it, then surely the next generations will be able to do it. It is important that each new generation does not have to "secretly dig these problems out from under the carpet" from scratch, and the youngest and most productive years could focus on thinking about and solving them. (For example, mathematical books with a title beginning with the words "Unsolved problems..." are always inspiring, unlike the whining of some "outstanding" physicists about the end of science.)

It would be nice if the state, as the main sponsor of science, developed criteria for an independent assessment of the moral qualities of a scientist, his honesty and fairness in carrying out his work and evaluating the work of other scientists. At least, even the very formulation of such questions would make many think. Maybe then the process of self-purification of science from real pseudo-scientists in power, from the cronyism and authoritarianism of science officials would resume. It would be great that those who are engaged in science not to look for "their place under the Sun" in this field of activity, but to engage in a real search for Truth. I wish there were more Real scientists in the scientific community. There should be no competitors in such a field, but only honest and conscientious people — allies and like-minded people.

Bibliography

[1] S.N. Artekha, **Criticism of the Foundations of the Relativity Theory**, (Moscow: Editorial URSS, 2004, 224 p.), http://www.antidogma.ru .

[2] R.P. Feynman, R.B. Leighton, M. Sands, **The Feynman Lectures on Physics, vol. 1**, (Addison-Wesley Publishing Company, Massachusetts, 1963).

[3] L.D. Landau, E.M. Lifshits, **Quantum mechanics. (Non-relativistic Theory)**, (Pergamon Press, Oxford, 1962).

[4] R.P. Feynman, R.B. Leighton, M. Sands, **The Feynman Lectures on Physics, vol. 3**, (Addison-Wesley Publishing Company, Massachusetts, 1963).

[5] S.N. Artekha, **Criticism of some aspects of the relativity theory**, (Space, Time, Gravitation, Proceedings of IXth International Scientific Conference 7-11 August 2006, "Tessa", Saint-Petersburg, 2007, pp. 7-17), http://www.elibrary-antidogma.narod.ru/bibliography/Arteha2.doc .

Additional references

[6] T.A. Lebedev, **On some controversial issues of modern physics**, (Leningrad Polytechnic Institute, 1955, Part II).

[7] O.H. Derevensky, **“The hocus-pocus of quantum theory”**, http://newfiz.narod.ru/qua-opus.htm .

[8] R.V. F’edorov, **Physics: crisis problems, new beginnings**, (Chernovtsy: Prut, 2005, 400 p.).

[9] Ju.I. Petrov, **Paradoxes of fundamental concepts of physics**, (Moscow: URSS, 2012, 336 p.).

[10] J.J. Smulsky, **The Theory of Interaction**, (Ekaterinburg: Publishing house “Cultural Information Bank”, 2004, 304 p.).

[11] L.A. Shipitsyn, **Hydrodynamic interpretation of electrodynamics and quantum mechanics**, (Moscow: Izdatel’stvo MPI, 1990, 49 p.).

[12] A.L. Shalyapin, Stukalov V.I., **Introduction to Classical Electrodynamics and Atomic Physics**, (Izdatel’stvo UMC UPI, Ekaterinburg, 2006, 490 p.).

[13] B.M. Moiseev, **Physical model of a light quantum**, (Moscow: Book House “LIBROCOM”, 2011, 80 p.).