Sergey N. Artekha

FOUNDATIONS OF PHYSICS (CRITICAL VIEW): ELECTRODYNAMICS

The proposed book is devoted to a systematic analysis of the classical theory of electromagnetic phenomena. Some incorrectnesses for the application of mathematics in theoretical physics are discussed. Many controversial and insufficiently substantiated moments of the classical theory of electromagnetism are analyzed in detail: its foundations, interpretations, mathematical methods and consequences. The book contains an analysis of a number of electrodynamical experiments. All this shows the insufficient validity of the theory of electromagnetic phenomena and the need for serious work on its foundation. Also, the book discusses some alternative ideas related to electromagnetic phenomena.

This book can be useful for students, graduate students, teachers, scientific and technical workers and anyone who is interested in the foundations of physics.

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Preface

There is nothing reprehensible in not following the herd of those going ahead, but moving in the way it should go.
(Seneca)

In our time, so-called formal signs came to the forefront as the main criteria for assessing scientific activity (in the past centuries, it would have been just ridiculous!). In the pursuit of good indices (the number of publications and citations), many scientists forget that science “does not tolerate fuss”. Agree that it would be unpleasant to learn that in the course of a hundred years the theory, to which the whole life was dedicated, would occur false ... That is why the most serious and honest approach is needed to the foundations of science.

One of the main ideas of writing this book is to express explicitly the problems **of principle** that exist in electrodynamics [1]. Of course, in comparison with the theory of relativity, which has nothing to do with reality [2] and has slowed the development of science for a long time, electrodynamics is, in one way or another, a working theory (someone even considers it as a standard of rigor). However, on closer examination, its foundations are clearly unsatisfactory. In addition, in order to move forward, we must acknowledge the current state of affairs and stop “hiding the garbage under the carpet”. Only then physicists will be able to think over existing real problems not undergroundly, fearing the disapproval of the scientific establishment, but openly, and the matter will necessarily move from the dead end.

Of course, not all scientists "became bronzed" in their commitment to the once memorized "truths". Some are even ready to discuss something fundamentally new or to critically analyze the basics. The author is sincerely grateful to Feynman, who tries to make understandable the physics of the phenomenon itself (in contrast to "the mathematical chicanery" of other theorists) and, as a result, reveals many doubtful moments of the theory presented.

In general, the approach of theoretical physics is fraught with disappointments for true scientists; and for functionaries – even a transition to medieval obscurantism (!): they have to artificially declare for something as if to be universal and as if to have a final form. However, as soon as this “general” turns out to be incorrect, all particular consequences also fly off, and everything has to be revised from scratch. The approach of the general physics, like the historical one, has the advantage (in terms of the search for Truth). Experienced facts will always remain so, and at any time you can return to that “fork in the road,” where there were other possibilities for generalizing the data or their interpretation, and change the choice without affecting other (again real) aspects of the phenomena.

Let us recall the difference between “law” and “definition”. The law expresses the interrelationship between physical quantities, each of which can be measured independently of each other and independently of this law. All the rest is some definition of some physical quantity, which is immeasurable in an independent way (remember Poincaré’s statement on this subject). Therefore, a physicist should always understand the degree of generality (status) of a particular expression, equation, statement, principle, law – in order to clearly imagine what can be expected of them and what cannot. Let us take an expressive example: the law of conservation of energy. This is not even a principle, but rather a “Military Charter”:

Paragraph 1. The law of conservation of energy is always fulfilled; on today's date (substitute today's date and year), the expression of energy has the form

$$E = E_1 + E_1 + E_2 + \cdots E_{j-1}\,, \qquad (1)$$

(substitute all the expressions, which are considered correct for this date, into the right part of (1)).

Paragraph 2. If you find the non-conservation of energy by the value of E_j, then find a beautiful name for it, add the expression E_j to the right part of (1) and again read Paragraph 1 from the beginning.

This, of course, is a joke, but “in any joke ... only a part of the joke”. Obviously, the law of conservation of energy can help simplify the solution of the problem only if the causes and mechanisms of the phenomenon under investigation are **already known**. In such a case, this law is simply the first integral of **already known** equations of motion. If the causes of the phenomenon are not reduced to previously known reasons, then the (**now already unknown**) law of conservation of energy cannot help anything. New terms can occur to be added to it: in due time it was, for example, with thermal or electromagnetic phenomena. And do not be afraid to “go over the red flags”: this is the role of the researcher – to check whether these flags are placed along the way or across the road (for 2000 years the physics has “slightly” changed, and in the next 2000 years, too, this may happen).

Do not cost to confuse the **reality** and simplified **models of its description**, and, moreover, to absolutize any theory. It is clear that mathematics enchants with its strictness and consistency, but it is important not to deceive ourselves here. Just need to imagine clearly that our equations, most likely, are not absolutely rigorous and accurate, but only approximate. Therefore, **strictly** mathematical treatment of them, as with identities, **is deceptive**. There are many examples. So, as an example, the differentiation of both parts of the equation (not to mention the explicitly truncated-linearized equations) is often done for the proofs or deductions of the expressions. But the discarded terms (even immeasurably small!) can contain rapidly oscillating terms, which for the derivatives give already comparable or even larger values than from the remaining terms (the function and its derivatives are independent!). As a result, the consequence at some values of parameters may be completely incorrect. Therefore, the operation of differentiation (with respect to spatial and temporal coordinates) in proofs can only be applied to explicit **mathematical** identities. The same remark applies to examples of the use of approximate distribution functions and equations for them. In addition, for the fluctuating quantities, the so-called "quadratic oscillation effect" (quadratic terms from the product of the discarded harmonics) is obviously lost, not to mention the special case of the presence of "intermittency" in the system, when, for each moment (mean, dispersion, kurtosis, etc.), their own, practically independent realization of the process respond to. We also recall the concept of asymptotic paradoxes, which Birkhoff introduced [3]: the presence of arbitrarily small high-order terms in the system of differential equations can completely change the character of the solutions. So G. Birkhoff underlines: it is not always true that when the coefficient at some term of the equation tends to zero, then the solution of this equation tends to the solution of the equation obtained by dropping the term with this coefficient. Thus, mathematical calculations in physics in themselves are not a "pass to paradise" (they do not guarantee the truth of physical ideas).

The theory of electromagnetism (including the carrying out of all key experiments) was created long before the emergence of the special theory of relativity (STR) and did not have any necessity in the theory of relativity (STR) (and even now can work without it). Therefore, it is ridiculous to portray the matter as if STR is the "top of electromagnetism" and to expound the essence of the matter in reverse historical order. This is just a mockery of scientists who have honestly investigated real phenomena, and not mental combinations of symbols of the given false theory (see criticism of STR: [2], http://www.antidogma.ru). In determining our attitude to any false theory, it would be worth remembering the statement of L. Tolstoy: “The exposed lie is just as important an acquisition for the good of mankind as a clearly expressed truth”.

This book sets the following goal: to give a sufficiently detailed critical review of the state for the modern generally accepted theory of electricity. At the same time, criticism of internal contradictions, inaccuracies and arbitrariness of the electrodynamics itself (that is, its apparatus and fundamental theoretical basis), criticism of the modern interpretation of the generally accepted basic electrodynamical experiments (workable devices) will be presented, and some (not universally recognized) experiments will be discussed that contradict modern electrodynamical views. The appendix contains brief comments on some less common alternative theories. The author does not state his proposals for a change in the theory of electrical phenomena, since he believe that such works should be published in peer-reviewed journals, but a number of constructive ideas are scattered in the form of remarks throughout the book.

This book is aimed at physicists, first of all, specialists in the relevant fields, and is built on the basis of a sequence of critical remarks to the best-known (the best) training courses with pointing the relevant pages. And, this is not a claim to specific textbooks (it is just necessary to rely on something); the same moments (ideas, techniques and methods) could be traced through other textbooks and books. The author apologizes, but, unfortunately, a detailed citation of paragraphs, formulas, drawings, etc. from the criticized textbooks would make this book simply "unbearable" neither in format, nor for its publication. Therefore, although in many cases the essence of the issues under discussion is understandable, in some cases it is desirable to have the public textbooks at hand (links in the book are usually given to the beginning of the paragraph under discussion). The author did not set himself the goal of "thoroughly chewing" all the existing problems, but only to briefly point out the researchers' attention to the numerous inconsistencies, gaps and contradictions in the sections of physics under discussion (some of the key words and phrases are marked by exclamation mark or in bold type, including quotations). The book, in general, adopted a fairly diplomatic form of doubt in the validity of existing theories. If only people would pay attention to such signal phrases and think independently over the arising questions. Then only there will be a hope that situation will move from the dead point. The book can also be viewed as a Program of necessary additional explanations, changes and studies for the theory of electromagnetic phenomena. So, let us start analyzing the theory of electricity – to the good way of cognition!

Introduction

My aim is to tell the truth,
but not to force to believe in it.
(J.-J. Rousseau)

We make a general remark. Generally speaking, the description of phenomena by deductive methods of theoretical physics is, in some way, imperfect and suits rather for memorization by students than as a scientific method: it is necessary to know the result in advance to derive it from the "general principles" (but any discovery of new effects immediately leads to a change in some of these most "general" principles, which is not discussed in advance and is not foreseen).

We turn to the discussion of a huge section of physics that studies electrical and magnetic phenomena, a section that many scientists seem to be a model of rigor and validity, and the achievements of which are fairly well known and indisputable. Undoubtedly, this is a working theory and the overwhelming majority of experiments, ideas, methods and developments will remain in the treasury of science. But here, too, everything is not so brilliant, since in this section of physics there are quite a lot of controversial points and “punctures” that physicists should know about. Then, perhaps, solutions and justifications can be found faster.

In this section, we begin with preliminary remarks. The introduction of the term “positive and negative charges” is **not more than one way of describing** attraction and repulsion. However, such a method always raises the question: how do these pieces keep together and not fly apart? Hence, there is a need to introduce new forces that hold the same-named charges in compact objects, or, even worse, to artificially appeal to point objects, leading to infinite quantities for a number of physical characteristics.

The ideology of the field suggests the impossible: the introduction of a test charge does not affect anything at all (neither charges, nor bodies, nor their movements). How, for example, to keep the substance from polarization? Generally speaking, the ideology of the field encounters difficulties already for the number of bodies greater than or equal to two!

The idea of the equivalence of nuclear forces [5, Chapter 8, Section 4] between p-p, p-n and n-n is most likely strictly incorrect, since then there would be more compact and heavier isotopes (with a large number of neutrons), but in fact there exists a limited number of isotopes for each element.

The very idea of the Berkeley physics course “to link the exposition to STR and quantum mechanics” is alarming: how can the experimental subject (eternal Nature) depend on the purely theoretical conclusions (twists) of a particular present-day time?

On the example of our only Universe, we everywhere observe an asymmetry between matter and antimatter (not yet understood theoretically). Perhaps a weak asymmetry exists at a deeper level. Therefore, we should not strictly postulate the total charge symmetry of particles and antiparticles [6, Chapter 1, Section 1.2] (there may well be a vanishingly small difference in charges and not one “elementary charge”, but several quantities, or, in general, none). For example, there are hypotheses that light has a small charge (so far such a small value is not detectable experimentally). To determine the magnitude of the charge, there is only one way – the "force method". Therefore, in the statement about the **conservation of the charge**, it must be indicated that **all charges should** (“symmetrically”) **be at rest**, since the electromagnetic force depends on the velocity of the charge (and it is necessary to determine experimentally, to what this dependence should be attributed – may be, to the charge?). The structure and properties of charged particles (the very existence of a charge) have not yet been explained either by classical or quantum mechanics. So, one should not artificially raise any one of these theories, while the second one should be belittled, but it is necessary to weigh all the pros and cons of the given theories without emotions.

In the textbook [7], many questions are “turned inside out”. In reality, the rotation of the disc obviously proves the contradictoriness of the STR (since the shortening of lengths is attributed to the kinematic properties of space), and not the necessity of “complex deformations”; an absolutely rigid body in the classics only means that one can distract from its small deformations by describing a particular phenomenon; nature does not limit the rate of transmission of interactions (and with the speed c only electromagnetic interactions occur!); and the point-likeness (or punctiformity) of elementary particles – is the obvious (and experimentally long refuted) stupidity of the STR, but not the requirement of Nature.

Let us now turn to the sequential analysis of this section of physics (the theory of electromagnetic phenomena).

Chapter 1
Electrostatics

Let us start with the simplest – with electrostatics. Unfortunately, neither the ideology of the field nor the ideology of the potential help to solve even electrostatic problems visually for the case of more than one body (the equations remain equations for the formal solution).

Feynman's statement that "electrostatics is the law of Gauss plus ..." can mean only one thing: the equations of electrostatics are underdetermined (not self-sufficient).

The statement about the impossibility of equilibrium in the electrostatic field is a statement purely for the model problem, since real electrons and protons have also the magnetic moments besides the charges (there is no pure electrostatics for particles with spin). And why should the equilibrium be static (at absolute zero temperature, or what?), but not dynamic? After all, a solid body is held by electromagnetic forces, and we can use it for fixation. And, besides, the very fact of the existence of ionic crystals contradicts this statement.

The rigor of the equations of electrostatics in a vacuum is exactly equal to the rigor of the single law – the Coulomb law in a vacuum, and the presentation must begin, of course, with the Coulomb law in a vacuum. However, before making any abstract generalizations to the case of the presence of matter, it is necessary to determine what we mean by the field (the abstract introduction of the quantity **D** immediately denies the generality of the equations). The only possibility is to go, as in the case of vacuum, using a force approach. Then, under the force acting on the charge in the dielectric, if the latter is not liquid and not gaseous (when there are no problems), it is necessary to understand the force acting on the charge in a cavity (which is slightly larger than charge size), which is cut out in the given dielectric. But then it is better (and more correctly) to proceed from the law of force

$$F = \frac{q_1 q_2}{\varepsilon(r, X, Y, Z) r^2},$$

where in the general case the function ε depends also on the geometric characteristics of the sample. From here one can obtain differential generalizations for the most used particular cases (and only such an approach will be without deception). In the real physical situation (and not for model mathematical problems) the transition from the experimentally verifiable concrete Coulomb law to partial differential equations is somewhat questionable, since **additional** conditions are required (initial conditions in the transition to electrodynamics, boundary conditions, gauge conditions), which are not determined from the first principles (for example, the gauge conditions). In addition, for example, the real boundary conditions always fluctuate, but, in fact, the practical count starts “from the end” (and for different problems, the decrease at infinity is different in mathematical solutions, but indistinguishable in practice). In advance, modulate the solution by some symmetry? However it is possible to calculate this practically only for some particular cases (but not for arbitrary geometry), and for them the symmetry condition – is our mental concept of this process only (one more additional condition), which we would also like to check independently.

For electrostatics, a number of concepts (for example, potential) can be introduced strictly mathematically [6, Chapter 2], but in general case this is not quite so. When the spherical charged shell is narrowing, one can strictly (mathematically) introduce a function for the field energy: in this particular case, the field simply does not change in the remaining part of the space. However in the general case there are “explanatory” difficulties: if we formed two charged systems from a neutral system, then it is obvious that some energy has been expended **at once** for this (there arose a new nonzero potential energy of the given configuration), but after all the field will start to be installed **in all space**, and it will be **gradually**. But what we can say about the equality of the potential energy and the field energy throughout the space? After the reverse reunification (neutralization), too, remote (infinitely) regions could influence only after a long period of time, but by that time the system will be neutral and will not be able to interact with the field at all.

From the absolutely strict viewpoint, the differential form of the Gaussian law is derived for a **fixed** configuration, and the fact that it can be derived for any volume does not ensure the equality of the integrands from the equality of the integrals themselves! Why do we have to consider the substance as “amoeboid”: how many do not add, nothing in it does not change (qualitatively)? Why is it necessary **to postulate** that the system as a whole “does not feel” its own boundaries? It may be periodic additions, depending on the properties of the system as a whole (in particular, on its dimensions), and which for each new volume will be NEW. For example, this may correspond to some natural oscillations of the system (such as standing waves) or to reflect the static periodicity of the structure. Thus, the differential form is less rigorous than the experimental Coulomb law. A similar remark can be made to the Stokes theorem.

Thus, even an initial look at such a fairly simple section as electrostatics already raises some questions.

Chapter 2
Dielectrics

Let us now turn to dielectrics. The equality of the magnitude of the field in the slits in the dielectric, either **E** or **D**, as a function of the orientation of the gap, was not established experimentally (since we are dealing here not with laws but with definitions); this equality is only declared on the basis of belief in the equations of electrostatics [5, Chapter 11, Section 4]. Many tasks (even for ordinary water) were “solved” after peeping in response (in what was the predictive power of the theory?). An analogy with a liquid [5, Chapter 12, Section 5], if we follow the historical path, was inverse – the equations of electrodynamics (and, consequently, their solutions) were introduced by analogy with the behavior of an ideal fluid.

The fact that the assertion "the field potential in a dielectric is equal to the sum of the potentials" [8, Chapter 2, Section 21] – is a hypothesis (since the interaction potential is also possible), not to mention the fact that the exact behavior of the potential of bound charges at small distances is also unknown. The assertion that bound charges in a dielectric with uniform polarization are concentrated on the surface are purely formal (mathematical), since it depends on the accuracy of measuring these quantities and the dimensions, over which we average in reality (more precisely, the device itself averages). The statement that, in the absence of external fields, the polarization is zero, also depends on the accuracy of the measurements and the averaging scales: simply in pyro- and piezoelectricity, these phenomena are clearly detected in macro scales and drop out of general assumptions (puncture), so they cannot be ignored. The proportionality of the polarization to the magnitude of the field also depends on the scales (the sizes of the short-range and long-range order).

Since the acting force is the only real measurable quantity, then the introduction of any additional quantities (for example, the electric induction **D** in dielectrics) is simply a mathematical definition of some new quantity, and it is unlikely that the behavior of such the functions and their graphical representation are of independent interest for physics [8, Chapter 2, Section 22]. The dependence of **P** on **E** cannot be determined in a general form (and it can turn out to be more complicated than the tensor dependence). "Complete system of equations of electrostatic field" [8, Chapter 2, Section 22]

$$\mathbf{E} = -\mathrm{grad}\,\varphi, \qquad \mathbf{D} = \varepsilon\mathbf{E},$$

$$\mathrm{div}\,\mathbf{D} = 4\pi\varrho, \qquad D_{2n} - D_{1n} = 4\pi\sigma$$

is very limited: the properties of dielectrics and the distribution of charges are given not self-consistently and arbitrarily in this system, but fields (forces) will be those that are obtained (calculated). Even from a mathematical point of view, the system is correct only with the continuity of the potential and with a certain behavior of the field at infinity, while in physical reality, always some forces will exist under more extensive conditions (unambiguity of the system is also proved under certain limiting assumptions). The system does not describe dielectrics and field in the presence of ferro- and pyroelectricity (and in fact, ideally the description of a single phenomenon should be unified).

Can a homogeneous dielectric exist in Nature ($\varepsilon = const$) [8, Chapter 2, Section 23], which preserves its properties for an inhomogeneous field **E** (**R**) is a matter of experience. And at the same time it would be worthwhile to clarify how it is experimentally supposed to measure this very ε: if through the Coulomb law (and, apparently, this is the only method), then it is through Coulomb's law also that we must introduce the definition of ε (and generalize it to the case of arbitrary inhomogeneous and nonlinear media). The convenience of working with mathematical symbols for physics is a minor matter (assuming, of course, the practical needs of real experiments, and not the desire to have several purely “academic” mathematical solutions without reference to reality). Generally speaking, the introduction of the coefficient ε characterizing the medium assumes that all charges, including the test charge, are **immersed** in this **medium**, and its dimensions are much larger than the distance between the most distant charges. Then we can introduce the definition of ε in terms of the Coulomb law. If this is not so, then we do not have a medium, but a body and we must additionally take into account its geometric characteristics and the relative position with charges. The ideology of “immersion” in a dielectric suggests that the gradient of the charge field when immersed in a dielectric varies insignificantly over distances on the order of the distance between those dielectric molecules that “separated” a certain charge introduced between these molecules (otherwise additional polarization charges must be taken into account).

Calculation of the field of a point charge in a dielectric [8, Chapter 2, Section 23] is carried out approximately, since it presupposes the possibility of homogeneous polarization in an inhomogeneous field, which is not obvious (the possibility of the appearance of an inhomogeneous charge density $N(\mathbf{r})$ is not taken into account).

The replacement of the intermolecular field, which varies considerably in amplitude (and most considerable for neighboring molecules), by some “slowly” varying mean (weaker) field [8, Chapter 2, Section 28] is a plausible hypothesis that acts accidentally for some substances and also does not act accidentally for other substances (therefore, the generalization of the Lorenz - Lorentz formula for a dielectric with dipoles is often incorrect – not confirmed experimentally [8, Chapter 3, Section 29]). Generally speaking, the meaning of all this theorizing could be only when from the first principles (but not from the answer peeped from the experiment) we can establish the numerical values of the coefficients. But this in the textbooks (and in theory) is not yet!

It is strange that in reality a dielectric (solid or liquid in a shell), placed between charged plates, increases (!) the force of attraction between them, but does not reduce, as it would seem, should be, if we recall the Coulomb law and the concept of ε. However if the dielectric is liquid and the plates are completely placed in the dielectric, then the attraction force decreases! Why has the meaning of the variable ε changed (see [9])? Apparently, this is the question of whether the superposition principle operates only in a vacuum, or in a medium, too (or does the screening of the field with atoms and molecules play a role, in addition to superposition of fields)? The point is that the motions (even virtual ones) and measurements can be carried out only **between** dipole molecules, and in this case it can be seen from Fig. 1 that the fields have increased (and forces!):

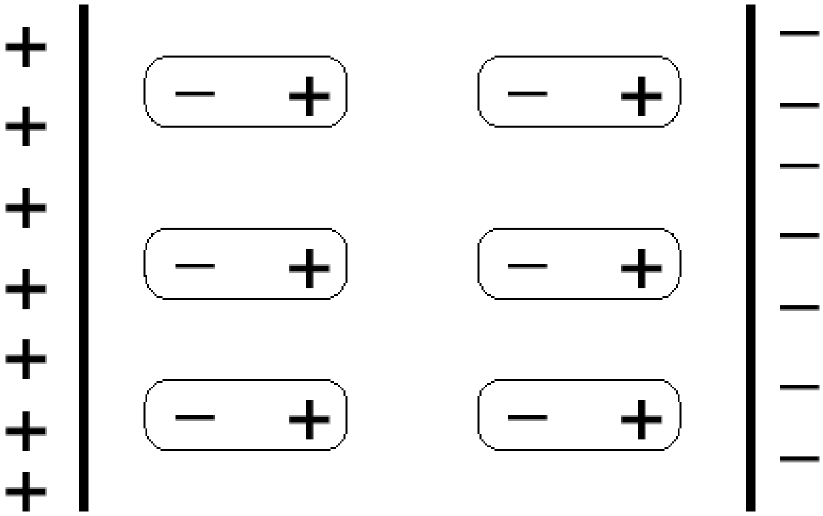

Fig. 1: Field between dipoles.

since the shortest distances will be up to the charges of the opposite sign (and if we estimate by the potential, then too - instead of the zero gradient a gradient of the required sign that increases the field was added). But in the case of a **complete** immersion in a dielectric, it would seem that in the case of pure superposition, the forces should not have changed **at all** (or had to change very little, since the distance to each other from the dielectrics **behind** the plates is relatively large). Therefore, we must take into account the **screening**! Only in this case ε can be considered as a constant (and no noticeable **new** dependence on distance will appear). Similarly, the question of the effect of substances on the magnetic field is of interest (why there are dia- and paramagnets, and, according to the equations used, the analogy of ferromagnetism and residual polarization would seem to be more complete).

The concept of dielectric permittivity in [6, Chapter 9, Section 9.1] is introduced using a capacitor capacitance and assumes **complete microscopic homogeneity of the entire medium** (including the plates themselves!) and neglect of edge effects (this entails methodological limitations). In fact, strictly speaking, any of the methods is the introduction of some **effective** ε for a given configuration (and the transition to $\varepsilon = \varepsilon(x, yz, t) = const$ is only **purely mathematically**). The question naturally arises, which of the quantities retain their theoretical and experimental meaning in the more general case? And the transition from such **introduced artificially jelly** $\varepsilon = const$ to the actual microscopic structure of matter is far from obvious (rather, simply assigning to the values of the required numerical value occurs here every time), that is, we have not a strict but approximate description of the phenomena (each time with the necessary “plausible spells”).

The multipole description [6, Chapter 9, Section 9.2] is **approximately true** only for large distances from the system of charges. Even for the dipole, which theorists are constantly using: the “real” dipole

$$q \to \infty, \qquad r \to 0, \qquad qr = const$$

cannot be realized in Nature. The assumption of the constancy of the distances between the charges of the dipole is inaccurate. Generally speaking, quantum mechanics is a refutation of electrodynamics in microscopic scales, and to mention it for “plausible spells” in favor of electrodynamics is not correct (of course, there remain also practical questions for electrodynamics: can each atom be considered as a static distribution of a spherically symmetric electron cloud around the nucleus with **p**=0, etc.).

When in [6, Chapter 9, Section 9.8] ones “collect” the polarization density with N molecular dipoles, then there is a share of cunning in this. Molecules are in disorderly translational, rotational and vibrational movements, there is space between molecules, etc. Therefore, one can only affirm “in fact” that some **effective** polarization is established, depending on the **geometry** of the experiment (but its connection with microscopic quantities remains hidden); and the equating of the polarized substance field and the field of two charged layers – is just some approximation.

All charges in atoms and molecules are in motion. And taking into account the **conservation of the orbital and spin moments**, purely from the point of view of conventional electrodynamics, it can hardly be asserted that inside the substance $\mathrm{rot}\,\mathbf{E} = 0$. Rather, it may be either $\langle \mathrm{rot}\,\mathbf{E} \rangle = 0$, or $\langle \mathrm{rot}\,\mathbf{E} \rangle = const$ and practically does not change in static fields (and does not affect on this experience). Therefore, the expression $\langle \mathbf{E} \rangle = -4\pi\mathbf{P}$ – is simply the **definition** of effective polarization, as well as the expressions [6, Chapter 9, Section 9.9]

$P = kE$ and $\varepsilon = 1 + 4\pi k$ – are just the **definitions** of some ε and k values (recall the general difference in "status" between an equation from which something can be obtained and a definition from which nothing can be obtained, since it itself introduces some new value).

From the expression of Coulomb's law in matter (in liquid oil) it follows [6, Chapter 9, Section 9.12] that the force (or field) is weakened by a factor of ε. The phrase that the charge inside any sphere is less than Q is not quite obvious (depends on the structure of molecules): if the sphere contains all "**whole**" molecules (the total charge of each molecule is identically zero), then the total value will be exactly equal to Q. It is not obvious that the force must always decrease: if in a chain

$$\ldots \rightarrow q \rightarrow\rightarrow Q \leftarrow\leftarrow \ldots$$

we put test charge q, then the strongest influence for him should be from the nearest neighbors, and it should strengthen the field (at least, this also can eventually be).

Further, the author try to apply the description with minimal changes to the case $\varepsilon = const$. For example, it is the choice

$$\mathrm{div}(\varepsilon\mathbf{E}) = 4\pi\varrho_{\mathrm{free}}$$

and an arbitrary choice of the description

$$\mathrm{div}\mathbf{E} = 4\pi(\varrho_{\mathrm{free}} + \varrho_{\mathrm{bound}}).$$

However all this description is applicable **only** in the case of a full immersion of the charge and a test charge in a (liquid!) dielectric. A transition to an arbitrary functional dependence $\varepsilon(\mathbf{r}, t)$ and arbitrary geometry is not feasible in the general case.

The introduction of $\mathbf{D}$ – is just another new definition. In fact, in order to avoid "tricky" questions about piezo- and ferroelectrics, the very notion of a dielectric is introduced as “a substance in which $\mathbf{P}\sim\mathbf{E}$” [6, Chapter 9, 9.12], but we can distinguish between substances on this basis only post factum.

Determination of the field energy density as εE^2 [6, Chapter 9, Section 9.14] has the same restrictions: $\varepsilon = const$ everywhere! In most cases, the mechanisms of polarization of a particular new substance are unknown in advance (even for ice, as is recognized in [6, Chapter 9, Section 9.17]), and it is difficult to divide the total current into a current of free charges and a current of bound charges (there exist ambiguities: there are two quantities, but the result is one), so the predictive power of the theory for new substances is small.

The introduction instead of $\mathbf{E}$ of a new vector $\mathbf{D}$ (experimentally undetectable without additional interpretations) and the definition of the Gauss theorem for dielectrics in [10, Chapter 1, Section 13] – is simply a postulate: you might think that one scalar equation can help define (find) two vector quantities (six variables).

The textbook [10] (good enough in terms of material and a set of tasks) is hardly intended to reflect on fundamental issues, but rather, only for students to remember the plausible way of obtaining the given (known) fixed results. It does not allow to trace those changes that may occur if some provisions adopted on faith, would occur inaccurate. A number of calculations are also not mathematical rigor.

As a rule, many phenomena are interrelated and an artificial attempt to choose the path of transition to the final state by fixing certain parameters is deliberately nonstrict (for example, not every such process will be reversible), and Nature chooses not an arbitrary path of transition, but the only process. Also, it is not rigorous the use of an "ideal" medium as a dielectric – an incompressible fluid with a constant dielectric permittivity (which does not depend on anything).

In describing the polarization of polar dielectrics [10, Chapter 1, Section 36], the estimate of the weakness of the induced moment is made for the average Maxwellian field **E**, but the microscopic field during ordering can differ substantially, and it is necessary to estimate the influence during the times of action of such an amplified field (this is rather related to an adequate **interpretation** of the quantitative value of the environmental parameters, since just such the information is extracted from here).

There is as yet no complete theory of ferroelectrics [10, Chapter 1, Section 39]. The qualitative remark on the following solution hardly makes much sense:

$$(1 - N\alpha\beta)\mathbf{P} = N\beta\mathbf{E}\,,$$

where α is the constant, β is the polarizability of the molecule, N is the number of molecules per unit volume, since the “solution” $(1 - N\alpha\beta) = 0$ is just one “point” for $\mathbf{E} \equiv 0$ (but if $\mathbf{E} \neq 0$, then the polarizability is infinite!). With allowance for fluctuations, $\mathbf{E} \neq 0$ always. The Ginzburg theory (based on the Landau theory) **assumes** that we have a second-order phase transition, that it can be described with the help of a thermodynamic potential decomposed at the transition point in powers of polarization (this again implies the continuity of **P**). Such “theories” do not investigate the **physical** mechanisms of the phenomenon at all, but simply artificially “select” the mathematical equations with solutions **resembling** the previously measured dependence.

In the derivation of expressions for the electrostatic field in dielectrics in [11, Chapter 2, Section 6], the value of **P** is artificially introduced as $\langle\varrho\rangle = -\mathrm{div}\mathbf{P}$. First, for an artificially introduced quantity, the condition **P** = 0 outside the body (and on the boundary!) – is an additional condition. Secondly, the quantity **P** is an ambiguous quantity, since it is “secreted” from the equality of the integral to zero (and there are many such methods of isolation). Thirdly, again the equality $\int\langle\varrho\rangle dV = 0$ for bodies of any form does not mean the independence of $\langle\varrho\rangle$ and **P** on the form! This is another additional hypothesis. It is also incomprehensible for this definition, what is the preference in it for dielectrics in relation to metals (why cannot such an artificial function **P** be introduced for the latters?).

In deriving thermodynamic relations for dielectrics in an electric field [11, Chapter 2, Section 10], one represents this field as created by charges on conductors outside the dielectric. It is assumed in this that since only the field inside the dielectric enters the final expression, then the expression does not depend on the origin of the field. However, this requires proofs: since for the same field inside the dielectric, but for different configurations of conductors, **changes of the field** in the surrounding space due to the **presence of the dielectric** may be different (also depending on this there may be different volume of the dielectric, the distribution of the temperature inside it and other parameters). The use of thermodynamic relationships **already requires** knowledge of all the specific properties of the body (and material), that is, their **preliminary** study (measurement), and again there arise the question of the predictive possibilities of the theory (let us recall, for example, electrostriction). This problem is especially acute for crystals [11, Chapter 2, Section 13] (including pyroelectric, ferroelectric and piezoelectric crystals): too much preliminary knowledge is required for the subsequent use of the theory.

In the derivation of electrical forces in a liquid dielectric [11, Chapter 2, Section 15] it is not specified – the dielectric is limited or unlimited; this is important, since in the case of a limited dielectric, the influence of its shape can be manifested (supported, for example, by forces from outside). The rejection of accounting possible dependencies on the temperature gradients is also some approximation. It is not proved the necessity to assume that the potential of the conductor remains constant with the virtual displacements, and the deformation of the dielectric is isothermal. Also, without proof, it is assumed that each particle of matter moves along with its value of potential (whether this is a “frozen-in” property of a substance?).

The “significance” of the formula (15.11) from [11, Chapter 2, Section 15] is not entirely clear:

$$P_0(\varrho, T) - P_{\mathrm{atm}} = \frac{\varrho E^2}{8\pi}\left(\frac{\partial \varepsilon}{\partial \varrho}\right)_T - \frac{\varepsilon - 1}{8\pi}(\varepsilon E_n^2 + E_t^2).$$

It is asserted that this equation determines the “density of a fluid near its surface by the intensity of the field in it.” However in fact it turns out that one must know the distributions of T, P_0, ε, $\mathbf{E}$ for determining the density ϱ: but is not easier to measure the value of ϱ itself? If, however, you cannot measure all this in an independent way, how can you verify the correctness of this formula? Thus, all expressions, including the magnitude of the volume force **f** in this section, are approximate (approximately true under certain conditions).

Thus, the theory of dielectrics, which is well known and familiar to all, upon closer examination causes some dissatisfaction and does not at all seem to be the only possible, general, algorithmic and strictly justified.

Chapter 3
Electric current

Let us now turn to the concept of electric current and the analysis of the phenomena connected with this. Let us start with the fields in metals.

Generally speaking, the proof of the absence of a field inside the cavity [5, Chapter 5, Section 10] – is not strict, since we do not take into account that the metal is held in some way as the whole (or are these external forces?). Otherwise, you can draw another drawing in the same way (Fig.2)

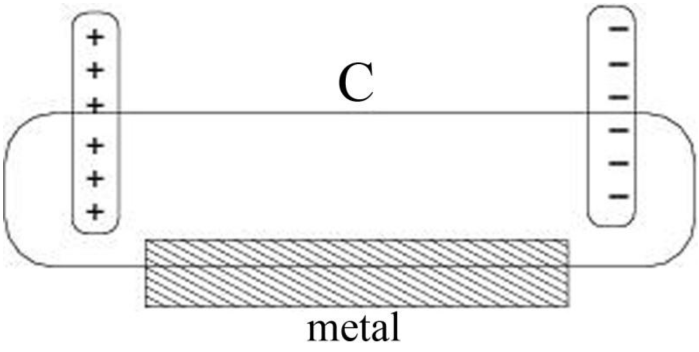

Fig. 2: The field inside the cavity.

In this circuit, it turns out that rot $\mathbf{E} \neq 0$, well, what follows from this? Here, just as in electrostatics in the presence of solid bodies (fixators), anything can be possible.

In fact, Ohm's law is the definition of some unknown quantity R, which depends on the material of the object, on its geometric characteristics (if to be exact – this is not so simple), on temperature, pressure, influence of external and internal fields etc. And that fact that identical formulas [8, Chapter 3, Section 35] of the law of Joule

$$Q = RJ^2 = J(\varphi_1 - \varphi_2) = J \int_1^2 E_s ds$$

have a different range of applicability, speaks about the approximate nature of the introduced concepts (averaged). Similarly, the expression for the current density **j** = λ**E** is the definition of the quantity λ. Although it is called the differential form of Ohm's law, it hardly has the status of a law (and it is derived under certain assumptions). Generally speaking, from the experimental macroscopic equations one cannot unambiguously obtain “micro equations” (exact) – always this will be only one of the possible variants. Again, declaratively declares a various degree of generality of the equations obtained in exactly the same way (i.e., with one and the same degree of generality) for the value of the specific power of current:

$$q = j^2/\lambda = \lambda E^2 = \mathbf{jE}$$

(the greatest generality of the first equation is declared).

For the condition of stationarity of currents, for the continuity equation and for current threads [8, Chapter 3, Section 37] it is assumed in advance that:

1. currents are neutral;

2. there are no “mechanically” fixed charges;

3. the effect of boundaries and material (quasi-elastic effect) is not explicitly taken into account.

Otherwise, not every change in the field or redistribution of charges causes a change in current; currents can be “semi-closed” – with the birth and destruction of charged “jets”. The postulate of the “conservation of electricity” – is a certain choice of description (the choices may be various, if only “benchmark predictions” coincide with the experimentally observed phenomena, and the intermediate “unobservable entities” can be arbitrary at this). The proof in the case of constant currents that the density of free charges is zero, is incorrect: and if, for example, we initially take non-neutral currents (beams)?

In occasion of external EMF [8, Chapter 3, Section 38], the description of electrical phenomena and properties should be uniform:

1) we recall about the superconductivity – in this case there is a current, there is no external EMF, the resistance is zero;

2) since it is believed that the magnetic field is caused by currents, the existence of permanent magnets without energy lost proves that it is not necessary to have an EMF for the current;

3) and atoms themselves also confirm that the presence of EMF is not necessary. In general, clearer definitions are needed for non-neutral currents. Adding $\mathbf{E}_{ext}$ to $\mathbf{E}$ raises the question: which field is actually measured in this area? Or are both these quantities – auxiliary, only for the convenience of the theory?

In [8, Chapter 3, Section 40] for the explanation of Tolman's experiments, ones use the general equation of alternating currents, that is, all “plausible” previous arguments are quite not checked. It is assumed that at the final moment both the current and the acceleration are zero, but this is strictly not true – since there will be a delay (relaxation in time). Further, the resistance can be a function of the process $R(\omega)$; and the inertiality of the galvanometer can influence the measurement of the variable

$\int J dt$, and, in addition, one must take into account the influence of the method of connecting the coil with a fixed galvanometer (through sliding contacts), that is, the result is obvious and not quantitative, but only qualitative.

An interesting question: the field **E** practically does not propagate (it damps very quickly) in dielectrics (in air, for example) and in a vacuum (disconnected electric iron does not heat), but the field extends over considerable distances in the metal. What is the mechanism of maintaining the field in metals?

Before expecting from the classical theory of electrical conductivity to quantitatively coincide with all experiments [8, Chapter 3, Section 41], and before "felling" it, it would be necessary to take into account all the factors:

1) the electrons do not move strictly along the field (since there is also a lattice field and field **B**);

2) the scattering does not occur "by the will of the case", but according to certain angular distributions;

3) the starting values of $\mathbf{v}_e$ are not zero;

4) the values of $\mathbf{v}_i$ in collisions are thermal (on average);

5) the average kinetic energy of electrons in the given cases should be the calculated value;

6) the introduction of an unknown "letter" l – the mean free path – does not add anything new;

7) with a specific heat – it is not quite obvious that for metals and dielectrics there must be differences, because in both cases electrons are outside the atomic nuclei **fixed** at the nodes of the crystal lattice, that is, the transfer of heat in both cases occurs

with the help of electrons. That difference that in a metal electrons are considered to be free, can play a role only for the **rate** of heat transfer, that is, for heat conductivity. Therefore, to estimate the value of the electron concentration by the difference in specific heat – is too crude;

8) in the quantum case for the mean free path, too, we have a value much greater than the average distance between atoms;

9) the dependence of λ on T in the classical case is not explicitly extracted in order to be able to criticize something;

10) the fields at small distances are unknown, and, therefore, the exact interaction of electrons with the lattice is unknown, but only then it would be possible to construct a rigorous classical theory;

11) the conductivity of metals should also be expressed in a single relation (including superconductivity), rather than *ad hoc* hypotheses.

From the unobservable expression $\mathbf{j} = \sigma\mathbf{E}$ (this is not a law, but a definition of the value of σ), we could obtain the experimentally observed expression $I = U/R$, but the reverse transition is not the only possible one.

If we believe in the free-electron conduction mechanism for Ohm's law and in friction that limits the growth of the electron velocity, then a wide range for fields in which Ohm's law is valid (that is, σ is a constant and the connection is linear) is very suspicious. The ideology with collisions is also quite paradoxical, because any atom is rather a “**void**” and it would be easier for an electron to fly through it than with anything collide (that is, contrary to the textbook, nothing “fatal” here is not!). But this is a common problem of modern theories of the structure of matter.

Therefore, the **non-ohmicity** of the contact of two metals is strange.

Recall that the speed of the ordered motion of electrons - is millimeters per second. Therefore, it smacks of forgery, when in the beginning [6, Chapter 4, Section 4.6], the characteristic time τ is estimated from the **conductivity** of the metal in the field, but then the **thermal** velocity (but this is hundreds of kilometers per second) is substituted to estimate the path of the electron in the crystal. And as a result we get the value $\sim 30\text{Å}$ – too much! Naturally, because to these values of τ not thermal speed should be related, but the speed of directional movement (then the path would have turned out much less). Strange with the generally accepted choice is also an increase in the conductivity of metals with a decrease in temperature. But for the “drift model” (in the resulting fields $[\mathbf{E}\times\mathbf{B}]$), the situation could be qualitatively better! An experimental question also arises: what is the share in the emerging magnetic field on the wire surface from the ordered spins of electrons?

Penetration of sodium ions through a filament lamp (!) in the electrolysis of $NaNO_3$ raises some questions [10, Chapter 6, Section 93]. Why do Na ions penetrate through the unmelted glass, but the lighter electrons do not? Why is Na not distributed internally over the entire glass flask, but is on top above the solution? Why is it not inside the glass itself after electrolysis and neutralization stop? Strange it seems the statement that all the energy of the current goes only to heating the electrolyte (Joule heat), and nothing goes to destroy the substance. It turns out that if the resulting products (for example, H_2 and O_2) can later enter into an isothermal reaction, can additional energy be obtained?

We see the Example 2 [10, Chapter 4, Section 84]. The Joule heat in the wire with current is "declared" to be the result of the influx of electromagnetic energy from the space surrounding the conductor. And where is it released? **Throughout the volume of**

the wire in the process of flowing energy in it, isn't it? But then, if you put a piece of wire (without current!) **near** to the wire, or, even better, to pass a wire with a current inside a piece of metal pipe, then **H** will be the same, **E** may be near the same. That is, the flow of energy passing through the sections of this **segment** of the wire (or pipe) will be comparable with the flow through the analogous section of the wire of the closed circuit, and the Joule heating must be close (the energy is either absorbed or not absorbed!). However, it is not! If we assume that the energy “flows all the way to the center of the wire" and there heat is released, then the resistance of the hollow tubes would be almost zero, which is also not the case. So with the treatment of "effects" from the Umov - Poynting vector, not everything is "smooth".

And in the following example 3, the concept of bias current (that was previously simply discarded) is already used for the capacitor. In addition, the Umov - Poynting vector is ambiguous. Sivukhin criticizes classical physics and praises the theory of relativity, but then “it turns out” that the electromagnetic impulse was introduced by Max Abraham before the emergence of the theory of relativity! Then the criticism looks absolutely unfounded (which concepts did not suffice before?). Theorists like to give examples (expressions of faith) that have never (!) been tested in an experiment. For example, when a cylindrical capacitor is discharged in an axial magnetic field, rotation can actually occur, but **specific mechanisms** for changing the state of motion of charged particles **at the time of changing external conditions** will be involved. And such an experience (even if it was carried out) would not prove the existence of a constantly "rotating" energy flow (where, for example, did the Joule heating of the **motionless** capacitor disappear?).

In the modern method, the calculation of the value of EMF of electromagnetic induction value assumes a high conductivity of the metal, but in practice, the EMF does not depend quantitatively on the conductivity, so there must be

$$\text{grad}\,\varphi \equiv -\frac{\partial \mathbf{A}}{\partial t}$$

(that is, **E** ≡ 0!), but then there is no reason for polarization (Z.I. Doktorovich 1994)!

The expression $\text{div}\,\mathbf{j} = 0$ for a direct current [11, Chapter 3, Section 21] reflects only our assumption about the impossibility of individual or separated generation of charges (but only – by local pairs). The relation $\mathbf{j} = \sigma\mathbf{E}$ also adds a new unknown quantity of σ and merely reflects our desire to choose the linear dependence that is most simple for calculations. The choice of $\sigma = const$ means that we have an infinite medium of one and the same type. The choice of $j_{n1} = j_{n2}$ on the boundary is justified by nothing. The possibility of boundedness of conductors and of the presence of additional inserted charges in dielectrics requires a more rigorous justification of the boundary conditions. The attraction of the general law of entropy increasing (of thermodynamics) to the proof of positivity of such microcharacteristic as σ looks artificial (in modern electrodynamics obvious things work not always: for example, despite the obvious movement of only negative electrons, sometimes we have to assume that positive holes move). Also the presence of permanent magnets proves the possibility of having $\mathbf{j}_0$ for anisotropic bodies, and the law of entropy increasing has nothing to do with it: the constant current (pyroelectricity) can, for example, always be perpendicular to the external field **E**.

Thus, even such an “engineering application”, as a description of the electric current, does not "shine" in its theoretical description under close scrutiny.

Chapter 4
Magnetic field

Let us now turn to the discussion of the concept of the magnetic field and the theoretical description of its manifestations.

Explanation of the effect of magnetic force on the wire with current [5, Chapter 13, Section 3] is not entirely correct: the positive basis – ions – are immovable (that is, the wire itself); the magnetic force cannot act on it. One might say, for example, that electrons move to the wire surface and create an electric field that acts on the metal ions, shifting the wire.

Separation into pure magnetostatics and pure electrostatics is too artificial: it is assumed that

1) the current is neutral as a whole (it is impossible, for example, to consider an electron beam);

2) we cannot investigate the same (!) phenomenon in different frames of reference moving relative to each other.

Generally speaking, in order to identify the presence of a source or a drain, it is not at all necessary for the lines to leave a point or enter the point, since perpendicular movement can also be present. Imagine that the current has just started to go (there is always the beginning of the process). The magnetic field will be present only at some distance from the current, to which the perturbation managed to reach. But after a while in a more remote place the field will also manifest itself in its final form. For example, for a rectilinear current, these will be rings. Consequently, the magnetic field rings do not stand still, but expand (in this representation, if they must be closed) in the direction from the source. This is also some flow from the source.

There are difficulties with a strict definition of the magnetic field of the current (force field) [8, Chapter 4, Section 42]. First, we are talking about **neutral** currents, but how to distinguish a **motionless element** of a neutral current is unknown. Secondly, again the properties of the magnetic field at small distances are unknown. Third, the formula

$$\mathbf{F} = \frac{J}{c}[ds \times \mathbf{H}]$$

is nothing more than a **definition** of the magnetic field strength of **H** (these can be various and they can be invented a lot); and else it need to take into account that the force **F** from Newton's law is also a simple definition of the "letter" **F** (remember Poincaré). Fourthly, the principle of superposition in this case is simply postulated in addition. Fifth, the Biot-Savart law determines the integral value in the experiment, and it is possible to extract the expression for the field of the current element from it in many ways.

The coefficient of self-induction in [8, Chapter 4, Section 52] was introduced in a purely formal way (in the definition, the current density is included in the integral). First, it is not proved that this coefficient is finite (that is, it can possess at least some physical meaning). Secondly, it has not been proved that it does not depend on currents (on the current strength), but depends only on the declared geometric characteristics, that is, it is a constant characterizing the contour itself (on the other hand, one is argued that the record in other textbooks, expressed through purely geometric characteristics has no physical meaning, since it becomes infinite). As a result, all subsequent postulative determinations using self-induction (flux Φ, potential energy) also have an unclear physical meaning.

Magnetic force lines [8, Chapter 4, Section 53] have a limited application in ordinary electrodynamics, not only because physicists do not wish to introduce a potential for a magnetic field in another way, or there are surfaces of continuous filling (torus, incidentally). In mathematics, from the fact of the tangency of the vectors $\mathbf{H}_1$ and $\mathbf{H}_2$ in perpendicular directions, it is not follow the tangency to any surface of the vector

$$\mathbf{H} = \mathbf{H}_1 + \mathbf{H}_2 \, .$$

Such the situation is only for a special case, in our case – cylindrical symmetry, and also because the picture assumes stationarity: $t = \infty$. But in such the case the description of the "continuation" of the line in time is inadequate. In fact, if we consider the moment when the currents are switching-on (unlike the separation of charges from one point, the switching-on of a current is always present), then we must represent the propagation and / or appearance of new lines of force. In this case, for the torus (due to symmetry), a line of force begins to appear **from each point** of the surface of the torus and "propagates" **along the existing** lines of force (but this picture **looks** quite stationary for the observer).

The given definition of connectivity [8, Chapter 4, Section 54] is incomplete. For example, for 2 ring currents J_1 and J_2 there will be 4 contours not reducible to each other by continuous deformation (and not 3, although the region is declared to be three-connected): 1) a contour not enclosing the rings; 2) a contour covering the current J_1; 3) a contour covering the current J_2; 4) a circuit that covers (through the center) both currents J_1 and J_2. The introduction of fictitious magnetic charges (using partitions supplementing up to the simply connectedness) by the proposed method does not prove the absence or existence of real magnetic charges, since there is no proof that they cannot be introduced in any other way. And the equations for fields can also be introduced in different ways: recall, for example, the expressions of Ampere. The question of the existence of monopoles, their “mathematical” (descriptive) properties and **observable** parameters remains open.

The introduced concept of magnetic sheets is very limited: it assumes a linear infinitely thin current J = *const*; the consideration concerns only external areas (there is also no account for the mutual influence of different elements of one and the same circuit). The multi-valuedness of the potential (an auxiliary function; the set of which can be introduced in different ways) is unimportant, since only the measurable physical quantities have meaning. When comparing the field of a solenoid with the field of a real magnet (and not with artificially dreamed up magnetic sheets), the question of how the field is distributed inside a real magnet must be solved experimentally.

In deriving the induction of currents in moving conductors, it is silently assumed that the conductor does not affect the external field, and there is no self-action (here, not even self-action, but rather the action of one part of the conductor on its other parts). If the expression for the force in the microscale is not strictly accurate, but also contains “microadditives”, then even the **integral** values can change for variable motions. The determination of the EMF-induction only through the total magnetic flux for variable motions, some changing sizes of circuits and variable fields is also doubtful (there arise also questions for non-neutral circuits), because, due to the finite speed of electromagnetic interactions, the very concept of a “contour” at the fixed time is somewhat uncertain. Even with the equality $\mathcal{E}_{\text{ind}} = 0$ for Φ = *const*, there are problems for the unipolar inductor and generator. The question with the possibility for the existence of the concept of “moving fields” (for example, rotating) [12] and the question with the possibility for the presence of inertial properties of the field are not closed.

When deriving the law of electromagnetic induction, for some reason it is considered that the quantities should not depend on absolute velocities, but only on relative velocities – but this is just a hypothesis. Hypotheses are also that the reason for the change in the magnetic flux is not important, and that only the total flux plays the role, but not its distribution. Generally speaking, this “spirit of field theory” (that all quantities at some point must depend on the field and its derivatives **at this same point**) is refuted by the phenomenon of hysteresis (i.e. by dependency on the path of transition to a given state). The presence of a magnetic medium actually means the appearance of absolute velocities in the dependencies (or at least more than two relative ones, which is the same thing). And a simple substitution of $\mathbf{H}_{\text{micro}} = \mathbf{B}$ is far from simple: different oscillations (in frequency) propagate and decay in different ways, the superposition principle is violated, and the result depends strongly on the averaging method. In practice, the confirmation of this formula for EMF [8, Chapter 6, Section 77] is possible only indirectly – from measurements of the current strength (and assuming the validity of Ohm's law). The idea of quasi-stationary currents is approximate and introduced from despair – to exclude rapid changes, secondary inductions and to give at least some (linear) approximation to reality.

From the methodological point of view, it can hardly be considered successful the derivation of EMF through the **postulated** (and unverifiable in microscale) force acting on the charge [6, Chapter 7, Section 7.3]. The latter has to be considered strict “for all time”, but this is too overstated claim. If the frame has a complex shape (twisted), then immediately the question arises in determining the flow – which of the surfaces stretched on the frame is “true” and why others will not work? Recall also that the preservation of the flow follows from the equality $\operatorname{div} \mathbf{B} = 0$, which we simply **chose**! When the rectilinear movement of the frame in a uniform magnetic field is considered, EMF does not arise, although the frame changes its position. But if an unipolar generator is considered in a **homogeneous** magnetic field, then the EMF arises (at once the inequality of “explanations” and the insufficiency of “reasoning” of modern electrodynamics are evident).

Before “to be jumping” between different systems and to claim what will be seen by one or another observer [6, Chapter 7, Section 7.4], it is necessary to clearly determine which of the quantities (whether all?) can be independently measured in each system. To talk about the invariance of Lorentz in Faraday's experiments [6, Chapter 7, Section 7.5] (at such low speeds) – it is simply ridiculous (and we should not be afraid to say only about the invariance of Galileo). Quite original “freaks” with mathematics occur when instead of a total derivative

$$\frac{\mathrm{d}\mathbf{B}}{\mathrm{d}t},$$

one substitutes the partial derivative

$$\frac{\partial \mathbf{B}}{\partial t}.$$

Have mathematicians seen this? After all, earlier all three experiences talked about independence from the causes of the flow change (that is, about the full derivative!).

In the self-induction expression for a toroidal rectangular coil with N turns [6, Chapter 7, Section 7.8], the relation $ln(b/a)$ is strangely included, where b is external, and a is the inner radius of the torus. That is, if

$$b \to 0, \quad a \to 0, \quad b/a = const,$$

then the inductance of this “disappeared” construction will be the same as before (finite). The flow turns out infinitely large through a wire loop with a wire of zero diameter! In the case of a finite diameter wire, however, the question arises: through which part of the loop wire to calculate the flow (this is the practical question: is the flow always proportional to the current, as in theory?).

To express the energy [6, Chapter 7, Section 7.10] through the integral over **the entire field** is not very good: from one state to another and back, the system can be translated much faster than it will be possible to “assemble” or “scatter” the energy of the field in the entire infinite space.

The derivation of the circulation theorem in [10, Chapter 3, Section 55] seems to be poorly substantiated methodically. First the concept of magnetic potential is used, but a little further (in deriving the differential form of the circulation theorem) one states that in a region where currents are present, the field **B** is non-potential. Then all the “derivation” is badly founded. If we assume, however, that everywhere, except for the wire of the contour, the field **B** is potential and only inside the wire is not potential, then there remains “too little space” for solenoidal fields. The equivalence of the magnetic field of the current and the magnetic sheet in the space outside the current (when the boundary conditions are joined) makes one wonder: after all, “inside the current” will mean “inside the moving electron”, but everything related to the internal structure of the electron (including the distribution of electric and magnetic fields) so far remains only in the field of our fantasies (or faith!).

Molecular currents in matter are also a matter of faith: we cannot measure them directly to confirm any dependence of field **B** on them. Rather, the form of this dependence is postulated in advance (to resemble laws in a vacuum). And with the help of measuring the field **B** we are suggested to fit the **required** values of $\mathbf{j}_m$. The **consequences** of our **choice** are also: a record [10, Chapter 3, Section 59] $\mathbf{j}_m = c \text{ rot } \mathbf{I}$, the definition of the vector **H** and the boundary conditions. Otherwise, in another form, for our "remaining" choice, we would simply get internally contradictory definitions or ambiguous decisions. Thus, our choice **does not at all mean** that Nature is arranged just like this, and you cannot introduce other description models! With the chosen description method and in the presence of inhomogeneities of the medium μ, the field **H** does not even mean the field of conductivity currents in a vacuum (!), since [10, Chapter 3, Section 61]

$$\operatorname{div}\mathbf{H} = -\frac{\mathbf{H}\operatorname{grad}\mu}{\mu} \neq 0\,.$$

To the question of the allegedly existing qualitative "differences" between the electric and magnetic fields, we note that the screening properties in both cases depend on the properties of the medium of the shell: for example, **complete magnetic protection** against the external field is possible inside the shell from the superconductor.

Thus, a close look at the theory of the magnetic field reveals much more of fundamental questions than the conventional theory gives answers.

Chapter 5
Equations of Maxwell

Let us now turn to the “holy of holies”, the foundation of the whole electromagnetic doctrine, namely to the Maxwell equations. The whole "triumph" of the theory of electromagnetism, all its "strength" and "rigor", must manifest itself just here! However, we’ll look more closely.

Let us start with an auxiliary comment. The principle of superposition of artificially selected fields for a nonlinear (for **v**) relativistic equation of motion is belief or definition (and not science). Taking into account the ideology of retardation, the superposition principle, even for two particles, turns out infinitely complex (declarative). Relativism is trying to "kill" the only verifiable force approach, and in return substitutes an abstract and unverifiable field approach.

The law (1.6) from [5, Chapter 1, Section 4]:

The flow of **E** through any closed surface S =

$$= \frac{\text{The charge inside it}}{\varepsilon_0}$$

requires clarification: S is a fixed surface that is motionless relative to the charge for a time longer than $t = R_{max}/c$, where R_{max} is the largest surface removal from the charge. A similar explanation is required for the law (1.8) from [5, Chapter 1, Section 4]:

$$\text{The flow of } \mathbf{B} \text{ through any closed surface } S = 0.$$

But the “law” (1.7) from [5, Chapter 1, Section 4]

$$\text{Circulation of } \boldsymbol{E} \text{ along the contour } C = \\ = \frac{\mathrm{d}}{\mathrm{d}t} \text{Flow of } \mathbf{B} \text{ through the surface } S$$

does not give any mechanism for such circulation. Firstly, the flow of **B** can be changed only in the center of the surface S, without touching its edges – the contour C itself (e.g. when inserting a long solenoid into center). Where is the cause then? Does the circulation of **E** change? Secondly, where is expressed the mechanism of the retardation of response in relation to the signal here? Similar remarks will be for the law (1.9) from [5, Chapter 1, Section 4]:

$$c^2(\text{Circulation of } \mathbf{B} \text{ along the contour } C) =$$

$$= \frac{\mathrm{d}}{\mathrm{d}t} \text{Flow of } \mathbf{E} \text{ through the } S +$$

$$+ \frac{\text{Electric current through } S}{\varepsilon_0}.$$

All the equal signs in the differential equations of electrodynamics are not entirely correct, since these are not equations but simply a record of the fact: **the cause causes an effect**; and they cannot be changed by their places or even their component parts cannot be transferred (although this is correct by all the rules of mathematics!).

To tie the accuracy of the Coulomb law [5, Chapter 5, Section 8] to the difference in the energies of the hydrogen atom in the Lamb and Retherford measurements – is a big hypothesis (both values – are generalizations of theories, and there is no direct experimental verification for them).

It may turn out that there is a field **E** inside the conductor (for example, if all the conduction electrons leave a certain region, then in this region the conductor locally ceases to be a conductor and the existence of the field **E** is possible). The condition rot **E** = 0 for electrostatics – is not a property of Nature, but a purely theoretical consequence of Maxwell’s equations (an additional condition).

The principle of superposition must complement the concept of the field, otherwise it is impossible to divide the field into an external field and the field of the test charge and the concept becomes meaningless (undefined or complicated). However, the superposition of fields can be postulated only for a vacuum (linear medium). The presence of a nonlinear medium leads to the fact that the response of the medium is related not only to the characteristics of the external field, but also to the characteristics of the test charge. There are no principles for taking into account the nonlinearity of the medium, and it would be necessary to postulate the method of introducing them (in this way, the generality of the field equations is lost).

Fixation of separate charges (whether by demons or what?) – is a fantasy. In any case, they will still "spread out" over a finite time, striving for the **equilibrium state**. Further, if we believe Feynman's confessions, then there is no special method for solving electrostatic problems with the self-establishing charge distribution (not fixed from outside). For example, in the image method, everything happens *post factum*: first, an unclear problem with fixed charges is solved, but if **some analytical equipotential surface** is detected, then one is pretended that the reverse problem is constructed with a charge near **the similar conducting surface**, that is, the theory is not algorithmic. The theory of functions of a complex variable is applicable to the two-dimensional case only and, in any case, it is an indirect method (again, backwards). There remains only a numerical method.

The question of the possible existence of a moving magnetic field or a moving electric field and their influence should be solved experimentally, and not on the basis of an unlimited belief in Maxwell's equations. As well as such remark relates to questions about the degree of electrostatic protection and about the distribution of current and fields in the wires (Ohm’s law).

The introduction of additional quantities, for which some combinations do not make sense, always causes certain doubts (as if theoretical "fitting" for Titius-Bode law, but with additional orbits). An increase in the degree of differential equations leads to the need to increase the number of boundary (and / or initial) conditions. However, where to get them, if they are not measurable experimentally? Again, we get the "fitting" for what we want to see (rather than algorithmic theory). Such a situation takes place, for example, with the introduction of vector and scalar potentials [5, Chapter 14, Section 1], when it is absolutely necessary to impose additional conditions in order to find a solution (the system of equations is underdefined), and therefore it is always possible to adjust it to the desired result (that is, the theory does not have a predictive power, since one must always peek in answer).

Remark to [5, Chapter 15, Section 4]: both the potentials φ and $\mathbf{A}$, as well as the fields $\mathbf{E}$ and $\mathbf{B}$ are conventionally introduced mathematical symbols (**auxiliary** quantities). Directly measurable are in classical physics: coordinates, time (or reference frequency), mass, force, speed. Of course, it is possible to calibrate the instruments also to other quantities, but this will depend on the **belief** in certain interrelations, mechanisms and interpretations. To compare modern electrodynamics and quantum physics [5, Chapter 15, Section 5] is not entirely correct, since they were artificially (postulatively) divided. The Aaronov-Bohm effect [5, Chapter 15, Section 5] – is more evidence of the incorrectness of our ideas about magnetism than about the "truthfulness" of the vector potential $\mathbf{A}$. Generally speaking, the goal of physics as an **experimental**-theoretical science can only be to find relationships between (all) independently measurable quantities and to predict the consequences of the found similar laws. Introduction of delay [5, Chapter 15, Section 6] into integral equations makes them completely unsuitable for calculations even in the case of emptiness, and in the presence of a substance whose state can depend on the process of propagation of the electromagnetic field, the equations become clearly underdefined and leave the possibility of fitting to the desired result (non-algorithmic theory).

Most electrotechnical devices use the same principle of action of a magnetic field on a current, that is, in fact it is the same device with graduations for different physical quantities already in accordance with generally accepted theoretical interpretations. Therefore, it is impossible to measure **independently** with one and the same instrument different physical quantities and to check any laws. Although in reality the Maxwell equation in differential form is a consequence of the equations of the same name in integral form (and only the integral form is tested experimentally), nevertheless, Feynman cites exceptions to the “flow rule” [5, Chapter 17, Section 2], when the flow through the circuit does not change, but the EMF is, and, conversely, when the flow changes, but the EMF is absent. That is, the theory is non-algorithmic – one must know the answer in order to explain with an intelligent kind that this is the way it should be. Moreover, as a matter of fact for the idea, both examples of Feynman are identical, but the result is the opposite! It also looks badly the linking with the completely different equations for the results of the EMF’s appearance due to the motion of the circuit or due to a change in the field. All this indicates even the insufficient completedness of the base of electrodynamics. Further, the phrase about a "sufficient slowness of changing in values" is surprising when discussing fundamental questions: in fact, it is not known in advance which derivative is the most important – this is something that should be established experimentally (a similar phrase could make sense for an already well-tested theory including the indication of what the comparison will be made with).

The “moving field” of an infinite plane [5, Chapter 18, Section 4] – is in itself an unrealistic example, and even more so, when such a movement arises instantly (if only because that endless derivatives arise and passage to the limit can lead to different results). Artificial including of joint rules for changing scalar and vector potentials, as well as the joint calibration conditions [5, Chapter 18, Section 6], first, states the fact that the force is only one, and all the quantities **B**, **E**, φ, **A** are auxiliary ones, and, secondly, it can limit or discard certain solutions of the Maxwell equations (but there are no theoretical grounds for this).

Potentials – are artificially entered quantities. It is also artificially “taken from the ceiling” assumption that the potentials depend only on the position and velocity of the charge at a retarded time (rather than, for example, from acceleration). How far can all supporting assertions be elevated into the rank of principles? Methodical error – in [5, Chapter 26, Section 2] Feynman describes the change in the field **E** during the motion of a charge: the field decreases in front and behind the moving charge, and on the flanks it increases, and in [5, Chapter 26, Section 3] writes the transformation of fields

$$E'_{\parallel} = E_{\parallel}, \;\; E'_{\perp} = \frac{E_{\perp}}{\sqrt{1 - v^2/c^2}}$$

and says that it is the same thing. It also causes bewilderment to mention the "famous problem of determining the speed of an airplane" [5, Chapter 26, Section 3], if these calculations have never been tested (real oscillations of the field for many other reasons in the hundreds of volts will not allow us to feel these calculated microvolts), this effect cannot be separated in practice.

It is hardly possible to regard Maxwell’s differential equations as more general than the experimental macroscopic laws from which they are “extracted”. For example, the use of Gauss’ theorem for mathematical transformations is very limited: the Coulomb law, from which the flux is derived, has been verified only at sufficient distances from a **non-point** charge (and the actual scatter of accuracy with respect to coordinates is comparable to the particle sizes). Even the charge was considered somewhat idealized – actually averaged: there is no structure of microparticles, they do not have magnetic moments, and their properties are spherically symmetric. Consequently, even for a single electron, this conclusion is inapplicable (exactly, non-strict, unproven). And even more so to break the elementary charge into even smaller non-spherical parts (with an unknown real structure, density and moment) – is a completely absurd hypothesis. Since in STR the charge is a point charge, then div **E** is infinite at these points, therefore, STR and electrodynamics are not at all fit together. Therefore, in addition to the requirements for the finiteness of values div **E**, ϱ and the absence of charges on the confining surface, in fact, the averaged values div **E** and ϱ (over some dimensions exceeding the dimensions of the elementary charge) were used in the deriving. And the equality of the integrals for the averaged quantities does not at all imply the equality of the integrands, since the volume dV is not completely arbitrary: it is not less than some finite volume. Thus, it is simply a belief in the possibility of introducing functions that produce the same result on the macroscale as the averaged microvalues of div **E**, ϱ. But on the microscale (however, as on the megascale), their behavior can be arbitrarily different; it is the prerogative of experience to investigate them for elementary particles. In addition, the Coulomb law is more definite than the differential equation

$$\operatorname{div}\mathbf{E} = 4\pi\varrho,$$

because from the magnitude of the charge in the Coulomb law the force acting on the test charge, that is, the field, uniquely follows. But in the case of a differential form, a lot of fields can correspond to a single charge (that is, in the equations there is actually no equal sign and the derivation is not exactly identical).

The definition of the work of electrostatic forces – is methodical nonsense at all. After all, all this is deduced for mutually resting bodies (otherwise, what is statics?)! In fact, the electric force depends not only on the speed, but also on the acceleration, therefore, for paths of different shapes (since the accelerations will vary at least in the direction), the work will be different (and the integral along the closed path will not be zero). And for adiabatic motions, one should search for a limit and prove its constancy for **v** → 0, **a** → 0. Using the mathematical Stokes theorem to determine the circulation of a field [8, Chapter 1, Section 7] is not entirely correct. First, the contour must not intersect either surface or volume charges, the field value must be finite and continuous (STR with its point charges is immediately discarded). Secondly, the Coulomb law for the field is inferred (is checked) only for the "mesoscale" and its behavior in microscale, comparable to the particle sizes, is unknown. Thus, we cannot arbitrarily change L in

$$\oint_L E_l \mathrm{d}l$$

the contour L cannot be reduced as you like; similarly, S – is finite for $\int_S \mathrm{rot}\,\mathbf{a}\,\mathrm{d}S$. But then from the integral condition

$$\int_S \mathrm{rot}\,\mathbf{a}\,\mathrm{d}S = 0,$$

it does not follow that $\mathrm{rot}\,\mathbf{a} = 0$ – this quantity can appear arbitrary on the microscale, for example, contain rapidly oscillating and rapidly decreasing terms with distance. The continuity of E_t is also not entirely obvious; for example, the case of a jump by a constant quantity is possible, and then the proof by means of work on a closed contour does not exclude anything.

The introduction of physically infinitesimal quantities and averaging over a certain small volume assumes that this quantity is linear and has a “linear” effect on the quantity of interest for us. Otherwise, it is necessary to enter effective values (other than average) and to seek a relationship between effective, not true values. Because only this connection and only such quantities are measurable in experiments. The equality

$$\frac{\overline{\partial\psi}}{\partial x} = \frac{\partial\bar{\psi}}{\partial x}$$

for the scalar [8, Chapter 2, Section 25] was derived under the additional assumption of the non-deformability of the volume and its continuity (connectivity).

Since the equations of the microscopic field are not derived strictly for microscopic scales (rather, they are inconsistent at small distances), then rigor is also absent in the transition to the averaged macroscopic equations [8, Chapter 2, Section 26]. Some internal dissatisfaction is felt in the description of dielectrics (non-self-consistency, non-self-sufficiency) – so, the postulation of quasi-elastic forces [8, Chapter 2, Section 27] induces “peek in the answer” (to measure in advance some properties, so that they can then be described in the “right way”). Equating the average values in determining the polarization of the dielectric is a big “stretch”. The fact is that the average values, as a rule, are much smaller than the amplitudes of the quantities (for example, fields) at the microscale. Therefore, such an equating – is a latent belief that everything can be linearized in certain limits. However, such an average leads to a replacement of the polarizability by some effective value (since nonlinear regions of the dependence are also “included”). As a result, we have some primitive phenomenology, since there is not even a theoretical connection between this effective coefficient β_{eff} and true β from quantum-mechanical (or any microscopic) calculations.

What can, and what cannot be expected from Maxwell's equations?

First, one and the same phenomenon can be described by several equations. It is obvious that any **physical** dependence of the quantity on the parameters is defined not as a **mathematical** line, but with some finite accuracy (recall also about the accuracy of determining the parameters themselves, about the inertiality of the instruments, about some weak relationship of this isolated phenomenon with other phenomena and about the presence of fluctuations), that is, on the graph the **physical** dependence is expressed by a strip, and not by a **mathematical** line. Thus, one and the same physical dependence (even, perhaps, a complex of phenomena) can be described with the same degree of accuracy by several different equations (even different "in quality": algebraic, transcendental, differential, integral, operator, etc.).

Secondly, if some specific equations are chosen that describe some specific phenomena (for example, the Maxwell equations or the Dirac equation), then there is no guarantee in advance that they describe the **whole complex** of similar phenomena (including those that will be discovered in the future). Unfortunately, very often the fanatical belief in the "infallibility" (the fundamental rigor and completeness) of certain equations forces pseudo-researchers to save this faith at any cost by inventing various *ad hoc* hypotheses (especially for the concrete individual phenomenon) and pronouncing in "in the difficult places" some "scientific spells".

Thirdly, there is no guarantee that **all the particular solutions** of some "eminent" system of equations have at least some physical meaning. Let us recall the "nonphysical regions" that are emerging from somewhere, spells about the inapplicability of equations in some area, about which physicists “stumbled” backdating, and so on.

One of the tasks of physics – is clarification of the physical meaning for particular solutions. Maxwell’s equations, like all systems of partial differential equations, are “overloaded” (many “arbitrary” solutions can be obtained by imposing certain additional conditions – boundary, initial, gauge, and also depending on imparting the physical meaning to a particular combination of symbols). According to the private opinion of the author, ideally (sometime in the future) physics should develop not in the way of complicating the equations (of mathematics), but, on the contrary, seek simplicity, a search for a combination of principles, for a combination of solutions (possibly the simplest ones – algebraic).

So to recognize Maxwell’s equations as an “ideal for all time” is in no way possible.

In most of the equations of physics the equality sign cannot be put, since it is impossible to change the cause and effect (and it is necessary to clearly divide what is **given** as an external condition, and what is searched by the meaning of the formula). For example, in chemistry, instead of equality, arrows are often placed to indicate the **direction of the process**. One and the same effect can be caused by several causes (expressed by different laws and formulas, and knowledge of the effect does not lead to automatic knowledge of the cause). Each formula has its own area of applicability (the conditions for the implementation of the law and its applicability to this particular case). There are cases when the conclusions made with the "turned" formula turn out to be incorrect (this should also be remembered when solving the system of equations).

When author attempts to “strictly” infer the macroscopic equations of the magnetic field [8, Chapter 5, Section 67] and makes a lot of simplifying assumptions, then one is pseudo-scientifically excuses that “all the same the auxiliary quantities will drop out of the finite equations”. But this “spell” is not a strict proof, since in this case the numerical coefficients may be different from 1 (for example, for volumes and areas the ratio of V^2 to S^3 depends on the shape of the bounding surface S).

Note that the displacement current is introduced purely formally [8, Chapter 6, Section 88] in order to coordinate the equation of the magnetic field of the steady-state currents with the continuity equation (that is, again, for postulating the former form of the equations). Since the first of these equations can contain unaccounted additives (the integral effect of which vanishes), then the new equation turns out to be true with a similar “accuracy” (a question may arise also: why do we attribute a new addition of the so-called “displacement current” to a new definition of the quantity $\mathbf{j}$, but not change, for example, the quantity $\mathbf{H}$?). Besides the generally accepted expression, **from the same principles**, one can add to $\mathbf{j}_s$ **any** function whose divergence gives zero, and, depending on the specific formulation of the problem (initial conditions, boundary conditions), these functions can be different! The conduction currents and displacement currents turn out to be nonequivalent for a number of effects (for example, Joule heating), and we actually have to “peek in the answer” to say “plausible spell”, “explaining” why in some cases they are taken into account, and in others – no. (The question of whether a field $\mathbf{E}$ can exist at all without charges remains open: physicists have long ago “drifted” away from absolute emptiness without properties to a “physical vacuum with a set of properties”, so that, perhaps, the field $\mathbf{E}$ can be determined by vacuum polarization?). The validity of Maxwell's equations in real media is also in doubt because of the presence of frequency dispersion and non-linear properties that depend on the process itself (to this part of the knowledge, physicists have not yet come up close). In the case of quasi-stationary or alternating currents for an open circuit, the result depends strongly on the “path of closure” of the contour, and the shortest path is not distinguished in any way – this is another plausible “spell”. It could be justified from the viewpoint of the energy flow, but under the assumption of the presence of inertial properties for the fields, then the energy of the electromagnetic field in a vacuum for inertial systems would propagate along a straight line. However still there arises the question of generalizations for the case of finding the current loop in non-inertial systems.

When the fields (and force) are calculated from the integral laws at a certain point, then one must know the distribution of charges and currents from the affecting region (and this setting of distributions does not cause difficulties of principle). But when solving differential equations for fields, we must know the initial conditions in the entire region and the boundary conditions on the entire (some) closed bounding surface in space. The assignment of such conditions is never rigorous and is not always even intuitively obvious (this is the question of the preferred form for the basic equations of electrodynamics). In fact, we are trying to postulate the desired for us form of distribution (fields) in the formulation of the problem, but there remain still questions about the realizability (and stability) of this “academic” solution.

Author quite clearly admits in [8, Chapter 7, Section 91] that after generalization of empirical laws the system of field equations is declared “mathematical axioms”, and after then they can be solved only strictly or approximately mathematically, without thinking about their physical rigor. Macroscopic field equations are derived in some simplifying **assumptions** about the properties of the medium (that is, these – are **model** equations and it is not necessary to declare them strictly true “for all times”). It is assumed that the function (which is an arbitrary constant from t) of divergence from $\mathbf{B}$ is always equal to zero, regardless of the statements of the problems. Further, we do not have a system of equations (!), since one of the equations:

$$\operatorname{div}\mathbf{D} = 4\pi\varrho$$

is a **definition**. A linear relationship is also postulated to describe the properties of the real medium. We have 17 linear equations with respect to 16 unknowns (hence, for the uniqueness of the solution one equation must be linearly dependent, or it is an adjustable function!). What can mean the choice of $\varepsilon, \mu, \lambda$ as the given functions? For such a choice, if we have already made electric measurements of these functions, then what is the significance of these equations, after all, instead of measuring the auxiliary characteristics, we could immediately measure those quantities that interest us. In fact, Maxwell's equations cannot be regarded as self-consistent (and complete!), and one must resort to some other theory that allows one to **independently** calculate these parameters of the medium.

It is remarkable that in [8, Chapter 7, Section 91] author clearly emphasizes that without specifying a **specific method for measuring** the impact due to electromagnetic processes, Maxwell's equations do not in themselves mean anything. But "to determine" is proposed through a change (**postulated** expression) of electromagnetic energy! The derivation of Maxwell's macroscopic equations from the microscopic equations of the electromagnetic field also occurs under a number of **additional** assumptions.

In proving the uniqueness of the solutions of Maxwell's equations [8, Chapter 7, Section 93], **too** strong conditions are used: either fields **E** and **H** are given at time $t = 0$ in the whole (!) space, which is completely unrealistic both theoretically and practically, or the fields **E** and **H** are given at $t = 0$ in some volume (which is also difficult), and additionally for one of the fields, the boundary conditions are known on the surface bounding the volume during the whole time $0 \leq t \leq t_1$ (it is required a bit too much the auxiliary data). Moreover, after then the following additional conditions are imposed: $\mathbf{E}^{\text{ext}} = 0$ and the possible different solutions coincide at the initial moment. The use of the approximate Poynting vector for such general proofs is questionable in rigor. The conditions at infinity are also very rigid, and, generally speaking, they exclude classes of tasks with radiation, with a constant flux, with given oscillations (fluctuations).

It is not clear, by what the integral equations of the XIX century were unsatisfactory from the practical point of view? Knowing ϱ and **j**, you can clearly calculate both the real forces and (auxiliary) fields **E** and **H**. Is it only from the principle, to abandon the theory of long-range action? As a result [8, Chapter 7, Section 94], instead of **six** unknown field components, the new **four** auxiliary quantities are introduced – the potentials φ and **A**. Therewith, the properties of the medium are considered to be constant (otherwise you would not count anything, although they are not known before the measurements!), and, in fact, div **B** = 0 – is only a special mathematical case: if μ – is a function, then we must choose either div **H** = 0 or div **B** = 0. Thus, the definition

$$\mathbf{B} = \operatorname{rot} \mathbf{A}$$

is already limited (the definite particular hypothesis). Further, an **additional relationship** is imposed on these **four** quantities:

$$\operatorname{div} \mathbf{A} = -\frac{\varepsilon\mu}{c}\frac{\partial\varphi}{\partial t}.$$

As a result, we again have to calculate φ and **A** using ϱ and **j**, and only then – the fields **E** and **H**. Eventually, we still get a half-way theory, since in the field theory, ideally the quantities (as sources of the fields) ϱ and **j** should be the desired functions. But this problem is not solved mathematically (since it is mathematically too complicated), that is, someone only practiced in writing “hooks” (since the statement of the problem since the XIX century has not changed: ϱ and **j** are given, find **E** and **H**).

In the solution of the spherical problem, in addition to the delayed ones, advanced potentials are also obtained [8, Chapter 7, Section 95], which, as a rule, are arbitrarily discarded. For one point source, this can still be intuitively accepted (again we “correct” the rigorous mathematics), but for charges of finite size (when each point is surrounded by other charged points and it is necessary to take into account the “reflected” fields at different times), a justification is required. Solution with retarded potentials [8, Chapter 7, Section 96] assumes a fairly artificial case of constancy of the medium properties ε and μ, otherwise we have not potentials at the point, but some “complex averages”. Writing scalars R and v instead of the vectors means linear motion of the charge and observation of it along one line. The dependence of the potentials at each point on $t - R/v$ for the whole space, means knowledge of the distributions of charges, currents and motions at every point of space throughout the entire time (that is, too detailed knowledge in the final form is required)! The equations obtained show that the displacement currents are an auxiliary concept, since they do not exist without the motion of charges. However, they are used in the mechanism of field propagation, and therefore the theory seems not entirely consistent, since the **intermediate** link in the chain again refers us to the unknown **root cause**.

In the textbook [6, Chapter 7, Section 7.11], the displacement current is introduced artificially with the sole purpose – to make compatible the system of introduced differential equations. But note that this compatibility has nothing to do with the following questions: are **all** cases covered by this system of equations, and also **whether** the solutions in solvable cases are exact or approximate, and how the such introduced method of describing electromagnetic phenomena is universal, unique and useful ("authors regret" seems ridiculous that Maxwell could not use the theory of relativity). In fact, in Maxwell's equations [6, Chapter 7, Section 7.13], we have 8 linear equations, but the charge and current densities are assumed to be defined throughout the space, that is, there are six unknowns (and the divergent equations – are the boundary conditions for fields). Note also that often it is necessary to specify the symmetry of the problem separately (and not automatically receive it in the solution!). Next, do not confuse the order of questions and answers: 1. Exists an **exact** solution in Nature? And if – yes, then you can check: 2. Does Maxwell's equations give this solution? But the reverse check order is not proof that any solution of the Maxwell equation is an accurate description of the Natural phenomenon! And finally, the powerful principle of using the superposition of solutions is applicable **only to** (linear) Maxwell equations in vacuum, and not to nonlinear equations in real media. In general, it is also strange to involve STR to electrodynamics, since charges in STR are pointwise in principle, rather than distributed; but then many practical problems would have mathematical features.

Of course, Maxwell's equations can be considered as the basic axioms of electrodynamics, but the purely deductive method (theoretical) is always vicious, since it does not allow any subsequent amendments to laws (dogmaticity). Further, the Maxwell equations themselves do not have any meaning at all until a specific "closing" equation is introduced for the force (only then does physics begin: what do “letters **E** and **B**” mean and how can the result be verified experimentally!). Therefore, semi-mathematical profiteerings about the properties of Maxwell’s equations (for example, their invariant properties) look strange. And to check the “principled” (that is, infinite!) severity of the introduced expression for the Lorentz force is not possible. Moreover, it is fundamentally impossible to prove the strict preservation of a charge (its **additivity**, independence on the motion of the system, etc.) – this is just our **choice of a way for describing** phenomena (perhaps it is the best?).

The Gauss theorem, as a **scalar** expression, always means **less** than the **vector** expression of the experimental (!) Coulomb law. Therefore, the elevation of this theorem into the rank of the basic postulates of electrodynamics makes it experimentally unverifiable (not provable and not disprovable) and represents a kind of "cunning" that allows one to assign the "required" form of solutions (“peeping” in the answer or artificially “assigning” symmetries, invariances, etc.).

That “fact” that $\operatorname{div}\mathbf{E} \neq 0$, but $\operatorname{div}\mathbf{B} = 0$ is only a consequence of **our choice**, when we agreed to direct the force lines of the field **E** along the radius vector **r**, and, from the expression

$$\mathbf{B} = \frac{q}{cr^3}[\mathbf{v} \times \mathbf{r}]$$

(by the way, which is completely untested at small **r**), we agreed to direct the force lines **B** perpendicular to the radius.

In the derivation of the Maxwell equations, when the notion of the displacement current is introduced [10, Chapter 4, Section 81], a great deal of arbitrariness still remains. First, it is argued that there are laws based on the concept of action at a distance, and allegedly these laws do not work. But in fact, a little earlier on the basis of just these laws their differential **form** (only!) was deduced, and the full equivalence of these forms was proved. Where did this equivalence go now?

Secondly, if one takes into account the finiteness of the velocity for propagation of influences and the rapidity of the process variability, it would be worthwhile to give some attention (apart from formal mathematics) to the discussion of physical principles in deriving these very differential equations: the choice of boundaries and the definition of their properties (are fixed and where, or propagate together with the field?), the properties of the fields at the boundaries (constancy of quantities or fluxes?). The fact that the equations can be represented in a differential form is only **an auxiliary mathematical condition** for use in field theory. We must proceed from the goals of constructing such a theory – the possibility of describing fast-changing processes, taking into account the finiteness of the propagation velocity of interactions. And only to satisfy this **physical requirement** it is worth to check the proposed equations (Gauss' theorem, the absence of magnetic charges, the law of electromagnetic induction). This is not done.

Thirdly, the introduction of displacement current (with the purpose of transforming the circulation theorem) looks completely artificial. After all, if we start from the law of conservation of charge (where **j** includes the whole current, and ϱ – is the whole charge) and Gauss' theorem, then it turns out that the whole current (!) is equal

$$\mathbf{j} = \frac{\dot{\mathbf{D}}}{4\pi},$$

but to this value they equate only the displacement current $\mathbf{j}_{\mathrm{disp}}$. The question immediately arises: and why does this "displacement current" not participate in the law of charge conservation? ("Trishkin kaftan" turns out: either here, or in the law of circulation, one must to "patch" the hole.)

Well, and fourthly, this choice is not unique (as the author of the textbook [10, Chapter 4, Section 81] noted also), since to the value of **j** it allows to add an arbitrary vector, the divergence of which is zero.

Fifth, the phrase about the "confirmative experimental facts" with two pointing considerations (examples) sounds absolutely "sluggish": they are talking only about the interpretation of data – in attributing the numerical values required for modern theory to those quantities that are not directly measured. In addition, we recall that the equations of electrodynamics were conceived and are still **deduced** as field definitions for all **given** (!) **charges and their movements** (currents). True, some authors clearly overestimate the possibilities of modern electrodynamics, believing that it is able to self-consistently describe all the parameters. However, life (experience) refutes this inflated claim (let us recall the fact of the creation of quantum mechanics, the permanent hypotheses *ad hoc*). Therefore, in particular, to the first example (radial currents from the ball), the question arises: why do you think that the process **by itself** must occur exactly as you set it artificially? If the **necessary** currents **are provided** by external forces, then these external forces must enter explicitly into the equations of field theory! In the second example (capacitor plates, connected by wires), it is not all smooth either. Inside (in the middle) of the vacuum capacitor there are no charges $\varrho \equiv 0$ at all, and then it follows from the Gauss equation that $\operatorname{div}\dot{\mathbf{D}} \equiv 0$; there are no polarization charges in the middle between the plates, so $\operatorname{div}\mathbf{E} \equiv const = 0$ ($\operatorname{div}\dot{\mathbf{E}} \equiv 0$) and only. Thus, by what law (expression) **to determine** the magnetic field – remains open (this must be solved experimentally for rapidly changing processes).

Finally, sixthly, the field theory being created does not at all require that its equations be partial differential equations: the **locality** requirement – is an artificial additional mathematical condition not at all grounded by any physical principles!

In the textbook [10, Chapter 4, Section 82], only four equations are honestly named as Maxwell’s fundamental equations, and it is suggested to look at them as axioms of electrodynamics. Naturally there arise doubts: why did one tried "to justify" them before, "to derive" them earlier? Yes, and a set of axioms of this kind can be introduced (the question arises if not about uniqueness, then at least about the best "model" choice). From the physical point of view, the proposed system is incomplete, since it does not include equations for measuring the used "letters" at all. From the mathematical point of view, the system is also incomplete: 8 equations are written for 16 quantities. Of course, in pure mathematics for nonlinear equations in the case of a nonlinear medium, the number of equations does not necessarily have to coincide with the number of variables. However, for the equations of physics (because of the continuity of the quantities and the principal possibility of linearization in a small area), apparently, these numbers must coincide. Neither the presence of boundary conditions nor the addition of material equations change the state of affairs. In addition, material equations introduce new unknown functions (or constants) of the medium for which there are no equations, that is, they are externally given functions (or numbers). Consequently, it is necessary to part with the claim of electrodynamics to the complete self-consistent solution of problems. Thus, the phrase about “the fundamental nature of Maxwell’s equations” looks unfounded.

There is also some discomfiture with gauge invariance [7, Chapter 3, Section 18]. First, it is easy to see that the possibility of adding any arbitrary vector to the vector potential, and any constant to the scalar potential – is not “in particular” (as written in the textbook), but **in addition** to the gradient invariance. Secondly, the influence is determined by the force and, therefore, the values of **E** and **H** must **not be identified individually**, but in their **combination**, which is included in the expression of a single force! This provides even greater arbitrariness in the choice of **A** and φ. Therefore, it is completely unknown how many, when and what additional conditions can be imposed on the potentials (especially in nonlinear cases).

The boundary conditions for the Maxwell equations are not established from some physical principles, but simply in order to the Maxwell’s equations have an unambiguous solution in model cases. And what are these conditions really (in Nature)? Is it really impossible to create magnetic fields with jumps of values of **H** or **B** (and what do you mean by the scale of the transition area)? For example, in several parallel-coupled different infinite solenoids there will be different magnetic fields; when it is pulsely switched on, the front has a sharp boundary; recall the impulse of light. And if there exist examples in some cases, then why they will not be under other conditions (what are the principles)?

Despite the publicized universality and rigor, Maxwell's equations are introduced under the assumption of the immobility of the medium (we recall that Hertz's attempt to take the medium's motion into account in an analogous way to Maxwell's method was unsuccessful). However, changing the fields (even just their switching-on) leads to the action of the field on the environment itself and its movements (and changes in properties). Consequently, strictly this could be done **only in the electrodynamics of moving media** (not yet created, but not in the modern electrodynamics, based on Maxwell's equations). And the "coding phrase" about the smallness of the ratio v/c has nothing to do with it. Firstly, such a phrase would make sense if in strict electrodynamics the values of the same effect were compared in two cases: when the medium moves with the velocity v and with the velocity c. However, we do not know what effects might be observed when the medium moves with the speed c. Belief in the "relativistic cataclysms" in modern electrodynamics leads to the impossibility of estimates from such a comparison in general. Secondly, comparison of effects for the case of motion ($v \neq 0$) and in the static case ($v = 0$) is also impossible, since the probable appearance of the magnitude of some **new nonzero effect** in the transition to the electrodynamics of moving media is also unpredictable: the ratio of a nonzero quantity to the zero value will be such indefinite (uninformative), as well as the ratio of the corresponding nonzero velocity to the zero velocity.

Thus, Maxwell’s equations represent rather “quicksand” than an “unshakable foundation”. The given foundation of the electromagnetic theory cannot be considered as the standard of the scientific theory, either for validity, or for uniqueness, or for rigor, or for algorithmicity.

Chapter 6
Energy of fields. Force

Let us now turn to such capacious concepts in the theory of electromagnetism as potential energy, potential, force and analyze their internal self-consistency, non-contradictoriness and conformity to experiments.

Generally speaking, the potential energy is always associated with the mutual position of interacting objects [5, Chapter 8, Section 5] and the rest of the Universe has nothing to do with it (it is tied or localized usually at the location of the object under study). To connect this energy with one single charge, and even indivisible, (as well as to abstractly consider charges as point ones) – is useless intellectualization. To connect the potential energy of interaction of charges with the energy of the field [5, Chapter 8, Section 5] is incorrect in principle. Imagine that we quickly changed the relative position of the two charges. The potential energy will change immediately, but, for example, what does the field at a very large distance from both charges have to do with it? After all, even with the speed c, this disturbance would not have reach. If we cut off the surface of integration not at infinity, then the integral over the surface does not disappear in calculations and the formula still turns out to be incorrect. In general, if it is experimentally established that the force acts on a straight line between charges, what “value” does the field have in all other places of the Universe? Undoubtedly, a field can have energy, but *any* field? The formula is strange also because the fluctuations lead to a non-zero level of potential energy. It is strange to associate potential energy also according to the theory of short-range, because the field of a fixed charge at a certain point in space is completely definite, and placing a test charge at this point in space immediately causes the force action (that is, the value of $U = -\int \mathbf{F}\mathrm{d}\mathbf{r}$ is already determined) at this instant t, and all previous $t - \mathrm{d}t$ are unimportant.

The state of the body is characterized by its coordinates (x, y, z) and velocity $\left(v_x, v_y, v_z\right)$, and one is inseparable from the other. In this particular state, a single force **F** acts on the body, and the separation of it into electric and magnetic forces is rather conditional. The expression for the magnetic force is also conditional, since for non-neutral currents (for example, charged beams) it is not clear relatively what this speed is determined (relativity certainly disappears and it is necessary to introduce locally absolute velocities).

To explain the effect of current on a moving charge from the viewpoint of a system moving with the speed of electrons, one has to invent an electric force for a neutral wire [5, Chapter 13, Section 6]. But the charge allegedly appears on the entire wire. Where did he come from? The change in density due to the reduction in longitudinal dimensions is unconvincing. For example, for a closed superconducting frame, the charge must remain invariant according to the current concepts of electrodynamics, and, therefore, the frame will remain generally neutral. To explain the obvious classical invariance of the behavior of particles in STR, one has to invent first a density transformation, then a transformation of forces and also time transformation. Is it not too difficult at scratch?

On the basis of changing some parameters only, the question of the truth or of the untruth of the expression for the energy of a loop with a current is not quite clear [5, Chapter 15, Section 2]. After all, the kinetic energy also changes if the body **independently** passes from the state with a potential energy U_1 to the state with a potential energy U_2, that is, to maintain a constant velocity **v**, it would be necessary to take away or to add energy. The mechanism of current maintenance can generally be rather different: for example, if we take magnets, then, according to modern views, their magnetism is supported by internal electrons. Can the movement of these internal electrons change noticeably because of our weak motion of the magnets? But what then would it be with the movement of the outer electrons – should it change yet more? Perhaps the temperature could change also? But all similar questions should not affect the introduced definition itself of energy!

It is also very strange to use the spatial conditions at infinity in the derivation of formulas. What significance can the behavior of anything be so far from the given particular place? And at what instant this behavior should be taken into account (yes, no one even takes retarded potentials in the derivation of integrals – it is infinitely difficult). Especially when it is dealt with the potential energy of the body or field, which is associated with specific places – the initial and final ones. In fact, one wanted to get rid of the long-range theory and suggested a field theory, but it turned out that this becomes not the short-range theory, but the "theory of interactions at the infinite-range distances"!

The change in the sign of spatial quantities in 4-vectors, in 4-gradient [5, Chapter 25, Section 3] was invented artificially only in order to satisfy the Lorentz transformations, and since the latter relate only to fields in empty space, the applicability of these definitions is limited. Feynman emphasizes that the law of conservation of charge is fulfilled in all frames of reference, but does not emphasize that the charge itself according to STR is changing, which is obviously meaningless: the wire or current will not rupture with (imaginary) length contraction, and charges will be distributed uniformly; neither the number of electrons nor the number of protons will change (there is no birth or annihilation of the particles themselves); the summary charge from an individual electron or proton is assumed to be invariant, and hence the description of the appearance of an additional charge density is meaningless, because it was artificially invented only to “explain” the mutual transition of different forms of forces (electrical and magnetic) during the transition from one system to another. This simply shows that exactly such a description of the separation of a single force into two forces with artificially invented transformational properties is internally contradictory.

Problems with the local nature of conservation of energy [5, Chapter 27, Section 1] are present in the theory of relativity only. For example, in classical physics, if two bodies have a finite mass, then the speed of both bodies (that is, the kinetic energy of each) will change at the interaction, and the same potential energy can be attributed both to the position of the 1st body (that is, is linked with him), and to the position of the 2nd body, depending on which of the movements we are interested in. The statement that the local conservation law is derived for arbitrary volumes [5, Chapter 27, Section 2]:

$$-\int_V \frac{\mathrm{d}u}{\mathrm{d}t} dV = \int_V (\nabla \cdot \mathbf{S}) \mathrm{d}V + \int_V (\mathbf{E} \cdot \mathbf{j}) \mathrm{d}V,$$

and therefore it is possible to remove the icons of the integrals – is also not entirely true. For example, if we take as a volume V – the variable volume $V(t)$, then it will be seen that additional terms appear, and the result will be different. In fact, the proposed derivation implicitly assumes a non-self-consistent fixation of the volume (by all forces of the Universe), which already limits the processes under consideration only to those, at which the interactions of matter, radiation and the measuring device do not affect the characteristic working volumes at all. The accounting only the quantity $(\mathbf{E} \cdot \mathbf{j})$ at the same time moment – does not take into account possible hysteresis phenomena (delay of the response of the substance) and also limits the scope of applicability of the solution (it is impossible to create our world only with the help of electromagnetic forces – there exist also other forces and properties).

Feynman tries purely formally [5, Chapter 27, Section 3] to derive the expression for the energy density and its flux from Maxwell's equations (to simply fit the formula to a beautiful form and to name the appropriate terms in the required way). Besides the elementary objection that such expressions are not unique, there is another objection. In Maxwell's equations, one cannot interchange the cause and effect (for example, the existence of fields does not imply the guaranteed presence of currents, many electrodynamical mechanisms do not allow inversion), that is, in fact no mathematical sign of equality presents there (but rather an "arrow" exists or "reason causes a consequence"). Also the currents are taken from the Lorentz force there, that is, it is a current from real material moving particles. In fact, again we have the simple postulation of the quantities (especially since the experimentally obtained expressions are not directly verified).

When considering the energy of the field of a charged particle [5, Chapter 28, Section 1] Feynman takes rough models and shows their internal problems, but this has no direct relationship to the actual state of affairs in Nature. What actually implies the potential energy of two bodies by itself? It implies the necessity of accomplishment of work for changing their mutual arrangement under **condition** of invariability of bodies themselves. Otherwise, we must consider the energy that keeps the shape of the bodies unchanged. If we try to compress the neutral body, or, how this was intricately called "letting its radius go to zero" (with the other properties unchanged), then the body will also resist (and without integration over the non-existent field at infinity). Therefore, the question is simple: what is the real (and not convenient for theoreticians) radius of an electron? Such a value should be used. In the book, all expressions for energy are derived under the condition of the existence of **only** electromagnetic fields (which is not the case) and therefore can be considered exclusively outside bodies (until the moment of their contact). And in general, the idea was actually to collect "charged non-interacting dust" from infinity into a single particle, provided that no other forces exist. This is too big (unreal) abstraction to accurately describe the laws of Nature.

Introduction of the concept of potential difference [8, Chapter 1, Section 8] requires a definition of the limit for any adiabatic motions. It is obvious in this that if this concept is intended to be used in other sections of electricity, then the time of motion cannot be infinite. The proof of the potentiality of electrostatic forces is illusory, since similar reasoning (through the perpetual motion machine) could be applied to any other forces (especially when adiabatic movements) and this seemed to indicate the potentiality of any forces, which is incorrect. The question consists in that how the **real** test charge affects real charges (internal processes in them and changes in their motion) – but this must be determined by experiment.

One clarification about the constancy of the potential inside the conductor [8, Chapter 1, Section 9] – its accuracy is determined either by the excess of electrons (for example, imagine that there are one, two, N electrons for a non-neutral conductor), or by the total number of free electrons (there exists a limit on the maximum value of the “neutralized” field). For electrostatic shield under sharp convex-concave forms of protection, local charge distributions may differ from the average. The very introduction of the potential of one “object” is not entirely correct, since this value depends not only on itself, but also on its environment.

The introduction of potential – is a purely mathematical hypothesis (for convenience of calculations). The concept of the line of electric force is too conditional to discuss the need for a physical meaning for this concept and its physical properties [8, Chapter 1, Section 10]. These lines can start on charges and end both in infinity and at the finite point – for example, in the middle point between two equal charges, and then move in a symmetry plane perpendicular to the line connecting the charges (despite the fact that charges are absent in the middle point, everything looks as if these lines emanate from there).

Generally speaking, both the concept of the potential and the Poisson equation cannot be considered more rigorous than the expression for the potential of a point charge. Therefore, a long, purely mathematical derivation [8, Chapter 1, Section 12] of the integral

$$\int \frac{\varrho \mathrm{d}V}{R} + \int \frac{\sigma \mathrm{d}V}{R}$$

does not have much physical meaning (it is simply the sum of the potentials of the points). There remain all the same open questions about the behavior of functions near and inside the charges (on continuity, finiteness, and the form of the function itself as $R \to 0$); besides, it is added a completely unnecessary knowledge of the behavior of the potential at infinity. Wait, say, before you determine the malfunctions in your radio, I have to fly to the Aldebaran-star to find out the local behavior of the potential from your chip No. 33. The proof of the uniqueness of the real potential in the physical (and not mathematical) plane has the same degree of non-rigor.

In practice, we cannot experimentally know neither the charge density, nor the field, or the potential at each point. Therefore, many questions remain at the mercy of faith. Even the **electrical** potential of the conductor (and its constancy) is a certain hypothesis, since experience gives the constancy of the total potential, but inside, under the action of the field, it can arise the “induced EMF” compensating for the variation of φ. Probably, the polarization of the electrodes (reduction of the electrolysis current) is a manifestation of such the effect. The only reliable condition in the formulation of the problem can be the total charge of each conductor. It is not rigor at all the determination of the jump in the potential on the surface or when considering a double electric layer [8, Chapter 1, Section 14] (the potential is authentic **far from the final layer** only).

The energy of interaction for point electric charges [8, Chapter 1, Section 15] (more precisely, its change) can be physically determined too only away from the charges themselves ($R \gg r_e$). But even with the introduction of a finite (volume or surface) charge density, the finiteness problem does not disappear, since it is physically meaningless to consider the problem of moving a charge to a point where there is already some charge (and still consider them unchangeable in this case). An attempt to introduce localized field energy with the help of mathematical transformations [8, Chapter 1, Section 16] is also not entirely correct. For example: 1) the potential is physically determined only to within a constant, which cannot be seen from the formula; 2) the limiting values of all quantities near the charge represent a hypothesis; 3) we are loaded with superfluous knowledge about the behavior of the field at infinity (for $R_s \to \infty$); 4) the equality of integrals over the complete infinite volume does not at all imply the equality of integrands (otherwise many "absurdities" can be "proved"); 5) the accounting for self-interaction in this field energy – is methodologically incorrect.

The expression of ponderomotive forces at the point of location of a charged surface (or charge) [8, Chapter 1, Section 17] through the field energy at this point is a hypothesis. The determination of ponderomotive forces from the expression of energy is very limited [8, Chapter 1, Section 18], since in reality for finite systems the change in one of the parameters always leads to a change in a number of other parameters (such as the charge distribution, the distance between charges, the potential induced by the "micro EMF" of the sample, etc.), and to consider that everything is fixed except one parameter – is an academic task. And for electrostatics there must be $\mathbf{v} = 0$. Investigation of the stability for the system of charges [8, Chapter 1, Section 19] (or of an individual charge) relies only on the R^{-2} dependence, which is a hypothesis for small distances. Also, the condition that the first, second, third derivatives are zero, and the fourth derivative is greater than zero, is not refuted by the fact that the number of conditions is greater than the number of variables (since some conditions may be mutually dependent for some special disposition); therefore, a more rigorous proof is needed. Interestingly, could the Earnshaw theorem be "confirmed" by ionic crystals (at $T \to 0\,K$ they should, apparently, explode!)?

For the case of the presence of dielectrics, the notation of the energy of the field (the work) in the form [8, Chapter 2, Section 30]

$$W = \frac{1}{2}\int \varrho\varphi \mathrm{d}V + \frac{1}{2}\int \sigma\varphi \mathrm{d}S$$

with ϱ and σ for free charges – is simply the definition of the letter *W*, since even when the work of the field forces is deduced when moving free charges, the expression $e_1\varphi_1 = e_2\varphi_2$ is based on the unknown properties of the quantities at small distances from the charges (since integration is used), but the work of the field forces when moving dielectrics cannot be found in general form at all. Thus, the formula for energy (more precisely, for the letter *W*) – is simply a postulate. Fields at infinity do vanish not for all charge configurations. This is one of the reasons limiting the possibility of introducing energy density. In addition, the introduction of a volume energy density is not in any way related to short-range or long-range effects, since the difference should be consisted in the presence of a delay, but from the formula for w

$$w = \frac{1}{8\pi}\mathbf{DE} = \frac{\varepsilon}{8\pi}E^2$$

it is not at all clear which time argument should stand there (more precisely, we see that only the current moment t presents, i.e., there exists no(?!) delay, otherwise it is not clear by what such long-range-acting surfaces we again will surround the charges). The absence of rigor of the reasoning in the derivation of the expression for energy also follows from the discrepancy of this quantity to the experiments for the variable fields (here simply modern electrodynamics was caught by the hand).

The expression for the energy density of the field in the form

$$w = \frac{\varepsilon}{8\pi} E^2$$

methodically seems strange, because force, on the contrary, decreases in the presence of medium. If we consider the field itself, it is clear that in addition to the incident wave there are waves reflected in all directions, that is, we have $(E + \sum k_i E_i)^2 \geq E^2$, where k_i are the wave reflection (scattering) coefficients. What we must understand in reality under elastic energy is also a question, since dipoles can be not quasi-elastic (there exist anharmonic oscillations) and transition to time dependence (accounting for reaction and relaxation) cannot be made in a general form. Thus, we simply have some unknown set of energies (total), and the quantity $w = \varepsilon E^2/(8\pi)$ – is simply **postulated** equal to the free energy of the electric field in dielectrics [8, Chapter 2, Section 31] (then this is faith, and no concealing pseudo-grounds are needed). And also there arises the question of practical measurability and the correspondence of this quantity in real **limited** dielectrics (not infinite). That is, the expression w is approximate, but not fundamental (for example, is it not necessary to normalize $E^2/(8\pi)$ to a volume free from the dipoles themselves – renormalize?).

Ponderomotive forces in dielectrics are deduced only in model assumptions. We list some of them. Even in the “general method” [8, Chapter 2, Section 32] it is assumed the smoothness of all quantities, homogeneity (the absence of a geometric factor means that all sizes are infinite); we also recall all the uncertainties in the dependence of the quantities at small distances. Not at all the conditions and distributions of fields or charges, surface integrals turn to zero. The energy of the field immediately includes some unknown elastic energy (variable). The medium is considered incompressible (without tangential stresses), that is, for liquid and especially solid dielectrics additional substantiation is required. The reduction of the volume forces to tensions implies the continuity and boundedness of the quantities.

It can be pointed out that leads to restrictions on the degree of generality for the introduced tensor of electric field tension [8, Chapter 2, Section 34]: the smallness of the sizes of the measurement region (averaging over large distances) – is not up to microscopic values; independence of the used ε from the real limitedness of the dielectric (from the influence of boundaries) and its shape (geometric factor).

The expression for the interaction of the two elements of currents looks very suspicious [8, Chapter 4, Section 43]. First, the law of equality of action and opposition is not fulfilled. And if we start to tend to $R_{12} \to 0$? After all, in this case the total force must obviously go to zero. Hence, the entry for the elements of currents is incorrect. Secondly, it is strange that there is no interaction for elements of currents on one axis. Perhaps, Ampère’s expression more corresponds to reality (any “disagreements” can be tried to be attributed to the spreading electromagnetic field).

As for the expression of the field or ponderomotive forces: questions arise both for linear and volumetric currents [8, Chapter 4, Section 44], regardless of whether we consider these results to be theoretical or experimental. After all, the moment about interaction of the nearest elements of a current (and also carriers) is not clarified. From the theoretical point of view: how to go from the written expressions, if we take them as exact theoretical microscopic values, to the observed fields and currents (we need to take into account self-consistent changes in the motion of the current carriers and the effect of additives to the external field from neighboring current elements)? If, on the contrary, these results are considered to be rigorous experimentally, then all the quantities in them are **effective** self-consistent ones. And how then should a true expression for the elements of a current be extracted from them?

Since in electrodynamics all the quantities depend on the distance R, especially strongly – at $R \to 0$, then the notations of the average expressions [8, Chapter 4, Section 45] should be more precise: $\langle \mathbf{j} \rangle = e\langle n\mathbf{u} \rangle$. Similarly, there is not the slightest reason to consider the Lorentz force notation as strict for small R. Experiments [12], [14], [15] are more likely to support the notation of force in the form of an Ampere force for magnetic forces. Then the contradiction exactly disappears when the work is accomplished by magnetic forces in the electric motor (generator). Further, the Hall effect is a macroscopic effect and quantum mechanics has nothing to do with it. The presence of positive Hall coefficients for some substances indicates an incorrect definition of electromagnetic forces. Further, the verification of some dependence should assume that all the parameters entering into the equation are already determined in an independent way. Since the values of **v**, **R**, self-consistent fields **H** and sin(**v**,**H**) are strictly not measured, then the determination of e/m is only approximate.

The conditions for potentials at infinity – are not physical, but purely mathematical ones, stipulated by this specific particular method of description (of solution). For example, you can draw a potential that behaves differently at plus and minus infinities, although the field there will tend to zero (Fig. 3).

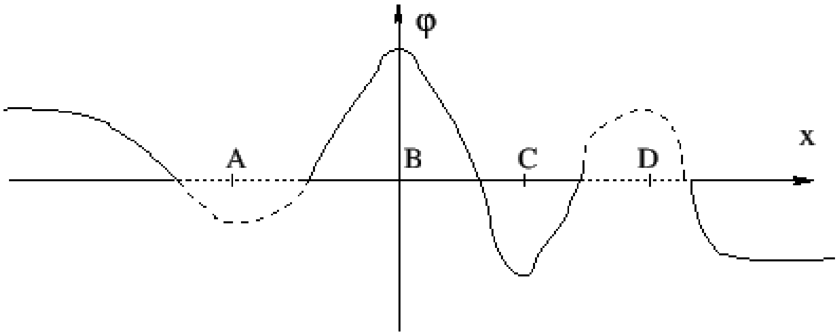

Fig. 3: Behavior of potential and field.

In all six "points": $-\infty$, A, B, C, D, $+\infty$ the field is zero, although the potentials are everywhere various and different from zero (and by the displacement of the origin, it can be made equal to zero at only one of these points, but not at all points at once). In reality, the solution at a point should not at all be any appreciably dependent on conditions at large distances from this point. Why, for example, should we not consider conditions at a large distance be corresponding to fluctuational characteristics (which may even depend on spatial and temporal coordinates)? Next, consider the following situation. Our Earth can be considered as an infinitely remote "boundary point" for any planet from another galaxy. But do we everywhere have no fields, and potentials are equal (to each other and) to zero? But how can we practically separate that part of the potential that is only for that galaxy and is supposedly close to zero from all the other, often huge, fields and potentials? Thus, the real conditions at infinity are unknown and remain only the faith (that required for the chosen beforehand mathematical formalization of the problem only)! Therefore, the significance (advertised) of the transition from fields (already ready solutions) to potentials (with the addition of unknown boundary conditions to the differential equation) is highly doubtful.

Obtaining **rigorous** relations (equations) with using specific boundary conditions (for example, at infinity) and with transition to integration over a remote surface is also highly questionable, since in reality such conditions **are never strictly met**. The introduced surface characteristics [8, Chapter 4, Section 49] and the equations are not realized in Nature (but only represent some choice of the mathematical limit). And the dependence of the supposedly rigorous equations at the local point (!) on the behavior of the function being **calculated** at infinity (on the rate of its decrease) characterizes the theory not from the best side (it demonstrates its boundedness). The behavior of the potentials (**A** and φ) at infinity in reality cannot be limited to artificial conditions like $AR < \infty$, $R \to \infty$ (this limits the range of problems – see Fig. 3 – where is the universality of the Maxwell equations?). Derivation of the characteristics for infinite systems (for example, an infinite solenoid) occurs only through "spells", since in the Maxwell equations, their derivation uses the thoroughly different behavior of the quantities at infinity.

When calculating the work of ponderomotive forces of a magnetic field [8, Chapter 4, Section 50], the question arises: how these forces, acting through the flow of electrons and moving the entire mass of the conductor, do not at all affect the current **j** itself? Additional undisclosed questions concern the role of possible deformation of the contour and accelerated final motion of the system, which also can change the current strength. The expression of work through a change in magnetic flux is of limited nature. First, for unipolar induction (both the generator and the motor)

$$\mathbf{H}\delta\mathbf{S} = 0,$$

although the effect presents (and should be described uniformly, that is, algorithmically). Secondly,

$$\delta\Phi = \int_{\Delta} H_n \mathrm{d}S$$

does not at all correspond to a change in the magnetic flux through the contour, since Δ is the lateral surface of the "cylinder" that the contour depicts during its motion at **different instants of time** (although numerically this value coincides with the real value for an undeformable translational motion of the contour at $J = const$ and in neglecting the change in the magnetic field **H** itself).

When calculating the ponderomotive effect of currents and the coefficient of mutual induction [8, Chapter 4, Section 51], limitations are also used: the absence of influence of currents on the field **H** (self-consistent), rigid "fixation" of the current strength **J**, the absence of deformation and accelerated motions of the contours and their elements. That fact, that the principle of equality of action and counteraction for closed currents preserves, seems a miserable justification not only because of the above-mentioned limitations of generality. Indeed, in reality, the "continuous" current **J** is actually composed of **individual** electrons, and "noncompensation for the elements" will have to be experienced by individual electrons. For example, suppose one pair of diametrically opposite electrons rotates around one positive center and the other pair of electrons – around another positive center located on the same axis of the cylinder, but the second pair is turned 90° relative to the first pair. Since when moving at the angle of 90°, the law of equality of action and counteraction is allegedly not satisfied, then the second pair experiences acceleration without additional expenditure of energy (is that a perpetual motion machine?).

Electric and magnetic fields, in fact, express two "parts" of the same field – the field of interaction of charges (**conditionally** divided into the interaction of resting charges and of currents – moving charges). The problems for the classical interpretation of the spin of an electron arise **only** when the conditions of the theory of relativity are imposed; and quantum mechanics "succeeded" – not to explain, but **to describe** some phenomena by introducing artificial postulates. The ratio of the force due to the electron spin to the Lorentz force [8, Chapter 4, Section 58]

$$\frac{F_{sp}}{F_L} = \frac{h}{4\pi mv}\frac{\nabla H}{H}$$

testifies that the accounting a spin is required at low speeds of movement; also this accounting is necessary at small distances (for example, in an atom, since all the fields vary substantially at these distances).

It is impossible not to say “a couple of unpleasant words” about the many “artificially born” systems of units in electrodynamics. Anyone who has ever tried to make numerical estimates or practically verifiable calculations (but not just finished working only with the manipulations of theoretical physics with “mathematical symbols”) has certainly had to encounter with a heap of various conversion factors and substitutions in formulas. Such an artificial situation should not last forever. And the whole matter lies in the infinite pride of relativists, on the one hand, who have spawned a lot of systems of units (to demonstrate that they can be invented a lot, and they are allegedly equal), and, on the other hand, zealously defending the absolute system of units and advertising it in theoretical calculations instead of the international SI system. It should be noted that the advantage of the SI system in practical matters is obvious. First, historically, all research and discovery was done just according to measurements in practical units. Secondly, at the present stage, too, any measurements, verifications, devices (appliances) are produced (graduated) and controlled by practical units (and for theoretical excogitations, they are forced to introduce recalculational functions and coefficients). Thirdly, we finally come to what is so attractive to relativists in the absolute system of units? The letter "c", written explicitly in the formulas (without invoking the physical meaning of the formulas themselves)! But this letter is related **only** to the speed of propagation of electromagnetic interactions in a **vacuum**. Even for a vacuum, this hinders the constructing of light propagation models, and for real media its explicit writing is completely superfluous (knocks into a single rut), since the generalizations of the Maxwell equations or the wave equation in the medium depend on the specific physical mechanisms of interaction in the medium and of waves with the medium. Thus, the number of "generalizing" coefficients and of the ways of their inclusion in one or another term of the equations can vary. The choice of the absolute system of units completely unreasonably programs the boundedness of the choice of models and of the system of equations in the medium.

The following remark once again concerns the accepted ideology of finding solutions using boundary conditions. What relation can the boundary conditions somewhere very far have to the process under consideration? In fact, starting from a certain distance *R*, the decreasing fields and forces for any solutions (allegedly exact functions) become indistinguishable from each other within the existing accuracy of measurements and can turn out to be much smaller than actually observed fluctuations of these quantities in nature. Apparently, the cause and effect are confused: the state of the source determines the fields and forces at a large distance (if there were nothing else), but not vice versa.

The notation of the density of force (as of the Lorentz force) is a postulation, otherwise it is required the possibility to independently determine all four (!) vector quantities and two scalar quantities. If we believe in the rigor of Maxwell's differential equations, then "the closing equation" for the force (since the fields in themselves only are the letter abstractions and do not express anything) - is an expression different from the Lorentz force (see [13]). If we believe in the rigor of Maxwell's differential equations, then the "closing equation" for the force (since some fields in themselves do not express anything – letter abstractions) is an expression different from the Lorentz force (see [13]). In addition, it is strange to postulate the same form for both macroscopic and mean values: both $\mathbf{j}_{\mathrm{micro}}$ and $\mathbf{H}_{\mathrm{micro}}$ vary in a vast range on microscales, and also the average of the product is not equal to the product of average values in the general case. In addition, any inhomogeneities of the medium change μ (quadratic oscillation effects are again possible). Generally speaking, the derived formulas are applicable only to the case of a **uniformly homogeneous** medium ($\mu = const$). Therefore the measurement of the quantities [8, Chapter 5, Section 65] **H** and **B** in parallel and perpendicular slits in the dielectric is not strict, but merely a postulated **definition** of some new quantities close to those required.

When in [8, Chapter 5, Section 75], ponderomotive forces acting on a magnet in an external magnetic field are calculated, then possible changes in the properties of the magnet itself in this field are not taken into account (and the total field should then be determined somehow self-consistently). In general, it is strange to calculate **measurable** forces through the **postulated** field energy. As a result, a density of forces appears (even two different expressions with different values!), which can actually be another one in reality, and only the integral value with it makes sense. This is obvious in advance, since it is possible to extract microscopic interrelations for elements of current from the **measurable** macroscopic relationships by an infinite number of different methods with different results. Why so much chicanery to generate, if we do not advance at all from the integral laws discovered before the 20th century?

In the “derivation” of the written form for the energy of the magnetic **field** [8, Chapter 6, Section 81], a number of doubts arise naturally:

1) the case $[\mathbf{HA}] \rightarrow 0$ is faster than $1/R^2$ does not cover all possible cases;

2) all potentials are defined only up to an additive constant.

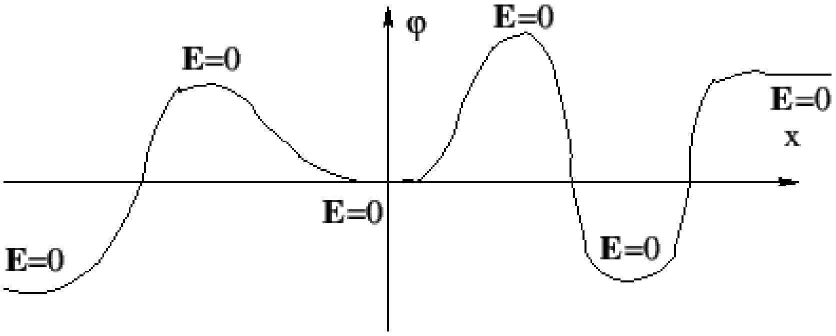

Fig. 4: Field and potential.

For example, in Fig. 4 the field $\mathbf{E} = 0$ will correspond to different potentials in different regions; similarly for the vector potential. Consequently, the result should not change when $\mathbf{A} \to \mathbf{A} + const$ is replaced, and, therefore, all the decrease should belong to $\mathbf{H}$, but this already essentially limits the range of applicability of the conclusions;

3) and, finally, the density obtained as an average when integrating over an infinite surface can characterize **only the integral** magnitude of the effect. And, therefore, it does not differ in any way from the denotation form for the energy of the interaction of currents: the discarded terms in the interaction regions can contribute a comparable or exceeding contribution from the preserved terms and play an important role in the **real distribution of the local density** (as, for example, the average energy density of the atmosphere is not having to do with the determination of the energy of a particular tornado who turned a certain car over.)

It must be remembered that the separation of two parts from the single force – parts from the electric field and from the magnetic field, and also the fields themselves – is rather arbitrary. STR **does not eliminate** the asymmetry between induction from the magnetic part of the Lorentz force and from the electric field excited by the changing magnetic field [8, Chapter 6, Section 85], but simply **postulates** the equivalence of these cases (although they are clearly nonequivalent, at least for the time of appearance of the effect). The obtaining the differential form of the Maxwell equations does not mean the automatic generality of these equations, but, in fact, clearly fixes the conditions (statement of the problem) under which they were derived. The question naturally arises of the Lorentz force: does the magnetic part of the Lorentz force already take into account that part of the electric force that arises from a changing magnetic field in the path of the particle? Most likely, according to the "generally accepted scenario", this happens only with linear accuracy. The separation into fields for the case of alternating current is not entirely successful also [8, Chapter 6, Section 86] – many quite intuitive things turn out to be allegedly useless and wrong for such an "algorithm". Perhaps the division into a potential and a vortex part would be more productive and descriptive.

For moving media, in pursuit of meeting the ghostly requirements of the STR, electrodynamics rejects its more important part [8, Chapter 8, Section 110]: the connection with the real microscopic structure and micromovements. As a result, Maxwell's equations turn into a set of equations for some nothing meaning "letters", and therefore the invariance itself of these equations does not make any sense at all. The only equations (closing) that could give meaning to Maxwell's equations are introduced **as definitions** (and turn into fitting ones). Having abandoned common sense (intuitive and visual representations) in favor of STR, relativists further try to justify some elements of formulas intuitively. However, it looks rather clumsy. For example, when in [8, Chapter 8, Section 111] in expressions for polarization and magnetization (even the meaning of which had previously ceased to be defined; only the name remained!) only a part of the addendum is attempted to be explained intuitively, and to seek "the other part" is sent to the STR. The worth is a penny for the such science-like deceptions (this is how any imaginary desired effect $b + c + d$ can be extracted forward from any value of a, and the "remainder" contains all the “rest”: $a \equiv b + c + d + (a - b - c - d)$. In this regard, for such quantities, which are introduced as "definitions", it is also strange to refer to "experimental data", because they describe only part of the effect (intuitive!). But "additions" of STR has not been verified.

“Explanation” of unipolar induction [8, Chapter 8, Section 112] also does not cause satisfaction. First, the “material” contour is nevertheless obtained strictly open-ended and it is not clear why we supplement it with a new “material” segment. Secondly, it turns out that the flow is continuously increasing. But this is nonsense! After some time, the circuit will begin to make full turns, and it would be possible to bring the flow to an arbitrarily large value. And if there were no closing wire (only a rotating magnet)? What a senseless increase in magnetic flux? Or the reason is the wire? Too ridiculous! And if instead of a permanent magnet there would be a solenoid with a constant current, and we would turn it off instantly – there would have been a gigantic change in the magnetic flux, which essentially depended on the time of the previous work of the unipolar machine (but no one observed this!). Third, the experiments of J. Guala-Valverde can be interpreted as evidence of the reality of rotating fields (or the presence of Ampère force), that is, inertial properties of the electromagnetic field, which STR cannot in any way allow (many things immediately become elementary, and STR interpretations are meaningless). From the classics, without any STR, the component E_r in the rotating magnet (and in the conductor) elementary follows.

Coulomb’s law [6, Chapter 1, Section 1.4] was not verified strictly at very small and very large distances. In general, just point charges do not exist, but only charged particles of finite sizes with a magnetic moment. And, in essence, Coulomb’s law – is a definition of the concept of “charge”. Even the statement about charge additivity is based on the assumption that one charge remains unchanged when another charge approaches. The principle of superposition always needs experimental confirmation and cannot be used as a strict principle (but only as an approximate method). When checking the form of the Coulomb law k/r^2, there will always be a question: what is being tested – the constancy of the proportionality coefficient k or the exponent of degree "2"?

It is unlikely that the introduction of the concept of "energy" can be called [6, Chapter 1, Section 1.5] "penetration into the essence" – this only simplifies a number of calculations, since just the integral of motion is used. When calculating the energy of a system of charges, firstly, it is considered that our charges are already "somehow assembled into a single point". And, secondly, it is believed that their characteristics (for example, the sizes on which their "own energy" depends) do not change when these charges approach each other. All this cannot be absolutely rigorous. Unfortunately, calculation using commonly accepted formulas cannot explain the stability of ionic crystals (for example, NaCl) – potential energy does not have a minimum, so again there are doubts about the rigor of conventional electrodynamics. Of course, the introduction of the concept of the field allows us to simplify the calculations, since we focused only on the behavior of the selected charge. However, it turns out to be justified **only** for a fixed configuration of all other charges, otherwise different test charges would produce different changes in the system, and all values would have to be recalculated "from the zero" in the same manner too.

Coulomb's law is more informative than Gauss' law [6, Chapter 1, Section 1.10], since, in addition, it explicitly identifies spherical symmetry (assuming that it strictly exists) that does not automatically follow from the Gauss law for the flow (therefore, in the second case, the symmetry must be artificially searched **where possible** and added to the Gauss' law). The writing the law of Newton is correct: the magnitude of the gravitational constant G (local!) may be not constant in time and in the scale of the Universe. The recording of the Coulomb's law in the SI system is also preferable than the choice of units of measurement for charge, which eliminate a similar coefficient in the GHS system. After all, all experiments have so far been carried out under terrestrial conditions, and there is no evidence that the local values obtained are "global constants" at the spatial and temporal scales of the Universe (for example, there are etheric concepts, where a set of constants may vary somewhat with vortex flows of ether).

It is just ridiculous to "build on the student assignment" clearly inoperative "models of an electron" for the sole purpose of equating its electromagnetic energy with the postulated value of mc^2 from STR.

One more phenomenon demonstrates the insufficiency (non-self-sufficiency) of electrodynamics itself: chemical reactions obviously have an electromagnetic nature, but, nevertheless, the charges in galvanic cells move against the electric field, and we are artificially forced to consider EMF as external. Also, it does not follow from any general principle that the potential (having no independent physical meaning) must be assumed to be zero at infinity – this can be either any constant or non-decreasing function (for example, a periodic one or in general waves – "zero oscillations") and even an infinitely increasing function (but slowly increasing, so that field $\mathbf{E} \to 0$, or tends to a constant), since we do not know the real conditions at infinity at all (and where to look for such an "empty" infinity in our only non-empty Universe?!).

For the force acting between the wires, it would be better not to insert a sheet of metal to "prove" the absence of charge, but to shield at least one of the wires from almost all sides (better two wires).

When E. Purcell says [6, Chapter 5, Section 5.2] that the expression

$$\mathbf{F} = q\mathbf{E} + \frac{q}{c}[\mathbf{v} \times \mathbf{B}]$$

is a definition of **B**, then it looks like a real “state of affairs”. Indeed, the definition does not give anything new: for all (!) $\mathbf{v}(x, y, z)$ you need to know q (open question: how to make this measurement in motion?); you need to know $\mathbf{E}(x, y, z)$ (open question: does the experimentally measured field change when moving?); it is necessary to measure the force $\mathbf{F}(x, y, z)$ (it is not yet known how to do this for a moving object!). As a result, we will get some “well, very valuable” function **B**, and wasn’t it easier to measure immediately what we really need? At the same time, it is actually **postulated** (this is a modern choice and nothing more) that the effect of the electric field does not depend on $\mathbf{v}(x, y, z)$. It is impossible to measure the amount of charge in motion: at rest, the charge is measured by the Coulomb force, but if we measure the charge in motion by force, then we will be forced to postulate the noninvariance of the charge! To **postulate** charge invariance, the author [6, Chapter 5, Section 5.3] utters a “plausible science-like spell” about the need to measure the average force acting on a large number of infinitely small trial charges distributed over the surface of the sphere. In fact, it is just a **choice of a new definition** for the value of the field **E**, which is postulatively not dependent on speed. Naturally, the following questions remain open: is this particular choice the **most convenient** for the chosen method of description, and is Nature **really** constructed like that? There are no direct experiments and evidence (and all the more “exhaustive” ones [6, Chapter 5, Section 5.4]) that the charge thus defined is invariant. Not to mention that it is even difficult to imagine how a mathematical integral can be **measured** over a surface fixed in a certain system.

Comparisons of the neutrality of different atoms and molecules are too (!) coarse and artificial: these are not just systems differing in particle speeds, as stated in the book by E. Parcell. First, **all** speeds vary and they are **coordinated** with each other (but not independent for different systems). Secondly, **each** of these systems **not simply mechanically** consists of **the same** charged subsystems (particles), but according to modern views has different **sums of masses**. As a result, mass defects are expressed, for example, in radiations that are carried away when a system is formed, that is, in some of the systems there is absent "something" (what this "something" is expressed is not known for certain – is it $+q_1$ and $-q_1$ or equal ratio q_1/m_1 and q_2/m_2, etc.?) and comparison with **another** system without such knowledge is already inadequate. When comparing the optical spectra of isotopes, also not only one parameter changes, but several at once! And the insertion of the "imaginary" experiment [6, Chapter 5, Section 5.4] (never performed) about the mythical change of mass during movement is completely superfluous. Yes, and the mass spectrograph does not prove a relativistic change in mass (if only because the speeds in such systems cannot be directly measured in experiments), but simply fixes another mass for another object (the mass difference went to radiation!). But the statement of the STR that "the magnitude of charge in different moving systems should be the same," let us keep in mind (we will return to it when discussing magnetic forces). In order not to discuss field conversions based on the STR (shortening of lengths), we refer to [2] (see also http://www.antidogma.ru), where the fallibility of STR is shown.

Some inconsistency of the expression for the field of a moving point charge [6, Chapter 5, Section 5.6]:

$$E' = \frac{Q}{r'^2}\frac{1-\beta^2}{(1-\beta^2\sin^2\theta')^{3/2}}$$

consists in the fact that as $\theta' \approx \pi/2$ and $v \to c$, we have $E' \to \infty$ (and the force tends also) up to large distances r' (you can choose such the parameters!). It is also strange to describe the switching of lines (dependence on the past history) as in [6, Chapter 5, Section 5.7]: if the charge is quickly stopped, then at a distant point it turns out that the charge still moves further (the instantaneous direction of force always indicates the **mathematical** direction to the charge!); and it turns out that he should then go back to the real stopping point?

To pretend that if Coulomb's law and STR are correct (and if not?), then magnetic phenomena "must" exist [6, Chapter 5, Section 5.9] – mockery of students (magnetic phenomena are known as an experimental fact even long before the creation of an electromagnetic theory and certainly do not depend on later speculations and "explanations" of relativism theorists). In the subsequent consideration of the symmetric current in the wire, the "explanation" of relativists about the appearance of a charge in a moving system simply terrifies. First, the real experiments are done with finite frames (no game at infinity), but then we recall that the total charge in the STR must be invariant (we have a contradiction of the STR to itself). Secondly, the equality of fields from positive and negative charges **is postulated**, regardless of the **motionless of the one** and the **movement of the second** (after all, the electric field is considered **only from an excess** charge!). Third, if the electric force was zero in one system, then it should also remain the same after the "relativistic" transformation in the other system. Fourthly, and if one of the wires (let us now have two frames with currents) is slightly charged with such an excess charge, so that with such relativistic transitions into its own system of charges, the total field (from this excess charge plus the allegedly occurring relativistic field) becomes exactly equal to zero? Indeed, in a laboratory system of reference, such a charged (non-neutral) frame will not interact with the "uncharged" one, but the magnetic field and interaction (classically discovered before the appearance of the STR!) will remain the same. However, when viewed from the standpoint of the STR, an embarrassment is obtained: now there should be no forces in the moving system! Fifthly, such “relativistic leaps back and forth” were invented only in order to fit the solution with the help of a scientific spell up to the historically well-known answer of the problem (i.e., with the “back mind”).

When calculating the ponderomotive forces, it is considered that matter and electricity are continuously distributed in space (which in reality is not quite so). That is, this means that some averaging was carried out, but for average values

$$\langle D^2 \rangle \neq \langle D \rangle^2,$$

$$\langle \frac{1}{\varepsilon} \rangle \neq \frac{1}{\langle \varepsilon \rangle}.$$

It is also assumed that the dielectric is liquid and isotropic (which is already approximate in the presence of the field), and electricity moves along with the substance (such a rigid freezing-in is also an approximation). Further, the process itself is considered as isothermal [10, Chapter 1, Section 34]. As a result, instead of honestly speaking about the consideration of a specific model of a dielectric, it is asserted that ponderomotive forces were found in a general form.

The following phrase in [10, Chapter 3, Section 49] looks as a complete deception: “The law determining the force $\mathbf{F}_m = (q/c)[\mathbf{v} \times \mathbf{B}]$ ... is obtained by generalizing experimental facts ... , where the vector **B** does not depend on the magnitude of the charge and on its movement”. Firstly, this could be **only** in the case if it was possible the **independent determination** of all quantities (from fundamentally other experiments). However, this is not the case, at least for the quantities $\mathbf{F}_m$ and **B** (which means that we are dealing not with law, but with definition). Secondly, very soon, in the STR it will be declared that the field **B** changes depending on the reference system. Thirdly, the set of expressions for force were obtained by the “generalization of experimental facts”, among them the Weber formula and the Ampere formula are most known (the latest experiments [12] argue in favor of precisely this last expression). Generally speaking, each of the “experimental” expressions has its own advantages and disadvantages.

In fact, physicists (starting from Heaviside) simply artificially **postulated** the denotation of the magnetic force in precisely this currently accepted form (all the “unknownness” was attributed to magnetic field **B**). As “a total” of this choice, we are now dealing with the writing of force in the form of the Lorentz force:

$$\mathbf{F} = q\left(\mathbf{E} + \frac{1}{c}[\mathbf{v} \times \mathbf{B}]\right).$$

And suddenly, as a result of all such “postulative discoveries”, it turns out that the values of **E** and **B** need to be postulated in the STR as dependent on the reference system, that is, as **dependent on speed**. Thus, the **only purpose** (and charm) of such a choice, namely, the division of force into the sum of two terms, one of which does not depend on speed at all, and the other one in proportion to speed, is not achieved. Consequently, the task of finding the most convenient choice of the form of fields and forces is not removed (and “all this” does not work separately, but **only in a complex!**). Experiments with individual particles are hampered by the fact that the slightest effects (measurements, for example) can significantly change the parameters of motion (recall quantum mechanics). Experiments with elements of currents are impossible in principle, and therefore only expressions for closed circuits (for example, the Bio-Savart law, or the Ampére force) can be considered reliable with experimental accuracy. The violation of equality of action and counteraction is strange: in a frontal collision, when $r_{12} \to 0$, this equality must be restoring, and this is not the case in modern electrodynamics (therefore, there is a suspicion of inaccuracy of formulas for small r).

Maxwell interpretation [10, Chapter 3, Section 66] of the phenomenon of electromagnetic induction is just **one of** the mathematical notations of **some** observable phenomena, moreover, **without** disclosing the **physical mechanism** itself. This form of recording is limited, since the contour is considered **fixed** (motionless and non-deformable). "The extracting" of a differential form

$$\operatorname{rot}\mathbf{E} = -\frac{1}{c}\frac{\partial \mathbf{B}}{\partial t}$$

from the integral record form cannot fundamentally be justified purely mathematically, since it is necessary to clarify the **physical** role and influence of the system's boundaries (starting from some small distance, this factor may become noticeable). And that that "in all cases the induction current is caused by the full Lorentz force" is only a **consequence** of the definitions introduced (of faith!). Relativists are trying at all costs to exclude the inertial properties of the fields from the discussion of the unipolar machine (otherwise all relativism will "evaporate"). Relativistic transformations of the fields – are also physically not confirmed "games with mathematical letters", since only one quantity can be measured – the magnitude of the force.

In the definition of inductance [10, Chapter 3, Section 68]

$$\Phi = \frac{1}{c}LJ$$

so it was never said anywhere how to conduct the contour inside the ring wire. Note that the quantity Φ is non-measurable one (that is, just a combination of symbols). And the reference to the next Section 69 determines the value of inductance L also through a non-measurable quantity of magnetic energy W_m.

Note that the inductance of the solenoid tends to infinity as $N \to \infty$, even if we go to the limit of the surface current $JN = const$. The properties of **specific** materials $(\varepsilon, \mu, R, \varrho)$ are always in a certain way interconnected with each other (and with external conditions), and in the general case it is impossible to artificially give certain values to individual quantities, for example, $R = 0$ [10, Chapter 3, Section 69]. The hypothesis is the conclusion that it is possible to determine the "integral of motion", called magnetic energy and independent of the method of transition from the initial to the final state. For example, if absolutely strictly, then you cannot deform the wire and its material without expenditure of energy. Yes and what a "magnetic energy" means conceptually? If this is the field energy in the whole space, then it, of course, depends on the order and method of switching on and of increasing the currents. What is declared in this textbook can take place either if by magnetic energy we mean something like the effective potential (or even kinetic?) energy of the currents, or if we introduce an artificial additional condition that the situation remains stationary for an infinite time.

The derivation of the expression for the density of magnetic energy in order to "determine its localization" [10, Chapter 3, Section 70] is rather limited. First, the entire configuration is rigidly fixed. Secondly, the cause is "hidden" because in the "proof" for the infinite solenoid, the field **B** can change only when the current $J(t)$ changes. Also the phrase is vague that "formulas (69.3) are meaningless" for rapidly alternating fields. This would be so, if these formulas in their meaning could express a certain **law**, but they express only the **definition** of W_m and therefore it is quite possible to choose such a quantity $L(t)$ so that the non-measurable quantity W_m satisfies any customary formal requirements. The "rigorous mathematical derivation" of the formula is not entirely perfect: faith in the equation div **B** = 0 (that is, in the existence of vector potential) is used, “salvatory” conditions at infinity are used, and finally, the deduction does not imply the absence of sufficiently arbitrary “zero oscillations” depending on a specific volume and giving zero when integrating over it.

The derivation of the expression, more precisely, the introduction of the **definition** for magnetic forces through energy [10, Chapter 3, Section 72] **under additional conditions** of the constancy of some parameters (already some artificialness) means that the result is limited (approximation). For example, even in a uniform magnetic field, there arises a moment of forces, and it depends on the **shape** of the body (and hence on the magnetostriction) and on μ of the substance. For ferromagnets, the introduction of the concept μ (or κ) does not give anything, since this is just a **definition** of some new unknown function (and more one equation is required for the completeness and uniqueness of the system of equations, but it is absent).

The expression for the work of field and current [10, Chapter 4, Section 84] is introduced without taking into account the displacement currents (“read here, do not read here, here is a fat spot, ...”), thermal conductivity is assumed to be zero (in the presence of ohmic losses associated with resistance!) and it is stated that all allegedly can still be in order, that is, the expression simply "should be considered as one of the postulates of the macroscopic theory of electricity”. Also it is “verbally” declared that the quantity u means the density of **all** internal energy. It turns out how easy it is to do physics! You enter some postulate, if only the number of “extra” parameters would be enough (for example, as in GTR), and then you simply look for interpretations of the resulting solutions. And it is not at all necessary that the postulates strictly follow from the experimental laws (look at the whole system of Maxwell’s equations in the modern interpretation!). However, for real calculations, practicians come precisely from experimental laws and only theorists “play with mathematical hooks" – if only the latter would be fitted. The expression for energy density, as non-measurable, remains at the mercy of the theorists' faith.

After mathematical combinations in deriving the energy-momentum tensor of the electromagnetic field [7, Chapter 4, Section 33], the asymmetric value was obtained. Some term was artificially added to this value for symmetrization, which changes nothing in the observed quantities. Therefore, either the requirement of symmetry of a tensor does not mean anything physically, or all the components of a given tensor are auxiliary quantities. If the vectors **E** and **H** are mutually perpendicular and equal in magnitude, then in the declared pseudo-Euclidean metric, to bring the tensor to a diagonal form, a system is required that moves faster than the speed of light (what the relativists never allow). In addition, **after symmetrization**, it turned out that for the field the trace $T_i^i = 0$, which is further used in the derivation of the virial theorem [7, Chapter 4, Section 34]. However, to obtain the virial theorem, incompatible assumptions are also used: finite (and therefore accelerated) motion of charged particles and the absence of radiation (of energy going to infinity). Therefore, the result may be approximately applicable only to a system of uncharged particles. When trying to artificially incorporate electromagnetic forces into the resulting expression, it turns out that the energy is infinite (due to the intrinsic electromagnetic energy of the point charges) and one has to pronounce a "pseudo-science spell" about its inclusion in the kinetic energy (that is, carry out a renormalization). In fact, the same limited results were used to derive the energy-momentum tensor of macroscopic bodies [7, Chapter 4, Section 35] (and one more the assumption of isotropy of pressure, true only for gases and Newtonian liquids). Therefore, the derivation of the limiting equation of state remains also hypothetical.

Note that in the case of **electrostatics**, even the elementary Coulomb law [7, Chapter 5, Section 36] can be obtained from

Maxwell’s equations not automatically, but only (!) with the **additional** condition that the field is directed along the radius and depends only on the distance. When obtaining electrostatic energy of charges from the expression for the field energy, it is assumed that the number of charges is limited and they are concentrated in a limited area (and in fact the law of decreasing of field at infinity is already taken into account when integrated over an infinite surface!). The situation with the full energy of the field [7, Chapter 5, Section 37] was "turned inside out" by relativists: from the fact that own energy occurs to be infinite, instead of making the natural, logical and obvious conclusion that real particles are non-point (and the special relativity is inconsistent), they limit the applicability of electrodynamics itself at small distances. By this they “remove” any attempts to **solve** the issue of the electromagnetic part of the electron mass (also contradictory in the framework of the STR and requiring its cancellation). From a physical point of view, formal renormalization cannot solve this problem, as well as the simple exclusion of terms with self-action that is adopted today. After all, such procedures for the resulting dependency also do not include the **finite changes** that occur with own energy depending on changes in the surrounding configuration (for example, it would be similar to the situation when placing a ball in a pressure chamber at different pressures, but we consider the potential energy of its rubber shell to be constant regardless compressing or inflating the ball). “Finding” the radius of an electron using contradictory relativistic formulas is an unverifiable and useless speculation.

The calculation of the field of a uniformly moving charge from the viewpoint of the STR in [7, Chapter 5, Section 38] makes a depressing impression with its formal-mathematical approach and the absence of any explanation of the phenomenon's physics. Apparently, the author was even afraid to say a pseudo-scientific spells in view of their apparent defectiveness. Even from the viewpoint of mathematics, it is not clear why it was necessary to search for the potential in the motionless system, if hereafter the field (the only real quantity measurable by the effective force) is searched first in the moving system, and then they use transformations for the field and go into the motionless system. It would seem that **once** the potential had already been found in the motionless system in the required coordinates, it was possible to differentiate according to them and find the field. The result itself is also somewhat strange: $E_{\perp} \to \infty$ as $v \to c$, that is, if one charge slowly flies past near another one, then at the moment of flight at a certain distance $R_{\min}$, the acting force is finite, but if it flies at sub-light speed, then the force tends to infinity (of course, the trajectory will be without singularity, since it will be determined by the “integral” value of the force effect over a short transit time, but physically this increase in force is not clear). However, theoretical physicists have long ceased to even try to deal with the essence of phenomena, but have become semi-mathematicians interested only in playing with "mathematical hooks". It would seem that if we are dealing with electromagnetic forces propagating at the speed c (constant in any reference system), then on the first body in the motionless system, the "image" of the second body will act from such point of the trajectory from which the impact was arrived with the speed of c at the given time moment. That is, for example, for movement along a straight line

$$R \to R' = \frac{R}{1 - v/c}$$

when approaching of the body, and

$$R \to R' = \frac{R}{1 + v/c}$$

when moving of the body away. The conventional independence of the result on whether the bodies come closer or are moving away is strange from the physical viewpoint.

In [11, Chapter 1, Section 2], it is not the “energy of the electrostatic field of the conductors” that is calculated, but the energy of the field away from the conductors. As far as it can be judged by the "piece of energy" about the whole phenomenon – is a big question (for example, an energy near the surface is discarded – the energy of the affinity of charges to the surface; it is also known that with large charges the wires can explode, i.e. ponderomotive forces inside the conductors are discarded). Therefore, linear connections (capacitance and electrostatic induction coefficients) remain approximate (for weak fields). The same remark applies to the conductor energy in an external field. In addition, such a field is represented as if the field created by the charges at infinity, but in deriving the formula for energy, the other boundary conditions at infinity were used: $\varphi, E \to 0$. It is also unknown whether one can simultaneously suppose whether $\varphi_0 = 0$ for grounded bodies and also at the infinity $\varphi_\infty = 0$?

When considering unipolar induction in [11, Chapter 7, Section 63] it is very strange that the effect was not explained in terms of a fixed wire (the mechanism is clearly absent! But in this one does not even try to see the question posed by Nature). Instead of studying physics of the phenomenon they formally make a transition to a system associated with a magnet (then allegedly the wire moves in a magnetic field). At the same time, **the non-equivalence of systems** is not even noticed: the original system was inertial, and the system of the magnet became non-inertial (rotating)! On the face – the fitting of mathematical “letters”!

Thus, such important electrodynamics’ concepts as energy and electromagnetic force were turned out for verification not as strict, consistent and practical as expected, but rather academic games with mathematical hooks.

Chapter 7
Radiation of waves

Consider another important section of the modern theory of electromagnetism, associated with the emission of waves, and analyze it for severity.

Initially, Feynman “proves” plausibly that, first, the waves must be transverse, completely arbitrarily excluding the longitudinal component, and, second, the waves can be represented as a superposition of plane waves [5, Chapter 20, Section 1]. But hereafter "suddenly" it turns out that this was not a common, but a particular solution, since there exist also cylindrical solutions and spherical waves. Generally speaking, such reasoning is not mathematically rigorous, since the general solution must always include sources and depend on their geometry.

In [5, Chapter 21, Section 2] the authors artificially (purely mathematically) introduce a point source, without even physically discussing what such a curiosity is. For example, for the vector potential we would have to introduce the concept of a point current. And what is it? If it is a moving particle (non-existent pointlike particle), then it does not stand in the same place, but moves: why then the partial differential equation is written? But in a similar source for a scalar potential, the charge seems to be standing still. The solution of the wave equation with a point source is all the same found incorrectly, since the potential $\varphi = S\,(t - r/c)/r$ **identically** satisfies the wave equation **without a source** everywhere, including zero. Therefore, in fact, the book declares that the equation should be without a source, and at zero it should not be considered at all with the source, but simply equate the potential to the potential of the source. But even in this case, plausible reasoning again fails, since for small r

this formula (numerator) can be honestly expanded into a series of r and we can get the limit:

$$\varphi = \frac{S(t)}{4\pi r} - \frac{S_t'(t)}{4\pi c}.$$

To where arbitrary fluctuations of the potential with time are thrown away (because they give fields in the general case!)? And, moreover, a solution with a source could be obtained in many other ways, for example, by adding $\varphi = \varphi_0 + St^2/(2c^2)$ or $-Sr^3/6$, etc.

Feynman has been trying very long to justify his formula (see (21.1) in [5, Chapter 21, Section 1]) for the electric field of a moving charge. Herewith, he considers the allegedly slow non-relativistic movements and then neglects the delay, then takes it into account [5, Chapter 21, Section 4]. The result was completely lax arguments. What does “slow motion” mean if it is necessary to compare the first and second time derivatives? How, in general, when differentiating by coordinate (partial derivative!), could the additional time derivative appear?! After all, this is not a complete derivative! Obtaining the **E** field via an additional (!) artificial condition of calibration instead of **direct** calculations is puzzling. It is also not clear why when he took the partial derivative with respect to the coordinate, an additional derivative with respect to time appeared, and when it integrates (the inverse procedure) with respect to time, nothing happens with the coordinate. The whole conclusion clearly smacks of fit, and all this just to state that the solutions are **very similar** to each other. Doubt is also caused by the inaccurate coincidence of the result with the law of Bio-Savart. What to consider in this case as an established experimental fact? And what is being checked? In our opinion, the answer for the physicist is obvious.

According to Landau, elementary particles in the theory of relativity and field theory should be regarded as pointlike. This, of course, is an abuse of authority, since experience has a prerogative over the demands of any theory (and even more so, the crazy hypotheses of relativity). Feynman, on the other hand, honestly makes an attempt to derive the Lienard-Wiehert potentials, considering the charge as distributed one [5, Chapter 21, Section 5]. All ideas here are purely classic! Therefore the next discrepancy with modern relativistic views will turn out if we recall that the charge volume according to the STR should have decreased: where did the relativistic factor disappear? If we also remember that the field is created just by charge, that is, the distance must be determined relative to the moving charge, and since the distance to objects must also be contracted, then we get a complete discrepancy between the modern relativistic theory and experience. Thus, the Lienard-Wiehert potentials can have such a conclusion (Feynmanian) only if the classical physics without STR is true. For a motion of the charge at a constant speed [5, Chapter 21, Section 6], in the expression for the potential (formula 21.39), it is also obtained a strange factor $1/\sqrt{1-v^2/c^2}$, which cannot be attributed to anything (neither to the allegedly invariant charge, nor to the radius – there already presents an own factor). That is, the multiplier is taken “from the air”, without physical reasons and mechanisms.

With the influx of energy [5, Chapter 27, Section 5] inward the capacitor (see Fig. 5 below), “small” questions arise.

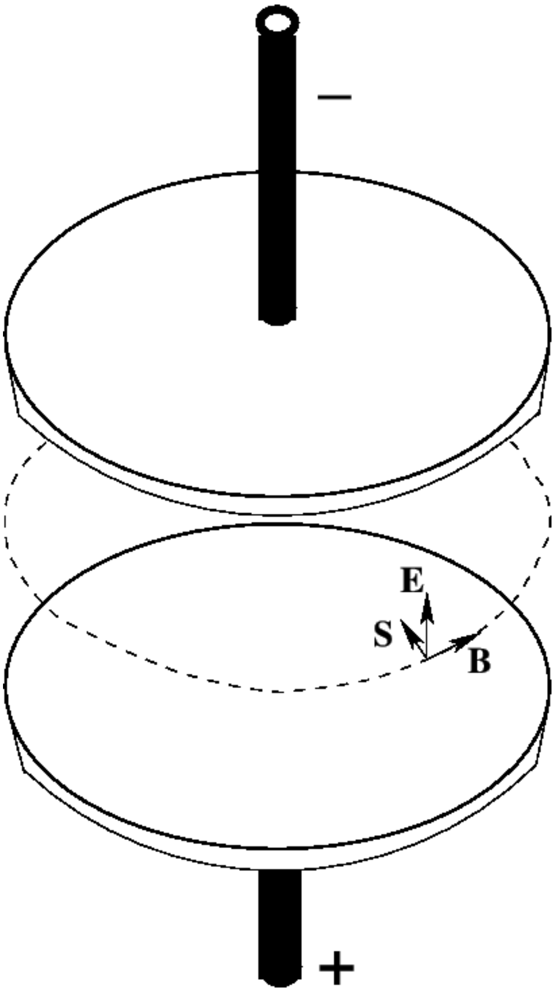

Fig. 5: Energy of capacitor.

It is claimed that energy flows to the axis. Imagine that we change the polarity constantly. The vectors **E** and **B** have changed signs, but the flow becomes still directed inward to the axis (for example, this would have happened even without external sources near zero simply due to fluctuations!). If there is a vacuum inside the condenser, then where does the energy flowing to the axis “settle” all the time and what is it expressed in? With a solitary wire, too, there are questions. The motion of particles in the fields is inconsistent with the shown diagram of the direction of the vectors **E** and **B** (recollect the drift). As a result, energy, even under the assumption of the correctness of all formulas, does not inflow inward the wire from the outside, but the flow has a component along the wire, just as prompted by "primitive intuition". In addition, it is not clear what will happen with the influx of field energy from the outside, if we shield the capacitor or the wire? And what will happen if the charged currents will be present? Yes, and methodologically, "the spreading of energy of distant charges over space and its inflow inward" – is strange for neutral currents. The circulation of energy around the static combination of a charge and a magnet is nonsense, since it is not clear in what this energy is circulating. Indeed, according to (allegedly equivalence of mass and energy) the STR, this should be a movement of mass, and besides – the accelerated movement (centrifugal acceleration around the circumference)! To where, then, did the radiation disappear (again according to modern theoretical views)? But what can emit in this case if the stationary magnet and the stationary charge do not interact with each other? Momentum of field [5, Chapter 27, Section 6] was introduced postulatively; and if all the particle velocities – are different spatial vectors, then there arise many questions, in particular, to Einstein’s conclusion about the magnitude of the field momentum.

The discussion on the momentum of field of a moving charge [5, Chapter 28, Section 2] is not related to Nature, but to some particular theory; as well as the discussion of the share of electromagnetic mass in the total mass. The question is simple. Does an electron experience collisions with truly neutral particles? Only if an electron does not collide with them at all (although it is not easy to determine), then its mechanical mass equals zero, and the whole mass is of a purely electromagnetic nature. And the expression for energy [5, Chapter 28, Section 3] through mc^2 – is an unverifiable hypothesis (needed only in

STR); and to make adjustment of the electromagnetic, and especially of the mechanical mass (or of Poincaré's stresses from "unknown" forces) to this expression – is "the hypothesis of turbid water". Generally speaking, the requirements of relativistic invariance are completely illegal and limit the possible choices for the theory of electromagnetism and for theories of other fields (for example, nuclear); most likely, these requirements simply exclude from the search the only true solutions in Nature. Well, the invariance of point light flashes in a vacuum has nothing to do with anything other than just with electromagnetic fields and only in a vacuum!

Several methodologically incomprehensible moments are also associated with the radiation energy [4, Chapter 28, Section 2; Chapter 29, Section 2]. First, if we introduce the concept of energy flow, how can we separately consider the dependence of the field on $1/r$? After all, near the charge there are other “parts” of the same field, but with other dependencies. Well, if the field energy already “moved away” from the charge, then where would this piece disappear afterwards? Whether it again "settles", "grows", or transforms into something? After all, the energy must be maintained when moving in a vacuum. Secondly, the proportionality of the field energy to the value of E^2 is actually taken from the classical kinetic energy of the test charge, proportional to v^2 and from the proportionality of the velocity to the magnitude of the field. However, 1) the charge moves not just in the **E** field, but also in the **B** field and the motion dependencies are more complex; the charge will not even oscillate around a fixed point (for example, a resonance with a wave may exist in a medium); 2) Why should we take the speed of trial charges $\mathbf{v}_0 \equiv 0$? Is this an absolute coordinate system? If you select the distribution $f(\mathbf{v}_0)$, then again all dependencies become more complicated. Third, let take two identical field sources at some distance. Then, depending on the ratio of the phases of radiation at a certain distance, the field turns out to be zero. But after all, if one source would be closed, the flow of energy from another source would pass through this point. So did the energy move away from the source or not? If the field energy flow is an independent entity, could he know that another field is flying to this point from another remote source? How, then, are the two (almost codirected) non-zero energy flows giving zero? In general, the principle of superposition ideologically works against the field approach: it shows that the field is not independent, and for a trial charge at a given point also depends on the location and on mutual movement of two sources. This is an obvious **potential energy of interaction**. And why can't it depend on $(t - t_i)$? And finally, if the energy of a moving charge can "go away", then why does the charge not disappear?

STR refused the ether and considers vacuum as an emptiness; how then can we talk about the impedance (resistance) of a vacuum [4, Chapter 32, Section 2], which is equal to 377 ohms? Yes, because it is detected, but the STR – is just a fantasy of an inflamed mind. When calculating the radiation energy of a charge during acceleration, Feynman is cunning [4, Chapter 32, Section 2]: although acceleration is included in the formula, taking into account delay, he says that it is wrong to assume that energy was radiated at that moment, and therefore it is necessary to calculate the average square of the acceleration over a period. How is it possible! After all, in STR at any movements of a source (and in the classics at sublight movements), if the wave "flew off" from the source, then the latter will not catch up with it and will not change its contribution to the radiation (one can always draw a sphere, from where the wave could arrive at this time to this receiver).

As already indicated, the mathematical expression for the energy density is written only approximately, and the real value **at a given point** may differ from the conventional record. The generally accepted notation for the Poynting vector **at a given point** is also approximate. In addition, only an integral value can be approximately verified, and the differential value can be changed to an arbitrary vortex function ($\operatorname{rot}\mathbf{f}$). The concept of Poynting vector [8, Chapter 7, Section 92] turns out, to put it mildly, to be strange in some cases:

1) For permanent magnets and motionless charges, you can "make" an arbitrary flow of energy moving along one or another characteristic trajectory (for example, in a circle) and which does not manifest itself in anything concrete.

2) For a wire, it is necessary to assume that energy “flows in” from the surrounding space radially, and in the EMF region it “flows out” radially, but there is no energy flow **along** the wire from the second region to the first one (how does the energy get there?).

3) For propagating light, it is suspicious that from the Maxwell equations, the fields **E** and **H** **simultaneously** reach both the maximum and the minimum: what about the analogy widely used in textbooks about the conversion of energy from one form to another one? The flow of energy turns out to be changing, but not constant – as if it turns out that not the source (light bulb) constantly radiates energy, but energy spontaneously appear and then disappear. And at the point where $\mathbf{E} = 0$ and $\mathbf{H} = 0$, there absent energy flow at all!

The assumption that when an electromagnetic field propagates through a medium, its properties (ε, μ) do not change at all – is only an approximate model concept. In fact, the wave itself slightly modulates the properties of the medium through which it propagates. The "explanation" of the question of why the energy "does not have time" to penetrate deep during the skin effect, looks naive when leans upon the viewpoint of the Poynting vector. Indeed, both in a constant and in an alternating field, the Poynting vector is **always** directed along the radius from the outer region to the center, that is, the energy always “flows” in one and the same direction and it would be worth explaining why the energy “almost does not reach” to the center of the wire.

When calculating the oscillator field [8, Chapter 7, Section 98], author found an “intermediate zone” where an approximate solution can be applied. It is only necessary to take into account that, firstly, we assume in advance that the solution (series) converges and, secondly, that omitting of terms leads to a result that is close to the true solution only in order of magnitude. There always arises a final inaccuracy, and the real **functional** behavior may differ significantly from the behavior of the solution calculated from the first members of the series. As a result, it may turn out that the differentiation of this approximate function (for example, in the presence of higher harmonics) gives functional dependences that are very different from the true ones. Of course, the oscillator field in [8, Chapter 7, Section 99] is not exact, but only approximate; naturally, there can always exist ω or λ, for which this solution is not applicable (the conditions under which the answer was obtained, do not satisfied), and for decomposition in harmonics (Fourier), restrictions may also exist.

The derivation of equations for plane waves [8, Chapter 7, Section 100] in the dielectric relies on the constancy of ε and μ, as well as the absence of ϱ and $\mathbf{j}$. In reality, $\varrho \neq const \neq 0$, but it is a rapidly oscillating function. Similarly, in the zone of passage of the ray, the value of $\mathbf{j}$ is also a rapidly oscillating function. And the solution with the simultaneous achievement of maxima by both vectors $\mathbf{E}$ and $\mathbf{H}$ – is no more than one of the possible models. Whether it really so - it should be set by a separate experiment.

All physical theories – are only a “hundredth” faint reflection of reality (no more than an approximate model); and the theories of long-range action and of short-range action [8, Chapter 7, Section 109] – are two extremes. Abstract “charges” do not exist, but there exist real particles that possess certain specific properties. Therefore, the Faraday-Maxwell “thickening of lines” simply is no longer valid, as a mathematical abstraction (not to mention the fact that the pure field concept was not developed in the final form and is not used). Even mechanistic theories do not prohibit spreading disturbances at a finite rate. So far, the hybrid theory has turned out to be more practical, and experience shows that the fields exist only in the presence of charges or / and currents (that is, material carriers). The very form of notation of the dependence only on the quantities at the moment t still does not speak about the instantaneity of the process. It is always possible to represent some ("**no matter which**") **curve** $\mathbf{r}(t)$ (that is, a curve depending on **arbitrary** derivatives) as a solution of some equation

$$m\ddot{\mathbf{r}}(t) = \mathbf{F}(t) \, ,$$

and you can always enter definitions and re-notations

$$\mathbf{F}(t) \equiv \mathbf{F}_1(t, \mathbf{r}) \equiv \mathbf{F}_2(t, \mathbf{r}, \mathbf{v})$$

$$\equiv \mathbf{F}_3[t, \mathbf{r}(t), \mathbf{v}(t), \dots, \ddot{\mathbf{r}}(t), \dots] \equiv \mathbf{F}_4(t - t_0) \, ,$$

and none of the notations in itself speaks of either long-range action, instantaneous action, or delay. And all equations (both integral and differential in partial derivatives) have the same experimental base, and, therefore, their degree of generality is the same (and in some cases they are also “loaded” with additional conditions!). All the difficulties with the question about the localization of the energy of static fields remain the same. The question of determining the exact expression for the energy and momentum of a field for rapidly varying fields is also ambiguous.

In [10, Chapter 4, Section 83] electromagnetic waves are “explained” by the example of the motion of an infinite plane with charges fixed on it (it turns out to be $\mathbf{E} \perp \mathbf{B}$). In the case of a bounded object, the relative position of the force lines becomes more complicated, since the **E-**lines – are diverging straight lines, and the **B** lines must be closed (oval in shape with increasing tension with distance). The relation $\varepsilon E^2 = \mu H^2$ also looks strange, that is, the electrical energy is equal to the magnetic energy at any moment. We are not talking about average values! Namely – about instantaneous values (which contradicts the very concept of waves)! Thus, one energy does not go sequentially into another one (as was the case with kinetic and potential energies in waves), but at some point it is maximum, and at some point it is zero. And in these propagating points, it always remains zero. The question arises, if at one point all the energies (and fields) are zeroed, then how can they “rise from nothing” at the neighboring point? Here it "does not smell" by the field approach at all (but rather all looks like ordinary particles)!

In [10, Chapter 4, Section 85] Sivukhin attacks on the SI system, which the practicians (experimenters, engineers, etc.) chose long before theorists. Note that the CGS system **does not fundamentally** (from the viewpoint of physics) **differ** from it. Maxwell’s equations in SI look simpler, but relativists cannot accept the absence of the letter “c” in the equations. Next, without doubting Sivukhin prescribes what in the equations is allegedly omitted, which of the quantities make sense, and which of them do not, but all this remains a matter of faith so far (but in fact, all the quantities entered are artificially introduced for the model description and none of them is measured directly without interpretations of the theory). Therefore, no (!) principal shortcomings of the SI system simply do not exist! The identifying of the quantities of **E** and **D**, and of **B** and **H** in a vacuum – is only Sivukhin’s faith (numerical coincidence of some values under some specific conditions has never guaranteed the identity of concepts themselves!). All this does not negate the unity of the electromagnetic field in the SI system at all. It does also not exclude that it is possible (with some different set of quantities) to describe electromagnetic interactions in another manner. In addition, the SI system allows you to make changes into the laws, if such a need is discovered in experiments (since it also contains the mechanisms of phenomena), and the Gaussian system is already rigidly defined (the system of axioms of faith), and all subsequent additions can be "above built" only *ad hoc*.

When describing the radiation of the Hertz dipole [10, Chapter 10, Section 141], the type in which the vector **D** is sought, is chosen rather artificially:

1) it is limited to only three types of dependence - on $\mathbf{p}$, $\dot{\mathbf{p}}$ and $\ddot{\mathbf{p}}$;

2) it is assumed that the solution can always be decomposed over the components **p** and **r**;

3) when the author limits possible dependences on **r**, it is assumed that there are no dimensional constants ($[\mathbf{r}_0]$, $[\mathbf{p}_0]$, etc.), but this may not be the case;

4) it is assumed that no boundary or initial conditions exist (the pointness of a dipole also implies this!);

5) the introduction of the dependence on $t' = t - r/v$, although understandable in terms of explanation, but in practical terms implies knowledge of all the laws of body motion (that is, the solution of this **additional** problem), which is inconvenient. For electromagnetic waves, it is strange that a wire mesh [10, Chapter 10, Section 142] can strongly interfere with the passage of waves (it would seem that the attenuation should depend on the ratio of the area, that is "closed" by wire, to the total area). Perhaps another explanation could depend on the mechanism of transformation for the magnetic component of the field.

For the Lecher system in [10, Chapter 10, Section 143], without using the system of Maxwell equations (!), the final velocity of wave propagation is obtained. And it does not matter at all that this is the spread of the “state of electrization” (or voltage, current), simply in *before*-Maxwellian physics, the concept of the field was not introduced at all (and anyway, we are forced analogously to evaluate the entered field only by its actual impacts!). And for coaxial cylindrical wires, the usual speed of wave propagation in free space is obtained. And the “excuse” looks very funny, that without using the “fundamental” (for the physics of propagation mechanism) displacement currents, the same result was obtained.

When deriving formulas for the density and energy flux, the authors of [7, Chapter 4, Section 31] quite arbitrarily (voluntaristicaly) change completely different types of differentials with each other

$$\frac{\mathrm{d}}{\mathrm{d}t} \leftrightarrows \frac{\partial}{\partial t}$$

(when this seems to them “computationally” more convenient). In addition, when investigating the field in electrodynamics, plane electromagnetic waves are often considered. But in this case (of infinite harmonic oscillations) the energy flux does not even decrease at infinity (and in the general case, the integral over **any** limiting surface always gives the total energy from the outgoing waves, which is not zero). Therefore, in the general case, the formulas that were derived by "throwing out" the integral terms at infinity, are incorrect in principle (and, at best, they are only approximate).

When using multipole moments in expansions of physical quantities, they are usually limited to a finite number of moments. Moreover, in modern electrodynamics [7, Chapter 5, Section 41], they are limited only to decomposition (or comparison) in radial dependence, but attention is not focused on the influence of angular dependence. For example, even for $\sum e_a = 0$ and $\mathbf{d} \neq 0$, the series expansion cannot be limited by the dipole moment for angles near $\pi/2$, since then $\mathbf{dR}_0 \to 0$, and the next (!) term of the expansion must be taken into account. Similarly, for forces and energies there are limits of applicability of the obtained approximate expressions in angle, and the conclusions when using such expressions become limited.

It is strange that when "solving" different tasks, **various** additional artificial conditions are often imposed on the potential. So, when obtaining the wave equation (for the three-dimensional case), it is assumed that $\varphi = 0$, $\operatorname{div} \mathbf{A} = 0$, and after then this looks ridiculous when we are “persuaded” [7, Chapter 6, Section 46], as if in a four-dimensional form, the Lorentz calibration is the best (since the latter is invariant!), although it is clear that it does not lead to an unambiguous solution! It is even more suspicious that Maxwell's equations, derived using the property of a decreasing field at infinity, are trying to directly apply to non-decreasing at infinity fields, for example, to plane waves [7, Chapter 6, Section 47] (from Maxwell’s equations they write the field, potentials, energy density, Poynting vector) or apply to infinite structures (of planes, flat or cylindrical capacitors, etc.). Also, when calculating the “natural oscillations of the field” [7, Chapter 6, Section 52], equations previously derived using conditions at infinity are used. In addition, the wave equation is applied. But a little earlier in the decomposition of the electrostatic field [7, Chapter 6, Section 51], the potentials did not satisfy this equation: it turns out that "what we want then we choose" (peeping in the answer?). The same situation is presented with the boundary conditions (standing wave or traveling wave?): what we want, then we obtain. And the very purpose of introducing this decomposition is incomprehensible: to determine the force influence (the only measurable) at some point, you need to know the field at this point; but in the proposed decomposition you need to know an infinite number of coefficients (and it is not known beforehand whether it is possible to be satisfied with their finite number and which).

In the derivation of Lienard-Wihert potentials [7, Chapter 8, Section 63], the authors proceed from faith in empty space (the principle of relativity): when choosing potentials relative to the particle itself. Further, the faith is used that the potentials should be 4-vector. Next, it is not proved that among the potentials satisfying the passage to the case $\mathbf{v} = 0$, the selected type is the only one (in fact, you can introduce as many as you like). The solution also raises questions: there is a term proportional not to $1/R^2$, but to $1/R$, that is, slowly decreasing at infinity (therefore, there exists nonobservance of the conditions of decreasing at infinity to those assumptions under which the basic equations of electrodynamics were derived); the field **E** (that is, the force) tends to infinity for $\mathbf{R} \parallel \mathbf{v}$ and $v \to c$; it can be seen that at $\dot{\mathbf{v}} = 0$ the field does not pass to the field of an uniformly moving charge (for example, $E_\perp \to 0$ for v → c, while for uniform motion we have $E_\perp \to \infty$). In the spectral decomposition of retarded potentials [7, Chapter 8, Section 64], it is not clear why to search at first the potentials from the values of ϱ and **j** (step back), if you can just look for the fields themselves. For expansions (for example, of the Lagrange function [7, Chapter 8, Section 65] in powers of v/c), we should not focus on the identical degrees of v/c from the terms, but on the identical **summary values** from different summands. For example, due to the presence of angular dependence, it will be necessary to use different expansions – depending on the angles.

In fact, by introducing potentials in modern electrodynamics (and equations for them instead of field equations), we increase the order of differential equations. If we analyze rigorously, then in the same measure we need to add additional initial and/or boundary conditions. The imposition of calibrations or other additional conditions implies a similar procedure, only its role is not completely clear: whether the uniqueness of the solution and its correspondence to the physics of the problem are preserved when instead of clearly verifiable matching with experimental data on the boundary, dependencies are simply introduced from some formal considerations (equations). It is noteworthy that even in single-type tasks (without specifics of boundary conditions or configuration) various additional conditions are often introduced: for example, for the field of the system of charges at long distances [7, Chapter 9, Section 66] and for plane waves (otherwise the angular dependences will differ). Thus, the theory has a clearly non-algorithmic character. Next, when comparing with the experiment, it is necessary to take into account that individual acts of radiation are practically not perceived, and only the statistical characteristics of the radiation are detected during collisions of a stream of particles [7, Chapter 9, Section 68]. When finding approximate expressions for radiation, the angular (not spherical) dependence of radiation is actually not taken into account (it is decomposed only in R), therefore, for some angles, the solution is not valid. It is strange that with $e_1/m_1 = e_2/m_2$ there is no radiation [7, Chapter 9, Section 70]. But after all, positronium eventually turns into two (or 3) gamma rays? But, on the other hand, atoms do not radiate (although this condition is not fulfilled for them). Thus, one can hardly agree that modern electrodynamics adequately describes processes of radiation.

In [7, Chapter 9, Sections 70-72] the authors receive formulas for effective (in fact, total) radiation (and intensity), although their meaning is not fully understood. This is not radiation **emanating** from the system (since nearby the system the field is another); its distribution over the angles will differ from the angular distribution both near the system and far from it, therefore, the integral value is not strict. It is not clear how to determine this radiation experimentally (if it is not at the point where the receiver is, but “smeared over space”): run with the “net” at large distances from the system, catch all the waves and integrate them? And would not the specific mechanism of interaction with the receiver affect the **measured** value?

Upon deriving the expression for the radiation of a fast moving charge in [7, Chapter 9, Section 73], the transition from the case of a particle at rest is used, which can be done only for a single particle (that is, it is a two-body theory – a particle and an observer). It is postulated that it is necessary to rewrite the expression in a four-dimensional form (again, blind faith in the STR) and that there should be a passage to the limit of the case of a resting particle. But from such a state (point) it is possible to draw an infinite number of dependencies. Thus, there is no proof of the uniqueness of the decision and the fact that it corresponds to Nature. It is noteworthy that the scalar expressions obtained contain too many variables: $\mathbf{v}$, $\dot{\mathbf{v}}$, $\mathbf{R}$, t, $\mathbf{E}$, $\mathbf{H}$. This means that the movement of the charge, given “manually”, is considered here. For example, when self-consistently solving the equations of modern electrodynamics (even in given fields), it is impossible to pre-set that speed is always perpendicular to acceleration: due to wave radiation and energy loss, this mutual arrangement can occur only at a single time moment. A similar remark concerns the very formulation of the problem with the introduction of the concept of “magnetic-bremsstrahlung” radiation [7, Chapter 9, Section 74]: despite the radiation, the particle continues to move in a circumference (that is, this movement with a constant external inflow of energy, which is spent on radiation). When they interpret applying for particle beams, it is necessary to take into account the change in the magnetic field from the particles themselves and the addition of waves from different particles (averaging).

To the theory of light and modern views on it there are a number of issues noted by Viktor Moroz (see his articles on the website http://www.antidogma.ru/library/katalog/). Light depends only on the source, but not on the receiver, that is, the theory would have to be non-potential. The coincidence of the velocities does not prove the electromagnetic nature of light (the current in the metal also “comes” at speed c, but it is not light!). The theory of light according to Maxwell – is a theory in dielectric (only displacement currents are taken into account), Ohm’s law is not taken into account, but, for example, the electrolyte is a good conductor and would have to be opaque, but this is not so!

On the question of whether according to electrodynamics should emit an electron moving in a circular orbit (Z.I. Doktorovich 1994). It is known from experiments that the quality factor of a dipole is less than one (this is a good emitter), and the quality factor of a circular turn is more than a hundred (this is only due to losses in it). That is, the kinematic decomposition of motion into two components does not give a decomposition of the final dynamic result into two corresponding results!

That circumstance, that you can enter [5, Chapter 8, Section 5] a certain coefficient m in the expression of energy E is in no way connected with whether this quantity m (even if we call it "mass") is a source of gravitational attraction. For example, according to modern STR interpretations, the mass of the cumulative radiation depends on its summary momentum, that is, gravity can strangely appear, disappear, jump, etc.

Strange is the factual postulation that allegedly in vacuum the wave front is always perpendicular to the direction of propagation of the light beam (for example, to “explain” the aberration in STR). And the isotropy of space has nothing to do with it (does not require such a postulation): the direction of the emitted light already determines the preferred vector, and the directions of movement of the observers can be determined relative to it.

Regarding the theoretical explanation of Cherenkov radiation in [11, Chapter 14, Section 115], some questions arise. In the interrelation $\omega = k_x v$, it is assumed that the particle moves uniformly through the medium, that is, does not radiate (and more precisely, does not interact with the medium), but what is the mechanism of such a process without interaction? The theoretical differences of this radiation from bremsstrahlung one (all limiting transitions) are based on an unrealistic assumption about the *pointness* of the particle. Initially, it is said that the **medium itself** radiates, and then suddenly from the total energy loss of the **particle** into radiation, they extract that fraction which is associated with Cherenkov radiation. So, what radiates?

Thus, we see that with the description of such a key phenomenon as radiation of waves, modern electrodynamics has a fairly large number of problems.

Chapter 8
Passage of waves through mediums

Let us now consider in more detail some applications of the modern theory of electromagnetism. We begin with a description of wave propagation in media.

To call the speed of light propagation through a substance c/n as “seeming” [4, Chapter 31, Section 1] – this is to put the invented theoretical principles of STR above EXPERIENCE. Propagation of disturbances in the medium is also a consequence of electromagnetic forces, but the speed of light (isotropic) has nothing to do with it: experience gives the propagation of disturbances with the speed of sound $(v_s \ll c)$ and often it is the non-isotropic propagation. And the explanation of the delay through the phase shift is half-hearted, since the moment of switching-on the field does nowhere included in the harmonic solutions from $-\infty$ to $+\infty$ (but only this formulation of the problem individualizes the wave front).

When deriving the expression for the refractive index [4, Chapter 31, Sections 1, 2], the idea of radiation by atoms of the **mathematical plane** is too model (after all, in the substance there are the most of a void space). In fact, atoms that are distributed along the entire layer $(z_2 - z_1)$, reradiate. In addition, re-radiation occurs in all directions with a certain probability, which must also be determined **from an experiment**. Therefore, the consideration of a purely one-dimensional “delay” is model and too rough. And, finally, it is completely unknown how the radiation passes **through the elementary particles themselves** (the fact that something should happening in this case is clear from consideration of the scattering of gamma quanta, if we believe that they are also electromagnetic radiation). Therefore, claims to use the “high principles of STR” are inadequate.

The experimental macroscopic Coulomb law does not guarantee a similar record form for elementary particles: for its guaranteed (proven) applicability, the distances between particles must be large compared to their sizes, and the accuracy of measuring the trajectory deviation (or location) must be high within the limits of these dimensions. For microobjects, the infinite smallness of test charges turns out to be an impracticable abstraction (as well as the absence of a change in the properties and states of charged particles in the presence of other charges - the principle of superposition). Methodically inaccurate the derivation of a jump in **E** on a charged surface: when deriving the Gauss theorem, charges on the surrounding surface are not considered, but in the given problem, as the height and area of the side surface decreases, this surface tightens to the **charged** line.

As it turns out from the calculations (Poynting vector), energy flows with the phase velocity of the wave [8, Chapter 7, Section 100]. Therefore, the phrase looks strange, that allegedly “it is necessary obligatorily to consider the wave packet”. And what, the monochromatic wave does not transfer energy (that is, it can exist without a source)? Everything former was derived under the assumption of the effectiveness of the superposition principle (of the linearity of Maxwell's equations). But in that case, using Fourier decomposition, we find that the energy of each harmonic is transmitted at a phase velocity (if we assume that a monochromatic harmonic does not transfer energy at all, then the sum of such harmonics will not transmit it also). To consider nonlinear media, a more general approach to the principles (to the fundamentals) is needed, which has not been done, so the phrase that, if there is dispersion, the group velocity should be considered to describe the transfer of energy, looks unsubstantiated (as naked phrase).

Note, in order to discuss the possibility of the existence of longitudinal waves [8, Chapter 7, Section 101], it is necessary to determine first their properties (the speed of their propagation a priori can be arbitrary) and detection methods. The laws of reflection and refraction were obtained simply from the assumption of the existence of plane waves, from boundary conditions and the constancy of the type of waves during this process (but what if the type of waves can change?). The refractive index or dielectric constants ε_1 and ε_2 are simply constants that do not follow from the theory in a self-consistent way, but, on the contrary, are to be determined. Therefore, the difference between n and $\sqrt{\varepsilon}$ means that everything is not so simple with the elementary models of a homogeneous medium, but nothing else in the Maxwell equations has yet been considered. Again, a more general analysis of the problem is required, and not just a correction (fitting) of ε values to the existing dispersion. In metal (as in any material) [8, Chapter 7, Section 102], one can consider the value of a charge density $\varrho = 0$ only as a quantity averaged over certain scales. On the microscopic scale, $\varrho \neq 0$, and in the field of the wave, naturally, there will be oscillations of ϱ (and the role of these small-scale variations must be determined). Further, all formulas are obtained **only** for the case with constant properties of the medium (ε), that is, they can be approximately applicable only **inside** the conductor, but not near its boundaries where the transition occurs $(\varepsilon_1 \to \varepsilon_2)$.

In [8, Chapter 7, Section 104] a whole paragraph is devoted to justifying obvious stupidity: continuous energy circulation (as the Poynting vector) in static fields (a charged cylindrical capacitor in a longitudinal magnetic field). However, the idea with a discharge (to turn off the **E** field) from a radioactive source to detect such circulation looks unconvincing: since free charges are produced, which in the crossed fields $[\mathbf{E} \times \mathbf{B}]$ begin to drift around the circumference around the axis, causing the gas to rotate first and then to rotate the capacitor itself. But this is a well-known explicit mechanism acting in this case indirectly, through two intermediate “chains”. But the “turning off” of the angular momentum could be carried out more simply: by switching off the external magnetic field. Is there then a mechanism for transition the system into rotation? Of course not!

When solving a number of problems in electrodynamics, some "intuitive" solutions are trying to offer for us, but a certain internal dissatisfaction is caused by the fact that not everything is intuitively obvious in modern electrodynamics (there is no algorithmic approach). For example, why the field **along** the conductor spreads over huge distances, but **inwards** the conductor it damps at the depth of the skin layer [8, Chapter 7, Section 106]? Just only because, that we immediately **chose** this type of solution? Is it really that the fast-varying currents and fields in the wire and cable are able to propagate only due to the wave in the dielectric? And at the same time they do not damp at all? Then the field practically should not change in the presence of discontinuities of the metal. Whether it is so (obviously it is not the case!)? Our **choice** of $\mathbf{j} = \lambda\mathbf{E}$ also looks strange (it does not follow from the uniqueness of the experimentally verified equality $I = U/R$ at all). Also, with damping the value of **E**, this would lead to a decrease in the value of **j**, but the quantity of **j** is similar to the flow of water in a pipe: is it really, that electrons really accumulate somewhere and create an additional charge (that means that we are not dealing with damping, but simply with changing the profile)? Or it is necessary to suppose along the wires that $\mathbf{E} = const(l)$ and $\mathbf{j} = const(l)$, but the field does not spread instantaneously (in the moment of switching on, for instance), so both values of **E** and of **j** and also of ϱ must be changeable (in the approximate theory of fast-alternating currents the charges of ρ in the intermediate calculations still appear!).

For ferromagnetic substances [8, Chapter 7, Section 108], we notice that some ferromagnetic substance in one field may differ in properties from the same ferromagnetic substance in another field. In addition to thermal (disordered) energy, ordered motions can also occur (for example, in a wave), the compression of a ferromagnetic material can occur (that is, it turns out, that we need to know a priori **where** to sew the boundary conditions together?), the interaction energy of domains can change, reorientation of structural “elements” can occur, the values of $\varepsilon, \mu, \lambda$ can change, and also properties strongly depend on the speed of the process (nonadiabatic). Thus, the proposed description is very suspicious. And also the definition of values through the Poynting vector “outside” is artificial, since we should know the amplitudes of transmitted, reflected, scattered and absorbed waves, that is, we would have to solve still the complete problem!

When describing electromagnetic waves:

1) we are “offered to believe” that all the nuances are described by Maxwell's equations;

2) an unreal “unlimited homogeneous medium without absorption” is introduced;

3) all characteristics depend only on x and t, that is, the "infinite plane" propagates "in the perpendicular direction" (but in reality we always have a three-dimensional case and a light beam is always "limited by the width and height"!);

4) it is not proved that there exist no other “propagating (or spatially closed!) solutions”;

5) the coincidence of the phases of **E** and **H** is only a consequence of our “faith” in the uniqueness of the solutions and the possibility of finding them only (!) from the Maxwell equations.

When describing the skin effect [10, Chapter 10, Section 144], firstly, it is assumed that there is no change in charge density at all (div **j** = 0) and, secondly, out of all possible solutions, it **is selected** only the one for which current flows along one axis (artificially chosen), the field **B** is directed along the other perpendicular axis, and all dependencies are on the third spatial component. Further Sivukhin honestly admits that there is no “strict and understandable” description of Tesla’s transformer operation based on Maxwell’s equations (here’s the “greatness of the theory”), but instead this he uses a description based on the theory of constant and quasistationary fields, that, of course, is unsatisfactory.

When “obtaining” the field equations in the medium from the microscopic Maxwell equations (for some reason, they are postulated to be correct, although in experiments we always deal with macroscopic parameters), an averaging procedure is used. However, the latter is unambiguous **only** in the linear approximation (when the maximum fluctuations of a quantity are much less than its average value), since in a mathematical equation we can transfer the summands and factors according to certain rules. For example, the equations $\operatorname{div}\mathbf{E} = 4\pi\varrho$ and $\operatorname{div}\mathbf{E}/\varrho = 4\pi$ are completely equivalent, but from the 1st equation it follows that

$$\operatorname{div}\langle\mathbf{E}\rangle = 4\pi\langle\varrho\rangle,$$

but from the 2nd equation (using the definition of fluctuations and decomposition into a Taylor series) we have:

$$\frac{\operatorname{div}\langle\mathbf{E}\rangle}{\langle\varrho\rangle}\left(1 - \frac{\langle\operatorname{div}\mathbf{E}'\varrho'\rangle}{\langle\operatorname{div}\mathbf{E}\rangle\langle\varrho\rangle} + \cdots\right) = 4\pi.$$

The question of **what** should be averaged, **from which equation**, **how** and what to assume about fluctuations goes beyond the scope of microscopic electrodynamics itself and requires clarification of **additional** principles and conditions.

When deriving field equations in dielectrics [11, Chapter 9, Section 75], it is not possible to strictly establish the relationship of $\langle \varrho \mathbf{v} \rangle$ with other quantities. As a result, initially, some combination is formally designated as $c\mathbf{H}$, that is, they use the same letter as for the magnetic field strength (without proof that just this quantity is the magnetic field intensity **H**). And after then they say a phrase about some “slowness of changing the field” (without any quantitative parameters of this slowness!), allegedly retaining the same dependence of **H**. In general, somewhere in the Universe such a situation with the equation could have been embodied, but the question of the applicability of this equation to each specific case remains open.

Relativistic generalization of relations between **D** and **E**, **B** and **H** (and obtaining Minkowski formulas) for moving media [11, Chapter 9, Section 76] is puzzling from the mathematical viewpoint: all curves are obtained **only** by the coincidence of the zero point for $\mathbf{v} \equiv 0$! Thus, these connections (model ones!) are not justified for the moving dielectrics at all, but without them Maxwell’s equations are non-substantive. And from the viewpoint of the measurement process, the fields are measured not in dynamics, but with the source and medium at rest. In motion, there is no procedure for measuring these fields and there is no procedure for comparing these fields with the static case. Not to mention that, strictly speaking, in the general case the interrelation should have not a simple linear, but an integral dependence (including the prehistory).

The phrase about “illegal refinement” of magnetic permeability $\mu(\omega)$ looks funny in [11, Chapter 9, Section 79]. And the “proof” of this is very strange: at the beginning, from the exact equation

$$\operatorname{rot}\mathbf{B}=\frac{4\pi}{c}\langle\varrho\mathbf{v}\rangle+\frac{1}{c}\frac{\partial\mathbf{E}}{\partial t}\,,$$

which the authors have not been able to average, they subtract the equation

$$\operatorname{rot}\mathbf{H}=\frac{1}{c}\frac{\partial\mathbf{D}}{\partial t}\,,$$

which was received only formally – by introducing some "auxiliary external charges" (which for some reason do not affect the properties of the medium at all, but only on the field!). As a result, an “excess” term $\frac{\partial\mathbf{P}}{\partial t}$ is obtained with an unknown accuracy, but it is declared that **M** has a physical meaning only when this term can be neglected. And maybe this term is just a method error, or can you redefine the value of **M**? And maybe there exist cases when automatically

$$\frac{1}{2c}\int\left(\mathbf{r}\frac{\partial\mathbf{P}}{\partial t}\right)\mathrm{d}V=0\ ?$$

Mathematically strange (even very much) is also the statement that to increase the value of rot **M**, the body size should be small (after all, the differential operation rot means taking the limit!). The choice of $\varepsilon-1{\sim}1$ is also arbitrary: since the estimates relate to high frequencies, then it must be $\varepsilon-1\approx0$. In addition to mathematical questions, there is also a physical one: why the estimates imply uniformity (homogeneity) of all quantities, but, for example, not their structuredness in scales (the presence of ordered micro-movements, spins, etc.), which is much closer to reality?

When introducing the spatial dispersion (frankly, it presents always), it turns out, nothing can be strictly derived. As a result, in [11, Chapter 12, Section 103], in essence, they refuse from the previous strong record of Maxwell’s equations (for example, they do not introduce **H** at all), but they assume that all unknown terms are included in the redefined value of **D** (that is, they essentially refuse from the former physical meaning of the latter). Next, they arbitrarily redefine

$$\langle \varrho \mathbf{v} \rangle = \frac{\partial \mathbf{P}}{\partial t} ,$$

that is, they also abandon the physical meaning of the polarization **P**. Thus, Maxwell's equations turn into a mathematical system of equations for some letters with an unknown physical meaning. The linearity of the relationship **D** and **E** turns out to be a hypothesis, especially since now ε_{ik} does not have the former physical meaning (for example, indirectly includes the dependence on μ also).

When describing the scattering phenomenon in [11, Chapter 15, Section 117], "suddenly" it turns out that the "general iterrelation" in Maxwell's equations

$$\mathbf{D} = \varepsilon \mathbf{E}$$

is insufficient (So, the previous great rigor is doubtful?), but it is necessary to artificially introduce one more dependency (in fact, an additional unknown function $\alpha_{ik}(\mathbf{r}, t)$):

$$D_i' = \varepsilon' E_i' + \alpha_{ik} E_k ,$$

where E' refers to the scattered wave, and α_{ik} to the falling wave (that is, the total field is not equal to the sum of the incident and scattered fields).

There are also "simpler" questions. Actually, why are some materials transparent to light, while others are opaque (after all, according to the modern concepts, an atom has more emptiness than of matter)? There are no fundamental differences between the atoms of different substances. What then completely not transmit the light in some cases, and in other cases causes the light to change its trajectory when falling on the surface of the body at an angle (if the photon does not interact with electron and nucleus, i.e. does not absorbed by the atom)? Why do photons interact with the surface as if it is a clearly expressed mathematical surface of the body, and not with a fairly random set of disorderly oriented and randomly “vibrating” atoms that are practically empty inside?

Thus, the applicability of modern electrodynamics to the description of wave propagation in media causes great doubts in terms of rigor and algorithmism.

Chapter 9

Motion of charges in electromagnetic fields

We now turn to the consideration of the seemingly simplest topic in the theory of electromagnetism – the description of the motion of charged particles in electromagnetic fields. This is pure mechanics, and there should be no surprises. But is it?

The separation into electrical and magnetic forces is conditional. If we take Newton's second law, then force formally may also depend on acceleration [16]. Generally speaking, why it is necessary to necessarily assume the possibility of the separation of all external parameters in explicit form? Is it possible that an entire class of implicit functions has no applying? The very conventionality of separation of the fields **E** and **B** forces us to abandon visualization, since the result is dependent on the observation system (and this is a strong constraint imposed by theory on Reality).

The mass cannot be determined physically as a coefficient of proportionality between momentum and velocity [5, Chapter 28, Section 4] (although mathematically such equality may occur to be true, for example, for neutral particles), since then mass would not be an independent physical concept, but expressed as a postulational definition through a more complex concept of momentum (the latter quantity is not measured directly, but only with certain theoretical interpretations). The biggest obscurity is the self-action of an electron, i.e. the possibility of a fairly fast self-acceleration of a charge with increasing energy to infinity. Therefore, something is wrong with the whole theory and with this “phenomenon” in particular. First, the consideration of motion along one axis (purely one-dimensional) is illegal, since the magnetic field will lead to the “untwisting” of the electron itself and to its three-dimensional movement. Secondly, the existing (measurable) magnetic moment of the electron itself is not taken into account initially. Thirdly, the tending of the radius to zero ($a \to 0$) does not allow us to calculate the limit of force, since we do not know the behavior of the higher derivatives; for example, if the motion turns out to be fundamentally oscillating (perhaps this is the way to get the physical connection between the corrected classical electrodynamics and the “quantum mechanical” effects), then the role of higher derivatives will increase, and all terms may be finite. Fourth, what does $a \to 0$ mean? Does an electron really have no its own size, if how we liked this, so we determine it?

From the table of elementary particles given by Feynman [5, Chapter 28, Section 5], the opposite to what has been claimed is visible: an additional positive (!) or negative (!) mass is in no way connected with the presence of a charge. And from what equation from which theory should “getting” (!) mass of an electron and a μ-meson [5, Chapter 16, Section 1] – this is generally obviously excessive belief in abstraction. The question remains about an experimental confirmation of the existence for an electromagnetic part of the mass (probably, in the strict sense, this is not mass, but momentum or energy associated with a specific interaction process?).

When inverse generalizations were made, and their behavior for microscales was derived from the observed behavior of particles on a macroscale, it was not taken into account that this was only average behavior. For example, the real trajectory can be rapidly oscillating. In addition, no one took into account that it is difficult to exclude their interaction from the behavior of a large number of particles (it is not known how to do this unless you know in advance the exact laws sought). Consequently, the chosen “generalization” on the behavior of an individual charged particle is one of a huge number of hypotheses.

There arise a number of methodological issues to the calculation of the thermionic current in vacuum between two plates [8, Chapter 1, Section 11]. First, it is no longer static, but motion, and therefore it is necessary to consider not electrostatic forces, but full forces and dependencies. Secondly, an assumption that $v_0 = 0$ is an unclear arbitrary hypothesis and a constraint on the regime (rather, a speed $v_0 = v_T$ is the thermal one). Third, the counting $\frac{\partial\varphi}{\partial x}$ =0 at $x = 0$ – again is an arbitrary hypothesis. Not to mention the fact that charged particles of the same sign are placed between the electrodes, which distorts the final field (and the field of the electrodes themselves), but this is not taken into account.

From the symmetry of the Coulomb force ($\sim 1/r^2$), the same (spherical) symmetry at small distances does not follow at all. For example, if particles are non-spherical or with a non-uniform density distribution, then deviations of effects from symmetry at small distances will be noticeable, but at large distances the dependence will approach spherical symmetry. That is, we can also attempt to describe the complexity of micro movements (allegedly quantum mechanical) by taking into account the asymmetric structure of the particles.

Even in the non-relativistic case, the ideology of the drift movement has oddities. First, it does not depend on mass, that is, one excess electron can cause a ton of substance to drift – a clear contradiction with observations. Secondly, for perpendicular fields, the drift velocity depends only on the ratio of the electric and magnetic field strengths and does not depend on their absolute values; therefore, for arbitrarily small fields (by themselves), it is possible to ensure drift with a high measurable velocity – also a clear contradiction with observations. But the drift velocity is a rigorous consequence of the equations for the Lorentz force (with a certain configuration of the fields). That is, again, something is wrong with the Lorentz force.

Perhaps in the experiments of Roentgen, Eichenwald and Wilson [8, Chapter 8, Section 113] it is necessary to take into account the specific mechanism of the polarization field (movement in the field of **H** and of the centrifugal force) and drift in crossed fields (Hall effect), based on the microscopic structure of matter and on the **nonjoinability** of charges. Do not hide behind the ideas of Galileo on the uniformity of laws in inertial systems. After all, it was about isolated **closed** identical systems, and not about open and interpenetrating ones. But there exist problems with isolation from magnetic and, especially, gravitational fields. The independence of the magnitude of the charge e on the motion of the reference system remains an untested postulate. It is strange that the transformation of fields (and forces) can be visually understood only for one particular case $\mathbf{E} = 0$ [8, Chapter 8, Section 115]. And also the invariance of the laws of electrodynamics has not been verified (by any means, not by Michelson’s experiment at all!). Perhaps the acceptance of the inertial properties of the electromagnetic field would lead to more understandable interpretations.

Strictly speaking, Coulomb's law assumes that all charges are motionless. But after all, the **average speed** of the body (= 0) has nothing to do with the **speed of charges** (at least, the latter is thermal speed!). And since the STR assumes the transformation of forces, then the presence of $v_i \neq 0$ leads, for example, to a **systematic** change in the magnitude of the attractive force of charged bodies as their temperature increases (and the charge is determined just precisely through this attractive force). It turns out, you need to postulate a change in charge (after all, we also do not know the structure of charged particles and their internal movements)?

Against the background of complete dogmatism, the attempt itself to portray moving lines of force from moving charges is rather remarkable [6, Chapter 5, Sections 5.5-5.7]. But it would be even more useful to pose the question (this is a hypothesis with far-reaching consequences) about the presence of inert properties of the electromagnetic field (in the spirit of Galileo).

When there are several parameters in the equation [6, Chapter 6, Section 6.1], the phrase that the fields **E** and **B** are independent of **v**, becomes speculative: in addition to the field conversion, the possibility of substituting the inverse solution must be mentioned $\mathbf{B}(\mathbf{r}, t) = \mathbf{B}[\mathbf{r}, t(\mathbf{r}_0, \mathbf{v}_0, \mathbf{v}, \mathbf{r})]$ and etc.

It is very strange to “deduce” the properties of the magnetic field, based on the fictional arrangement and properties of the lines of force [6, Chapter 6, Section 6.2] (for example, it is not known how the infinite space is filled with lines of force when the field is turning on!). Again the “extraction” of the differential part from the integral equality is not the only possible one.

The general solution obtained with the help of potentials does not always give the concrete answer [6, Chapter 6, Section 6.3], which speaks of the boundedness of the differential approach. In essence, the determination of the contribution from a separate current element is impossible uniquely (and represents a hypothesis); therefore, it is preferable to introduce an integral field through the experimental Biot-Savart law.

Point the attention that the inverse sign of the Hall coefficients [6, Chapter 6, Section 6.9] is, in fact, a refutation of conventional electrodynamics (and the all-generality of special relativity): the direction of current and its magnitude are known experimentally, the Ampere force (including its direction) can be experimentally determined also (the impulse must be preserving), also there is the certainty with electricity carriers (the metal does not “re-creep”). And suddenly there arises inconsistency both in the magnitude and in the sign of the effect! So in the microworld, electrodynamics is clearly limited in the area of its applicability (even approximated!).

A significant part of the problems of electrodynamics uses well-defined boundary conditions (or conditions at infinity), and from this point of view it is strange that when passing from the integral form of laws and equations (for example, Coulomb, Maxwell, etc.) to the differential form, it is considered that all equations become insensitive to boundary conditions (to volume). As if the phrase “arbitrary volume” automatically excludes a whole class of functions are depending on the size of the system.

The notion of the resultant force in Newtonian mechanics works well when applied to point objects. Non-point objects require additional study: see, for example, difficulties with drift motion. Determination of the specific electron charge [10, Chapter 5, Section 89] (although it is coordinated with other concepts) is based on the **choice** of the expression for the acting force (the Lorentz force with its separation into electric and magnetic components), i.e. associated with the introduced **definition** of the fields **E** and **H**. And to the determination of the elementary charge itself [10, Chapter 5, Section 90], there have always been many fair questions, since the method uses **uncheckable approximations** of all quantities to a region of small values.

The notions of electromagnetic pulse, electromagnetic energy and mass [10, Chapter 5, Section 91] were also introduced just by classical electrodynamics, why does the author of the textbook give “another regular oath” to the theory of relativity (after all, her priority does not exist here!)? Feynman honestly saw the problem in that it was **impossible** to reconcile the numerical values of the electromagnetic and non-electromagnetic masses of an electron (which means the inconsistency of modern electrodynamics!). But with Sivukhin everything is “elementary”: "what is the problem with the 4/3 coefficient?" – we simply **discard** it and **postulate** our loyalty to the theory of relativity (and all inconsistencies will have to be attributed to future postulations), although it would be more logical to consider this fact as a “failure” for STR! For the electromagnetic radius of the electron, also in [10, Chapter 5, Section 91], everything allegedly occurs to be "simple": let us reject the coefficient 2/3 and even “hide behind” quantum mechanics (to forbid to researchers in this field to even try to solve electrodynamic problems!).

The method of introducing the expression [7, Chapter 3, Section 16] for the action of a particle in the field clearly demonstrates the "fitting" nature (artificial, hypothetical) of this procedure. First, the additive nature of the action is postulated (the action for a free particle plus a term for interacting with the field). Secondly, the introduction of two characteristics is postulated: “charge” and “4-vector” (it does not follow from any general principles; as well as the absence of other members, for example, scalar ones, which is recognized by the authors of the textbook). So far, neither the unambiguity (it is absent) of such definitions of potentials, nor the necessity of just such a form of them, nor the sufficiency of such definitions for a complete and adequate description of Nature, have been proved. Further in [7, Chapter 3, Section 17], the equation of motion is deriving, **definitions** are introduced for some of the “letters” **E** and **H**, but there is absolutely no evidence that these are precisely the fields that we measure.

The problems associated with the drift (independence on m, on the values of **E** and **H** under the condition $E/H = const$, etc.) in perpendicular fields make it necessary to assume a fundamental non-rigor of other (relativistic and nonrelativistic) solutions of the equations in modern electrodynamics with Lorentz force.

It is strange when, starting from the two definitions for the values of **E** and **H** in [7, Chapter 4, Section 26], two equations are “gotten” by taking the rotor and the divergence. First, such actions can only reduce the amount of information, and not increase. Secondly, it remains a formal-mathematical operations (combinations) with symbols that are not related to any experimental laws of physics. It also surprises the fanatical desire to write in four-dimensional notation, if from it, only one "is seen that there are only four independent equations" (you might think, the same was not visible from the original pair of Maxwell equations!).

When deriving a full action and action for an electromagnetic field [7, Chapter 4, Section 27], several unjustified assumptions are made. First, the additivity of the action over particles is assumed and its representability as the sum of individual terms for particles S_m, for the field S_f and for the interaction S_{mf} (that is, S_m remains unchanged when the field is turning on, and S_f remains unchanged when real particles are introduced). In addition, it is necessary to take into account the difference between a trial charge and the real one (multiple of e). Secondly, what kind of fiction is this – a separate action for a field, if you can really determine if a field is present only when it interacts with material objects? Thirdly, the superposition principle is true only in vacuum and is approximately true for linear media, but in the general case of nonlinear media, it is not satisfied. Consequently, the requirement of quadraticity of the action over a field (for obtaining linear differential equations) is true only in a vacuum (and is it worthwhile for such a particular case to heap up so much mathematics?).

The results of special relativity and modern (relativistic) electrodynamics for movement in the Coulomb field [7, Chapter 5, Section 39] are completely unsatisfactory: if $Mc < \alpha$, where M is the angular momentum of the particle and $\alpha = ee'$, then the particle falls on the center! Even without taking into account the radiation! All of Nature (the stability of atoms and molecules) clearly contradicts this "result". It is also strange in this "solution" that a particle falls on the center **in a finite time**, but at the same time the radial component of the momentum tends to **infinity**! That is, a particle relative to the center will possesses infinite (!) kinetic energy (but the energy of the entire system will remain finite).

If the charges move in a finite region of space and have finite momentums, this does not mean that the motion will be periodic with a finite period: the orbits can be incommensurable. Then, when calculating the "average" magnetic field, the averaging time should be infinite, and, therefore, this calculation has neither practical, nor even theoretical value. The inconsistency of formulas in [7, Chapter 5, Section 43] is noteworthy: to obtain the law of Biot and Savart, they write averaging $\langle \mathbf{j} \rangle$ only, while for the vector potential they write the averaging of the entire expression. However, if in the first case to write the correct expression

$$\langle \mathbf{A} \rangle = \frac{1}{c} \sum \frac{\langle e_n \mathbf{v}_n \rangle}{R_n},$$

Then it will immediately be seen that the potential will remain a function of time. Similar remarks about infinite averaging time (and the derivation of formulas is based on averaging!) and about averaging the entire expression are also concerned to the derivation of an expression for the magnetic moment of the system. Consequently, in addition to the written expression (which is approximately true for all angles between the magnetic moment vector and the radius, except for angles near zero), there will always be variable terms as well. The same comments apply to the moment of forces.

The question of the need to take into account the charge self-action when braking by radiation [7, Chapter 9, Section 75] remains open, because for the external field all terms in the Lorentz force were introduced **by definition** (that is, with a pretense to be rigor). The use of transformation of potentials raises questions about their **physical** foundation and the uniqueness of the solution. An indirect question has not been worked out either: in some cases, why should one take into account only the **explicit** dependence on the coordinates when taking partial derivatives (which is mathematically true!), but in other cases this is “forgotten” and the partial derivative is replaced by the full derivative (that is, by the implicit dependency, for example, when use transformations). Since the system loses energy (is not stationary) during radiation, that is, it exists within a finite time T_0, then now the averaging of the finite members containing

$$\frac{1}{T_0}\int \frac{\mathrm{d}}{\mathrm{d}t}(\dots)\,dt,$$

gives only an approximate expression. The same remark applies to the average loss of angular momentum. Whether it is possible to apply the force of radiant friction to an isolated particle – is a big question.

Self-acceleration of charge in the allegedly "strict" modern electrodynamics proves lack of its rigor. And the profiteering that, because of the infinite proper electromagnetic mass, only “subtraction of two infinities” was made, does not solve the problem, but only underlines the feebleness in this matter. Therefore, the question about the possibility of quantitative application of results in the presence of fields remains open: whether the **strict** correctness of the "**subtraction of infinities**" could be restored by adding a **finite** field (which is less in infinitely many times than infinity!). In the task to [7, Chapter 9, Section 75] about the fall of two attracting charges on each other, the attention draws to itself that if

$$\frac{2|\mathcal{E}|M^2}{\mu\alpha^2} = 3,$$

then the change in energy

$$\frac{d|\mathcal{E}|}{dt} = 0,$$

whereas the moment change is present

$$\frac{d\mathbf{M}}{dt} \neq 0$$

(here μ is the reduced mass, $\alpha = |e_1 e_2|$). Thus, the accuracy of the result and the area of its applicability remain unknown.

In the case of high speeds [7, Chapter 9, Section 76], an attempt to generalize the obtained non-strict expression for the force by one formal reduction of the expression to the 4-vector and using the limit value, is non-strict also. The applicability of this type of radiation drag force causes doubts in the case when this force exceeds the Lorentz force, and the "proof" by the transformation of forces – is only relativistic faith. (On the other hand, it is noteworthy that energy losses at high speeds are proportional to the square of energy – it resembles a similar case in hydrodynamics when moving through a medium.)

According to the law of conservation of momentum, the frequency of the scattered wave for free charges in modern electrodynamics can only approximately coincide with the frequency of the incident wave (otherwise, since $E \sim \omega$, the energy would not change; but the momentum will changing!). In [7, Chapter 9, Section 78], the phrase that in the system where $\mathbf{v} = 0$ the particle does not emit, looks very strange (in modern electrodynamics). Firstly, if “a radiation was already outgoing from a particle”, then it moves at the speed of light c and when passing to any other system, that is, in fact, at any speeds $v < c$, the energy must also “flow” from the particle. Secondly, with such a transition, the charge rate $\mathbf{v} = 0$ will be observed at one time instant only, otherwise we would have to go to a continuously “jumping” (non-inertial) frame of reference, but for such a case all the conclusions of the formulas are clearly unfair.

Thirdly, it is strange to assume that “the entire momentum lost by the incident wave is absorbed” by the free particle (then we must speak of energy losses in the wave). For real atoms and molecules (consisting of several charged particles), the scattering process should be considered as a collective process.

When describing the Hall effect, the magnetic field **H** is described as an external field [11, Chapter 3, Section 22]. Methodically this is not quite true, because, firstly, for any selected electron the magnetic field created by all other electrons will also be external, and, secondly, only the summary (resulting) fields can be measured. It is simply assumed that in a limited conductor the Hall effect for an intrinsic magnetic field is already included in the definition of the conductivity coefficient. Such an approach makes it difficult to find dependencies from the first principles and limits the results only to the simplest terms of the expansion. Thus, in essence, a certain number of new unknown functions are simply introduced. Considerations based on the law of entropy increase, on the absence of $\mathbf{j}\nabla P$, $\nabla T\nabla P$ and other members in the formulas for thermoelectric phenomena are not strict, because the number of terms is greater than one, and it is not proved the absence of some their interrelationships leading to a sign-definite combination in the law of increasing entropy.

In MHD, for concordance of the number of variables to the number of equations (to write the “complete” system), the entropy conservation equation is added [11, Chapter 8, Section 65]. It looks methodically ugly and artificial, since the entropy itself is not measured by instruments (that is, consideration implies an artificial model character in advance).

So, the description of the motion of charged particles in modern electrodynamics also cannot be considered as a standard of mathematical rigor and physical validity.

Chapter 10
Magnetism

We now turn to the analysis of how the modern electromagnetic theory describe such the phenomenon as magnetism. This phenomenon has been known for a very long time, with the exception of its later theoretical expansion into micro-objects (particles), and it would seem that here the theory should be in an ideal state. We will see.

On the one hand, the textbook unfoundedly asserts that spin cannot be reduced to classical movements [8, Chapter 4, Section 58]. But, on the other hand, it is claimed that the spin magnetic moment of an electron can be reduced to the action of electric currents (but in fact, the latters express the classical motion of charges in terms of physical meaning). In fact, the separation [8, Chapter 5, Section 60] of the quantity $\mathbf{j}_{\mathrm{micro}} = \mathbf{j}_{\mathrm{cond}} + \mathbf{j}_{\mathrm{mol}}$ into the current of conduction and molecular currents means only that some average macroscopic part $\mathbf{j}_{\mathrm{cond}}$ is separated from the exact microscopic current. Description using the magnetization vector (magnetic moment of molecular currents per unit volume) means averaging over the period of intramolecular movements, and the inconsistency of the pulsations of various molecules and electron spins. In the general case, there may well be a transition to a coherent state, where all micro parameters are not random, but consistent.

Note that when determining the vector potential in the presence of magnetic substances [8, Chapter 5, Section 61], in the general case from equating integrals

$$\int \frac{\mathrm{rot}\,\mathbf{I}}{R}\,\mathrm{d}V = \frac{1}{c}\int \frac{\mathbf{j}_{\mathrm{mol}}\,\mathrm{d}V}{R}$$

for macroscopic quantities (**functions at a point!**), the equality $\mathbf{j}_{\text{mol}} = c \operatorname{rot} \mathbf{I}$ does not follow. The fact is that the integrals include the **entire volume** of the magnetic with its **predetermined** configuration. In the general case, equality of integrals does not imply equality of integrands at all. This value of $\mathbf{j}_{\text{mol}}$ characterizes the **specific given configuration as a whole** to determine the value of **A**. Since $\mathbf{j}_{\text{mol}}$ enters linearly into the expression, the same $\mathbf{j}_{\text{mol}}$ can approximately characterize some other integral **linear** dependences of physical quantities. But already for nonlinear functions (physical quantities), the substitution of this $\mathbf{j}_{\text{mol}}$ can give inaccuracies. If we take the other volume V, then this will be **another task** (again, in some cases, we can introduce some similar-in-form substitutions for it, but here there is no strict mathematical transition to an arbitrary volume!). This very general remark also concerns the derivation of the basic field equations in the transition from integral laws to differential forms. In addition, in nonlinear expressions there can, for example, be present effects of quadratic oscillations.

For nonlinear media (both dielectrics, for example, with elastic dipoles, and magnets, for example, ferromagnets), the very idea of the superposition principle becomes incorrect and we must look for equations to generalize Maxwell’s field equations (linear in principle). It would seem that the description should be unified, but the very existence of such different substances – paramagnetic, diamagnetic and ferromagnetic ones – looks strange from the viewpoint of the principles of the electromagnetism theory (different effects). Again, we see that to determine the field from differential equations, the condition at infinity is required: $HR^2 < \infty$ as $R \to \infty$. But what does the condition at infinity have to do with it, if the time to establish a single (to infinity) solution would be infinite, and from an experimental point of view, the behavior of the field at infinity becomes undetectable (beyond the limits of the accuracy of any devices)? On the other hand, purely mathematically, infinite system of currents and motions in theory could well be considered in principle; all fields, naturally, will be quite definite, but simply it will be impossible to calculate them using the generally accepted field equations – this is simply a limitation of the possibilities of modern theory. From the same point of view, the introduction of potentials (requiring the next step of differentiation and additional conditions, for example, calibration or additional boundary or initial conditions) can only lead to additional restrictions on the applicability of the theory and its rigor. And in general, the introduction of the concept of magnetic permeability for a homogeneous medium implies that all molecules, atoms and particles **within** themselves (inside an electron, proton, etc.) possess the **same permeability**, which clearly contradicts reality.

For the ponderomotive forces experienced by magnetic materials, the presented notation [8, Chapter 5, Section 66] in the form

$$\mathbf{F} = (\mathbf{M}\nabla)\mathbf{B} + [\mathbf{M} \times \operatorname{rot} \mathbf{B}]$$

cannot be simultaneously true for micro-objects and for macro-objects, because when averaged, the average of the function (of product of values) is not equal to the function of the average, and on microscales, changes of all quantities can be significant, and the homogeneity of the medium, under which the formulas were derived, is violated in microscales. Approximate expression

$$f = \frac{\mu - 1}{8\pi\mu} \nabla \mathbf{B}^2$$

suffers the same disadvantage. Therefore, when they “measure” κ and μ by the ponderomotive forces – this is simply the **definition** for the new effective values κ_{eff} and μ_{eff}, so that the effect of the invented force would be close to reality (the fitting of the form of expression for force under the theory).

Reference to the need for a quantum-mechanical consideration of magnetism [8, Chapter 5, Section 68] evidences only about the **lack of rigor of conventional** classical electrodynamics, and not about the impossibility of any other consistent classical electrodynamics. After the "description" of precession and diamagnetism, it turns out that the diamagnetic effect will be observed under the condition that the magnetic moment of the atoms is equal to zero. But in this case there will be no precession and no change in the angular momentum (try to force a non-rotating object to precess!). And with the description of the precession through the analogy with the gyroscope, not everything is in order either: the reaction of a support is needed (!) and only then the sum of forces leads to the precession (otherwise mechanical translational and wave movements are possible). Recall, for example, the Dzhanibekov effect in cosmic space where a spinning top experiences coups.

As for the law of Curie [8, Chapter 5, Section 70], it is necessary to *ad hoc* to pronounce a number of "plausible spells" about the reasons for the restrictions: about low temperatures, about "a number of substances", about other dependencies, etc. The magnetic field does not affect the kinetic energy according to the generally accepted electrodynamics; and the description of the process of changing the field through the streams (it is not known through which contours) – this is a scientific deception (fitting to the desired result): whether it is possible that with both instantaneous switching on and adiabatic switching on of the field **H** the result will remain the same?

Also in classical physics, the quantities M and $\langle R^2 \rangle$ are not at all obliged to take values from zero to infinity [8, Chapter 5, Section 71] and, accordingly, the value κ is **not obliged to be** $\kappa \equiv 0$. These are purely problems of the **generally accepted interpretation of electrodynamics**. The description of the precession under the action of the average **macroscopic** field **H** looks completely strange, because in reality it is necessary to take into account the fields on microscales, which can differ by orders of magnitude from the macroscopic field. And the orientations of all spins are also unknown in advance (therefore, the predictive power of the theory is doubtful).

In relation to ferromagnetism, there is also no predictive power, and many questions remain unanswered. Why in some substances the forces of interaction of magnetic moments are large, and in others – negligible? Why is not in all substances the electron spin is involved in the ordering (after all, a rotating top must maintain its orientation)? What does the exchange forces here? Either there exists an opportunity to create a large **self-sustaining field** due to ordering, or not, and the criterion is not to “explain” in hindsight, but to be able to separate classes of substances in advance. A phrase about the stability of the state of magnetization looks as an artificial "spell". In fact, from conventional electrodynamics, it turns out that all substances at low temperatures should be ferromagnetic. The description of the "Weissian regions" also looks like an artificial fitting for the result (why, then, does residual magnetization exist in some substances, but in others – not?). It is strange that the description of magnetic anisotropy (substantial) and magnetostriction was entrusted to a thousand times weaker magnetic interaction of atoms.

Linear Lorentz transformations and even the principle of **superposition**, of course, cannot be applied to real nonlinear media (to be strict up to "adherence to principles" and impose their limitations on all theories). The most obvious example is the phenomenon of hysteresis in ferromagnets. But for other environments and phenomena, in principle, the situation is no better: any changes are determined not only by the current state, but also by the whole **prehistory** of the process, since in real environments different disturbances (for example, in frequency) propagate at different speeds and are attenuated in different ways (and pass through barriers).

Essentially, Maxwell's equations cannot remain rigor in principle and become non-informative in fact: the coefficients of the medium become functionals (functions of the process under study itself) and the number of "unknowns" becomes more than the number of equations. And only if we artificially take the environment coefficients as given functions (that is, we either take a model mathematical problem or we already peeped in the answer we are looking for), then we can get an approximate answer (hence, for example, a model of idealized ferromagnets arises).

The concept of a homogeneous medium is strange – when the magnetic permeability must be constant both outside and inside the magnetic substances, which in reality never holds. Contrary to the unity of the description of magnets and currents stated in the theory of magnetism, even the forces behave differently: for two magnets they are inversely proportional to μ, for magnet and current they do not depend on μ, and for two currents they are proportional to μ (strange asymmetry). For $\mu = const$ Tamm [8, Chapter 5, Section 74] introduces a "new version" of the theory, where the last asymmetries are eliminated and asserts that this was only a matter of terminology (but this is a recognition that in this formulation the value of μ **has no physical meaning**). This last statement about the physical sense is also indirectly confirmed for the only plausible modification of this problem – the case of a homogeneous **external** environment [8, Chapter 5, Section 74]: there is no **universal** dependence on μ and the field is defined as a complex function of the geometric shape of the magnet and the permeability of the magnetic substances and the external environment. Thus, the meaning of introducing the very concept of magnetic permeability is immediately lost. And this is only a stationary case!

In the presence of magnetic substances, it turns out that it is impossible to calculate the field energy density in general [8, Chapter 6, Section 82] and then the latter is simply postulated.

Naturally, the question arises about the consistency of this, now the "condition", with the other equations. In reality, it is difficult to take into account for the self-consistent effect of the process (through temperature and magnetostriction) on the properties of the medium μ_{med} and the magnet μ_{mag} itself (nonlinear dependence of μ on the field and frequency dispersion, as well as the possibility of the absence of isotropy in the medium, can be mentioned).

When calculating the ponderomotive forces [8, Chapter 6, Section 83], the author again proceeds from the previously **postulated** expression for the energy density and again it is very strange when from the equality of the integrals, he claims that the integrands are equal. Thus, the obtained expression for the "density" of forces for a particular local point does not at all make any sense (it is the same as calculating the average density or composition of the surface layer of the Earth: somewhere water, somewhere mercury, etc., namely local deviations determine the anomalies of the gravitational and magnetic fields, fixed experimentally). Therefore, any local behavior of the particles can be very different from the behavior calculated with the help of such a "global average". From the same point of view, it makes no difference: whether to introduce "striction" forces (again calculated in a rough approximation) or not, since in fact, μ is an indefinite **fitting** function in this approach. Magnetic field stress tensor [8, Chapter 6, Section 84] can also be true only **on average**, and not in local places.

It is hardly possible to consider the theory of magnetism of substances as currently completed (although a number of phenomenological facts and dependencies are known, the predictive power of the theory is small), rather, it is about creating several diverse plausible models for each class of substances (for dia-, para-, ferromagnetic substances, etc.). The complexity is the determination of the magnetic properties of a material according to its composition [6, Chapter 10, Section 10.1]: for example, copper is a diamagnetic substance, *CuCl* is a paramagnetic substance, *Na* is a paramagnetic substance, but *NaCl* is a diamagnetic substance; graphite has an abnormally high diamagnetism, etc. It is noteworthy that, despite the **relative weakness of the magnetic forces**, the magnetic manifestations in different substances are **diverse** both in range and in qualitative sense (dia-, para-, ferro-, etc.). It is strange that both the dependence on the field strength (for ferromagnetic substances) and on the square of the field strength (for dia- and paramagnetic substances) are manifested. But textbooks almost always emphasize that the intra-atomic properties (especially for the inner shells of the atom) are practically independent on external influences. In this regard, the manifestation of a strong dependence of the magnetic properties on temperature is also completely incomprehensible.

Whether magnetic “charges” exist or not [6, Chapter 10, Section 10.2] – it is not known (since it is possible that the spins of electrons and atoms are involved in creating the magnetic field of currents, and the devices cannot be placed in the micro volume between the “poles of the magnet of elementary particles”). For example, the failures of their searches can be attributed to the fact that false properties are prescribed to such a “magnetic charges”.

Naturally, the definition of magnetic susceptibility (through a linear dependence of **M** on **B** or **H**) has an approximate character. But the phrase “despite the fact that the dimension of the magnetization **M** and the field **B** is the same, it would be wrong to express them in same units due to the coefficient 4π” [6, Chapter 10, Section 10.7] looks like a ridiculous masterpiece (can the length of a circle be expressed in centimeters?)!

The magnetic field **B** in the sample, of course, is **not uniform** on the microscale, and its **average** depends on the geometry of the experiment **as a whole**. For dipoles (electric and magnetic), it is impossible to determine expressions in a substance from a field in the far region: for example, the field near

$$E \sim \frac{q}{r^2}\ , \qquad p = qr = const$$

and for a small dipole ($r \to 0$, but $p = const$), the total field

$$E \sim \frac{const}{r^3} r^3$$

is the finite value, but the energy of the field $E^2 \mathrm{d}V \to \infty$!

Further, the author of the textbook [6, Chapter 10, Section 10.10] quite clearly says that the introduced both electric induction **D** and magnetic induction **B** – are not definable values in the samples. Thus, the expressions for **D**, for magnetization **M** and for induction **B** – are just some definitions of "letter" combinations. The introduction of these letter combinations complicates the system of Maxwell equations by the fact that the habitual boundary conditions disappear: in the general case $\mathrm{rot}\,\mathbf{D} \neq 0$ and $\mathrm{div}\,\mathbf{H} \neq 0$.

It would be worth agreeing that only those quantities that can be measured should be included in the formulas (and even better – to regulate in a controlled manner). And with units of measurement: if dimensionality of several quantities is the same, then you should not confuse yourself with “new” names and invent a non-existent difference in units of measurement. But from the point of view of the **experiment**, it is better to agree with the existing definition of magnetic susceptibility through $\mathbf{M} = \kappa\mathbf{H}$, than with the definition advocating by the author [6, Chapter 10, Section 10.10] through $\mathbf{M} = \kappa\mathbf{B}$. It is not clear from the theory, why **M** does not coincide in the direction with **H** in the general case: really, if the orientation of polycrystals (domains) is random, then, on average, the deviations θ of the lightest magnetization axis from an arbitrary direction of the field **H** are equiprobable at $\theta = const$ (do not depend on φ!).

Bohr’s conclusion that "magnetism should be absent in classical physics [10, Chapter 3, Section 75], because the magnetic field does not change the kinetic energy", can refer only to a **single (!) free elementary particle** in a vacuum. Since the magnetic field changes the shape of the particle movements, for solids everything depends on their structure and properties. The **hypothetical (as a postulate)** introduction of a number of relations in quantum mechanics is also not **an explanation**, and it is no better than the Langevin theory.

When “explaining” diamagnetism in [10, Chapter 3, Section 76], the following is striking. First, the nucleus is considered fixed, otherwise magnetic force also acts on it, and then the value of Ω will turn out for the nucleus to be other than that for electrons. Secondly, an isolated atom is considered, not a collective process (there are neither collective forces nor collisions). Thirdly, the calculation of the occurrence of Larmor rotation during switching on the magnetic field is too simplified (note that a paragraph earlier in the textbook **stated** that the classical system cannot possess a magnetic moment, since the magnetic field does not do mechanical work). In reality: 1) the orientation of the atom with respect to external magnetic field can be arbitrary; 2) whether we can consider **B** = 0, Ω = 0 for a single atom at the initial moment, if the nucleus and the **moving** (rotating in orbit) electrons themselves create a magnetic field (which can never be identically zero in all space)?

When allegedly explaining paramagnetism in the next paragraph [10, Chapter 3, Section 77], already a purely statistical approach is applied; and it is argued that precession cannot lead to paramagnet magnetizing: the magnetizing arises as a result of the interaction of atoms with each other (they already completely forgot about the process of turning on the magnetic field). “It turns out" that the paramagnetism of some metals "must" (after peeping into the measurement results of the dependence of κ on *T*) be explained by the spin magnetic moments of conduction electrons. Remarkably informative and predictive theory!

The need for artificial semi-classical introduction of short-range exchange forces for a quantitative explanation of ferromagnetism [10, Chapter 3, Section 79] also testifies to the inaccuracy of generally accepted laws of electrodynamics at small distances. It remains unexplained why, in the Dorfman experiment with a foil, these exchange forces do not act on β-particles at all. But maybe, after all, this is just some unaccounted collective process, and that is why a domain structure is formed?

It is strange that the Hall coefficient for ferromagnetic substances is 10-100 times greater than that for normal metals [10, Chapter 7, Section 98]. And the fact that there exist many metals with a positive Hall coefficient, simply means that even in a macroscopic plan, not everything is in order with the conventional electrodynamics. There is another oddity: according to Feynman, plasma cannot be either dia- or paramagnetic substance, since the Lorentz magnetic force does not produce work. However, this contradicts the experimental data (since in this case the magnetic confinement of the plasma could not work at all!). Also, the introduction of the concept of "hole conduction" (pseudo-explanation in hindsight by a scientific spell) negates all electrodynamics, which allegedly from the "first principles" determined what particles move in the reality (electrons), how the Lorentz force acts on these particles (and used it all in all subsequent evidences), etc.

When deriving thermodynamic relations in a magnetic field [11, Chapter 4, Section 31], one of the integrals for the work is transformed by authors into an integral over an infinitely distant surface. Firstly, there is not any physical principle here: this term will be different depending on where to take the bounding surface (that is, the term is undefined), and the infinitely distant surface is nothing distinguishable. Secondly, even a single excited atom can emit energy (not to mention a system of charged particles) and this impulse of electromagnetic radiation will fly to infinity, that is, the carried away energy remains the same finite value. Thirdly, how is it possible to use an infinitely distant surface at all, if the process takes place here, in a particular place, and any influence spreads at a finite speed, and if we are interested in the result of the action for a finite time, then we cannot wait indefinitely for establish at least some semblance of a causal connection with this infinitely distant surface. The same transition to an infinitely distant surface (that is, the discarding the term) occurs when they derive the total free energy of the magnetics [11, Chapter 4, Section 32] and the energy of the current system [11, Chapter 4, Section 33].

Further, it is hardly possible to say that the distribution of the currents $\mathbf{j}$ does not depend on the field created by them and the distribution of the magnetics. It is just that the modern electrodynamics is formulated in such a way that ϱ and $\mathbf{j}$ are assumed to be given (each charge is “entered by hands” according to a law given with varying degrees of accuracy), and all other quantities are sought (fields, for example).

When calculating the forces in a magnetic field [11, Chapter 4, Section 35], the following seems incoherent: at the beginning when calculating the stress tensor, they suppose **j** = 0, that is, rot **H** = 0, and after then “suddenly” recall that

$$\operatorname{rot}\mathbf{H} = \frac{4\pi}{c}\mathbf{j}$$

during the substitution of derivatives.

The arguments about the magnetic symmetry of crystals in [11, Chapter 5, Section 37] seem to be incomplete, since they do not take into account the presence of spins of particles and their possible ordering. In this case, the replacement $t \rightarrow -t$ changes the sign of only the quantity $\mathbf{j}$ caused by the motion of the particles as a whole. Initially, it is said that the exchange interaction is larger in magnitude than the magnetic interaction, but for some elements (for example, rare-earth ones) this occurs to be not the case. Again, it turns out: "the theory is true only for those cases for which it is true," that we can only find out *post factum*.

Thus, with the explanation and description of the well-known phenomenon of magnetism in modern electrodynamics, things are clearly not as smooth as we would like.

Chapter 11
Superconductivity

As another application of the theory of electromagnetism, consider the phenomenon of superconductivity.

The phenomenon of superconductivity [10, Chapter 3, Section 80] also demonstrates the incompleteness of conventional electrodynamics. For example, a two-fluid model was artificially invented to explain the resistance to alternating current; when determining the dependence on the critical field, purely formally, without explicit expressions, the author says about a "free energy"; in the formal theory of London & London there are also a lot of inexact reasoning and approximate calculations; with the help of “scientific spells”, it is told about the domain structure; through an immeasurable quantity (and this means post factum - to preserve the form of theory), a partition into superconductors of the first and second kinds is introduced; the BCS model is treated as a quantum phenomenon and can hardly claim to understand the real mechanism of superconductivity. At least, electron pairs, for which the dimensions substantially exceed the average distance between electrons – this is from the realm of fantasy, and bosons have nothing to do with this; really, for example, the nuclei of helium or argon atoms (bosons) do not fly inside the metal without resistance!

A strange “approach” is demonstrated in relation to the phenomenon of superconductivity [11, Chapter 6]: questions that are really interesting from the point of view of practical applications, namely, finding T_c, H_c and others, are not able to be considered, therefore, the electrical properties are declared to be uninteresting, but only as a consequence of magnetic properties of the conductor, and after that the a qualitative behavior of the magnetic properties is studied based on the belief in Maxwell's equations. Also, “by chance” (*post factum* – as needed), superconductors were divided into type I and II type superconductors (intermediate state, layered or vortex structure, etc.). Initially, it was said that in a superconductor the intensity **H** has no physical meaning, as well as the magnetization **M**, and then they are introduced in a “formal way” and used (again “read here, do not read here, a fat spot presents here ...”).

There is even an opinion that there exists no superconductivity at all (see, for example, the article by O. Kh. Derevnskyi “Blind Man’s Bluff and Electricity” http://newfiz.narod.ru/elvo-opus.htm [In Russian]). In this article there are many pretensions to the modern theory of electricity, and the author, as theorist, will not comment on them (it is better to read the source itself), but it would be like to hear the opinion of the experimenters on this subject.

Conclusion

So, for the modern theory of electromagnetic phenomena, summing up the analysis of its state and acknowledging its real achievements in the theoretical and experimental fields, we nevertheless see the following. Both the theoretical foundation, and the mathematical realization of the theory, and practical methods cannot boast of good physical, logical, or mathematical soundness; the severity of this theory is also very far from the required scientific rigor; and the algorithmicity is “not up to par”. Of course, experimental and engineering achievements in the field of electromagnetism will remain unshakable. However in the theory, some of them are replete with private hypotheses and with backdating fits under the result previously known from experience. Therefore, the author ventures to suggest that a number of theoretical points in the future will be changed. The author thinks that the new rigorous theory of electromagnetism will correctly describe phenomena on all scales, including in the microcosm, and therefore will completely “cover the area of electromagnetic applicability” of current quantum mechanics (and has “absorb” it).

We mainly analyzed electrodynamics using academic textbooks, but there is a whole group of experiments that contradict modern views (experiments of Rodin, Nikolaev, Sigalov, Chernikov and many others), which will also have to be algorithmically and consistently explained in the new corrected electrodynamics.

Whether it is time to go back to the first-source?

Appendix: Brief notes to related and alternative theories

In pursuit of formal quantitative indicators, mankind has spawn a huge number of physical theories – from highly-scientific to highly-absurd. And, paradoxically, a significant part of the latter is supported by academic science (an auxiliary criterion dividing highly-scientific and highly-absurd theories is the ratio of the number of artificially invented pettifoggery to the number of experimentally verifiable results; the smaller this criterion, the better).

Due to the huge flow of information, it is difficult to conduct any serious review of all alternative ideas in the field of electrodynamics. And the author is not an expert in alternative theories (if someone wants to seriously deal with them, it is better to read the original sources). Therefore, only some ideas will be briefly mentioned for the picture fullness, some very superficial remarks and assessments are given (the author apologizes in advance for not being able to analyze all alternative theories that even the author is familiar with).

To begin with, we make an obvious remark. If the new theory is based on the former false theory (for example, on the special or general theory of relativity, relativistic cosmology, the Big Bang theory, etc.) or includes it, then it is immediately clear that the result will be another new false theory. An example here is string theory (superstrings), trying to synthesize false theory of relativity and quantum mechanics (temporary construction) into some “genetically damaged hybrid”. At the same time, in order not to be unmasked in the foreseeable future, the string level is assigned to the very depth – under the subatomic level. Well yes!

We already have all the previous levels of “knowledge from A and to Z” (according to the pseudo scientists, the end of physics is nearing!)! Fairy tales about four-dimensional space, black holes and wormholes, dark energy and dark matter are not enough for pseudo scientists! Now they have new toys: 10 and even 26 dimensional space-time! What a wide sandbox for personal math games at public expense! To analyze these pseudo-theories and similar (M-theory, loop quantum gravity) does not make sense.

The following note. If a new theory includes the theory considered in this book unchanged (expands it), then it automatically transfers to itself all the “flaws” of the considered theories, that was found in the book (groundlessness, contradictions, problems, shortcomings). For example, quantum electrodynamics, electro-weak theory can be attributed to such theories. Naturally, the own specific problems will be added to the existing problems (infinite vacuum energy and its gravitational field, renormalization dubiousness, divergence of series and integrals, fundamentally non-extractable particles, fantastic colors and bad smells of other fictional supposedly quantum numbers, etc.), and the total number of problems can only increase. The results of all these mathematized toys are zero, except for the result of the noise raised around them in the mass media.

Of course, it is possible to evaluate in alternative theories only what was specifically done by the authors, and to compare with the similar results of their predecessors in the subject under study, and not to require full knowledge from them (“God’s equations”). Conventionally, all theories can be divided into two groups: 1) theories describing only observable phenomena and not going deeper than 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 first group (such type as “I do not invent hypotheses”) includes, for example, the work “Theory of Interaction” [13] by I.I. Smulsky, who quite clearly formulated that in this case the main question is the question about force (“the truth is in force”). If we assume Maxwell's equations are absolutely strict, then the electromagnetic force (in fact, the closing equation, giving experimentally detectable meaning to all the “letters”) should not be introduced externally, artificially (as was done with the Lorentz force), but must be automatically obtained from the Maxwell equations themselves. Such a procedure was carried out in [13], and a new self-consistent expression for electromagnetic force was obtained. Of course, the most important confirmation of the correctness of the expression obtained would be to obtain atomic spectra in a classical way, that remains to wish to the author of this theory (but until then it is worth reminding about the Maxwell equations that expressions for current elements can be extracted from experimental integral laws in many ways).

Another serious alternative to modern electrodynamics is the Gauss – Weber electrodynamics (emerged from the ideas of Ampère, framed by Gauss and Weber and developed further by Ritz). These ideas are quite serious, and it is better to get acquainted with them by primary sources (translations of some articles are contained on the website of S. Semikov http://www.ritz-btr.narod.ru/). Currently, there are proposals to adjust the initial expression of Weber’s force, which eliminates earlier existed problems. Here, too, only an experiment can say “its word”.

Another theory from the first group is autodynamics (“autodynamics” Ricardo Carezani 1940). The basis for it was a detailed analysis of “neutrino” experiments, from which it follows that the neutrino does not exist. As a result, a different expression was proposed for force (and for the dependence of mass on speed) than in STR. Regarding the change in the concept of mass, one can cite the same arguments as an objection as for the (false) theory of relativity (see [2]), but about the force (and dynamics) – the “resolution” should be given by experiment. By the way, some other researchers later also came to the conclusion that there are no neutrinos in Nature.

The following remark concerns theories that reject the existence of solid objects at all and consider everything in the world as a product 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-recover after interactions (the particles retain their identifiably discrete properties), it is necessary to recall that the wave formations pass through each other, but the particles collide and even bounce off each other. Take, for example, the Fourier expansion over the whole space: the harmonics without the environment do not interact, and how to determine which of harmonics belong to what of objects in our huge Universe?

Now we make a remark about the artificial opposition of long-range theories and theories of allegedly close-action in the field version. Writing expressions (equations, laws) in the form of partial differential equations does not mean at all that we have a theory of short-range action! Indeed, in modern theories, in order to find a solution, it is necessary to substitute boundary conditions, and the equations themselves are obtained taking into account certain boundary conditions. And in modern field versions, these – are the conditions at infinity. As a result, instead of the long-range theory (at a finite distance), it was not the theory of short-range action, as advertised, but the theory of super-long-range action (at infinity)! So, gentlemen, they are deceiving you. Well, the theory of potential or any boundary problem cannot express the theory of short-range action (to be local). An exception could be a problem with a local flow (in free space, reflecting something like an attached mass, determined by the properties of the particle itself, and for a general task, depending also on the configuration of the experiment). Till now such a field version is not observed. If anyone manages to embody such an idea, then praise will to him.

It is obvious that all ethereal theories are theories of short-range action and, generally speaking, they are physical theories trying to penetrate deep into matter and understand the causes and mechanisms of phenomena (as opposed to the pseudo mathematic character of many modern theories), that is, belong to the second group. The ethereal theories have the most enemies (and among highly educated semi-mathematicians, semi-physicists, and among specialists thoughtlessly believing in pseudoscientific advertising) who demand the impossible from these theories: immediately explain all the phenomena in the world (turning a blind eye to the fact that modern theories not only did not explain all phenomena, but also have many problems and internal contradictions). Theories of the ether are very diverse, even to list all the authors would be difficult, so just give some typical examples. For example, it is the gas ether (Atsukovsky; Prussov), and the electron-positron or photon ether (Rykov), and the granular ether (Zakazchikov), and the domain ether (Khaidarov), and the oppositely charged ether (Gorbatsevitch), and ether having a charge of one sign (Mirkin), and solid ether (Gusev), and liquid ether (Antonov), and many others. The particles of the ether itself can also be isotropic and anisotropic, and of several varieties, and possess a number of complex properties, and transform, etc.

Some theories are well developed; what of directions can be seriously analyzed? Obviously, only a set of experimentally **confirmed new predictions** could confirm or refute this or that theory, or force to abandon from everyone (it is clear that the experiments advertised by generally accepted science cannot be considered as critical ones). In the meantime, we can make the following comments on the “internal” problems of such theories. If the particles of the ether are able to transform, then what is the mechanism of self-healing and maintaining the experimentally verified identity and discreteness of many objects in our world? For ether particles with complex properties, again there are problems to explain such properties (their causes and mechanisms of their origin and action). For example, if we consider particles of ether that have charges of both signs, then the previous unresolved questions remain: what forces hold each charge as a single whole, what is the mechanism of attraction for charges of the opposite sign? That is, the same questions are again transferred to a deeper level. Why are these charges not immediately neutralized? And etc.

If ether – is the repelling particles of the same sign, then why is our world not purely gas (but it is also condensed into solid and liquid objects)? For the solid ether, the main "internal" questions are the following: what keeps this solid formation together, and to explain the mechanism of movement through it without braking for objects of 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 energy range).

What would you like to expect from any theory? At least:

1) internally noncontradictory, consistent approach to phenomena;

2) an algorithmic description of the whole complex of the phenomena under consideration in a unified way (without particular hypotheses *ad hoc* for each particular case, without peeping into the answer);

3) obtaining from the first principles of all experimentally measurable quantities, and not super-mathematized games with artificially invented things;

4) new experimentally verifiable predictions;

5) whenever possible, an explanation of the causes and mechanisms of the phenomena.

Afterword

In disputes there is no higher ones or lower ones,
neither titles nor names; only one thing is important
– the truth before which everyone is equal.
(R. Rolland)

The author views his book as a program to substantiate and make subsequent changes to the theory of electromagnetic phenomena. We think that many people have previously encountered some particular inconsistencies and problems of the theory in question, but they were hardly familiar with the whole system of frame-up, inconsistencies, artificial hypotheses and internal problems. Therefore, the task of this book was to “remove the blinders from the eyes,” to help think independently about the existing problems and attitudes towards them. But for this you need not to extract out of the memory once learned impromptu blanks, and learn to look at everything consciously, “with open eyes”. To know why sometime at a fork in the road, certain physical definitions, ideas, laws, methods were adopted. To be able to evaluate the ideas of the past from the perspective of the facts and experiences that have accumulated so far and, if necessary, return to the same “fork in the road” and make a better choice.

Someone may say: “Why do we need such critical books, especially since a ready-made theory is not proposed in return?” However, any work must be done at the need place and in the need time, otherwise it is “Sisyphean toil”. Currently, the scientific community is not yet ready to accept any new ideas in the field of electromagnetism applications, considering electrodynamics to be an example of rigor. But the foundation of science concerns every physicist. Should we wait for another revolution, or should we already learn to be more mature and to look for solutions to emerging problems in time (by evolution)? It is needed do not hide like ostriches in the sand, but discuss difficult moments.

Unfortunately, the “great physical revolutions” led to a deterioration of the situation in the scientific world. Instead of honest researchers (interested in the Truth), the scientific community began to turn into an "inert mass", where the proportion of true scientists is relatively small. The process of self-cleaning and self-organization almost stopped working. In the scientific community, at the present time, several groups can be conventionally distinguished: 1) true scientists, 2) simply paid scientific "workers", 3) officials from science, 4) committed pseudo-scientists.

The number of pseudo-scientists from science, who are ready with selfish interests to say on the “black” as the “white”, is small, but they have captured almost all of the “advertising time”. The modern cosmology and both theories of relativity should be attributed to the officially supported false theories (the very existence of quantum mechanics suggests that on a small scale modern electrodynamics faces internal problems). Academic officials maintain a strict bureaucratic order, they “hold their nose to the wind,” ready to occur “with the flag in front of” the prevailing opinion.

The overwhelming part of the scientific community consists of simply paid scientific workers who are ready doing with anything. Many are willing 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 a stereotype of the always hurrying digger is embedded in their subconscious mind, which is ready to present any find as the big treasure with craving for recognition as a final goal).

The position of the True Scientist is remarkably articulated in the following statement. **Who wants to bring out the truth, he is no less diligently looking for it and in the beliefs or assumptions of the opponent ... He tries to help the opponent to find words for his thought that would most accurately express it. He tries to understand the opponent better than that understands himself. Instead of using every weak point of the opponent's argument for the overthrow, debunking and destruction of the case that he defends, the participant of a 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)

Whether many are treated responsibly to the search for Truth, to the advancement of their own and others' true results and to the methods of conducting discussion as the True Scientists, or not? Not worth taking the discussion of scientific theories in the spirit of the animal instincts of competition “for a place under the Sun”! Let us finally move away from the vicious practice of “sweeping up the problems under the carpet”, but, on the contrary, let us honestly report inconsistencies in physical theories, contradictions with other facts or proven theories, non-algorithmic techniques, additional *ad hoc* hypotheses, methodological or mathematical problems. When these problems are honestly highlighted, then any researcher can try to solve them; and if our generation cannot do this, then next generations will surely be able to do it. It is important that each new generation is not forced stealthily "to winkle out these problems out of the under-carpet" from scratch, but the youngest and most productive years could focus on thinking about them and solving them. For example, mathematical books with a title beginning with the words “Unsolved Problems ...” always inspire, unlike the whining of some “prominent” physicists about the end of science.

It would be nice if the state, as the main sponsor of science, developed a criterion 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 cause many to think (in the absence of visible scientific breakthroughs, the modern formal criteria for evaluating scientific activity, which artificially imposed from the outside, rather force them to "drive a plan" and "pile up closer to powerful clans" than get worthwhile results). It is necessary to develop an adequate attitude in the scientific community to discussions on the broadest topics, to the willingness to admit mistakes made (there is nothing tragic either in mistakes or even in their recognition). Maybe then the process of self-purification of science from real pseudoscientists in power, from the clan system and authoritarianism of officials from science would be resumed. It would be liked that those who are engaged in science, searched not "their place under the Sun" in this area of activity, but did a real search for the Truth. It would be liked that there were more Real Scientists in the scientific community. In this field there should be no competitors in a hurry, but only honest and conscientious people – allies and like-minded people.

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