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1.Classical and Modern Topology.
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According to the opinion of the Ancient Greeks, the famous real and mythical founders of Mathematics and Natural Philosophy like Pythagoras, Aristotle and others, in fact, borrowed them from the Egyptian and Middle East civilizations. However, what had been told before in the hidden mysteries Greek scientists transformed into written information acceptable for everybody. Exactly after that the development of science in the modern sense started and had already reached a very high level 2000 years ago. Therefore you may say that the free exchange of information and making it clear for people have been the most important discoveries of Greeks. I would say it is the basis of our science now. As you will see, any violation of this fundamental rule does serious harm to our science and inevitably leads to its decay.

1.Classical and Modern Topology.

Prehistory. First fifty years of Topology. The first important topological ideas were observed by famous mathematicians and physicists like Euler, Gauss, Kelvin, Maxwell and their pupils, during the XVIIIth and XIXth Centuries. As everybody knows, it was Poincare' who really started Topology as a branch of Mathematics in the late XIXth Century. Many top class mathematicians participated in the development of Topology in the first half of our century. A huge number of mutually connected fundamental notions were invented: degree of maps and singularities of vector fields, homotopy and homology groups, differential forms and smooth manifolds, the fundamental idea of transversality, the simplicial/cell(CW) and singular complexes as tools for studying topological invariants, braids, knot invariants and 3-manifolds, coverings, fibre bundles and characterisic classes and many others. Deep connections with Qualitative Analysis, Calculus of Variations, Complex Geometry and Dynamical Systems were established in this period. Combinatorial Group Theory and Homological Calculus started from topological sources. A great new field of topological objects unknown to the classical mathematics of the XIXth Century appeared finally in the 1940s.

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At that time this new area was known to few number of mathematicians only. However there was a very high density of really outstanding scientists among them.

1950s and 60s: Golden Age of Classical Topology. The fundamental set of algebraic ideas unifying all these branches of mathematics appeared in the 40s; a new era started about 1950. Spectral sequences of fibre bundles, sheafs, highly developed homological algebra of the groups, algebras and modules, Hopf algebras and coalgebras, were invented and heavily used for the calculation of topological invariants needed for the solution of the fundamental problems of topology. Let me point out that, in many cases, it was a completely new type of calculations based on the deep combination of the very general "categorial properties" of these quantities with very concrete geometric, algebraic or analytical study of a completely new type. In the previous period, people even had no dreams as to how they could be calculated. Regular methods were built to calculate homotopy groups, for example. It was one of the most difficult problems of topology. A lot of them were computed completely or partially including the homotopy groups of spheres, Lie groups and homogeneous spaces. The topologically important cobordism rings were computed and used in many topological investigations. The famous signature formula for differentiable manifolds was discovered. It has an innumerous number of applications in the topology of manifolds. Besides that, this formula played a key role in the proof of the so-called Riemann-Roch theorem in Algebraic Geometry and later in the study index, the famous homotopy invariant of Fredholm operators.

The mutual influence of Topology and Algebraic Geometry during that period led to the broad extension of the ideas of homology: the extraordinary (co)homology theories like K-theory and cobordisms appeared. They brought a new type of technic to topology with many applications. Representation theory and complex geometry of manifolds deeply unified with homological algebra and Hopf algebras. The technic of formal groups appeared here. It has been applied in particular for the improvement of the calculations of stable homotopy groups of spheres. As everybody knows, during this period topology solved the most fundamental problems in the theory of multidimensional smooth manifolds:

Nontrivial differentiable structures on multidimensional spheres were discovered on the basis of the results of algebraic topology combined with a new understanding of the geometry of manifolds and bundles. The multidimensional analog of the Poincare Conjecture and H-cobordism theorem were proved. Counterexamples to the so-called "Hauptvermunung der Topologie" were found. A classification theory for the multidimensional smooth (and for PL-manifolds as well) was completely constructed. The role of the fundamental group in this theory led to the development of a new branch of algebra: the algebraic K-theory. Topological invariance of the most fundamental characteristic classes was finally proved. The so-called "Annulus Conjecture" was proved. No matter how elementary these results can be formulated, nobody has succeeded to avoid the use of a whole bunch of results and tools of algebraic and differential topology in the proof. The classification theory for the immersions of manifolds was constructed. The theory of multidimensional knots was constructed. Several classical problems of the theory of 3-manifolds also were solved during that period: the so-called Dehn's program was finished after a 50 year break; the algorithm for recognizing the trivial knot in three-space has been theoretically constructed as a part of the deep understanding of the structure of 3-manifolds and the surfaces in them. As a by-product of topology, the fundamental breakthrough in the topological understanding of generic dynamical systems was reached. A new great period started in this area. Qualitative theory of foliations has been constructed with especially deep results for 3-manifolds.

As a summary, I would like to add one more very important characteristic of the topological community in the golden age of classical topology:

All important works have been carefully checked. If some theorem had not been proved, it immediately became known to everybody.

So you can find a full set of proofs in the literature. Unfortunately, a full set of textbooks covering all these developments (1950-1970) has not been written yet. Many modern textbooks are written in a very absract way. Even if they cover some pieces formally, it is more difficult to read them than the original papers. Let me recommend to you the Encyclopedia article [1] written exactly for the exposition of these ideas.

1970s: Period of decay. In my opinion, the period of the 1970s can be characterized as a period of decay for classical topology. There are many indications for that. Several leading scientists left topology for the new areas like algebra and number theory, riemannian and symplectic geometry, dynamical systems and complexity theory, functional analysis and representations, PDEs, and different branches of mathematical/theoretical physics.... It is certainly a good characterization of the community if it could generate such a flux of scientists in many different areas and bringing to them completely new ideas. Anyway, this community dispersed.

What can we say about the topological community after that?

First of all, some important new ideas appeared in the 70s (like localization technic in homotopy topology, the nicely organized theory of the rational homotopy type, hyperbolic topology of 3-manifolds). However, **a huge informational mess was created in the 1970s**. Let me point out that a series of fundamental results of that period was not written, with full proof, until now. Let me give you a list:

Sullivan's Haupvermutung theorem was announced first in early 1967. After the careful analysis made by Bill Browder and myself in Princeton of the first version in May 1967 (before publication), his theorem was corrected: a necessary restriction on the 2-torsion of the group $H_3(M, Z)$ was missing. This gap was found and restriction was added. Full proof of this theory has never been written and published. Indeed, nobody knows whether it has been finished or not. Who knows whether it is complete or not? This question is not clarified properly in the literature. Many pieces of this theory were developed by other topologists later (they used sometimes different ideas). Nobody has unified them until now. Indeed, these results were used by many others later. In particular, the final Kirby-Siebenmann classification of topological multidimensional manifolds therefore is not proved yet in the literature.

The second story is the theory of Lipshitz structures on the manifolds. In the mid-seventies Sullivan distributed a preprint containing the idea how to prove existence and uniqueness of such structures on the manifold $N^n, n \neq 4$. This idea obviously included (for the uniqueness) the direct use of the Annulus Conjecture (and therefore of all ideas and technic needed in the proof of topological invariance of the rational Pontryagin Classes inside). Proof of the Lipshitz Theory has never been published. Indeed, many years later, already in the 1990s, some brilliant younger scientists developed a very nice theory of Fredholm (elliptic) operators on Lipshitz manifolds. As a corollary, they claimed that a new proof of topological invariance of rational Pontryagin classes has been obtained from Analysis (it was a problem posed by Singer in the 60s). Young scientists made a "logical circle" believing in the classical results. Nobody told them that corresponding theorems have never been proved. How could it happen? This funny story shows the modern state of information in the topological community.

Another informational mess has been created in 3D Hyperbolic topology. This beautiful area was started by Thurston in the mid-70s. For many years people could not find out what was proved here. In this area the situation has been finally resolved: it has been aknowledged that these methods lead to the proof of the original claim (the so-called Geometrization Conjecture) only for the special class of Haken manifolds. The Geometrization Conjecture means more or less that (in the case of closed 3-manifolds) the fundamental group can be realized as a discrete subgroup acting in the 3D Hyperbolic space if trivial necessary conditions are satisfied: all its abelian subgroups are cyclic and $\pi_2 = 0$. However, it is difficult to find out who actually proved this theorem? It seems for me that the younger mathematicians who managed to finish this program did not receive proper credit.

I would like to mention that this kind of informational mess has happened since 1970 not only in topology. For example, the famous results of KAM in the three-body problem known since the early 60s were found recently unproved. It was announced for the first time at the Berlin Congress last year. In this case, some works supposedly containing full proof were published in the first half of the 60s. Does this mean that nobody accually read them for at least 30 years?

Do you think that algebra is better? Let me tell you as a curious remark that all works of the Steklov Institute (i.e. Shafarevich's) school in algebraic number theory, algebraic geometry and theory of finite p-groups awarded by the highest (Lenin and State) prizes in the former Soviet Union since 1959, did not contain full proof. The gaps in the proofs were found many years later. Not all these gaps were really deep. However, some of these authors knew their mistakes many years before they became publicly known and could not correct them. They managed to fulfill gaps after many years , using much later technical achievements made by other people. Does it mean that in the corresponding time, despite many public presentations, nobody in fact read these great works? Can we say that all proofs are known now in all these cases?

There are much worst cases in modern algebra indeed. How many of you know that the so-called classification of simple finite groups did not exist as a mathematical theorem until now? In this case we can even say that in fact (as a few number of real experts have known since 1980) no one work existed claiming that this problem was finished in this work. All public opinion has been based only on the "New York Times Theorem" for the past 20 years.

1980s and 90s: Period of recovery. The role of Quantum Field Theory.

It became clear already in the late 70s that modern quantum field theory started to generate new ideas in topology. It gave several new alternative ways to construct topological invariants: Path integral for the metric-independent actions on manifolds was used for the first time. The famous self-duality equation appeared first in the works of physicists. It was applied in the 80s for the solution of fundamental topological problems in the theory of 4-manifolds. Quantum string theory brought in the early 80s new deep results in the theory of the classical Fuchsian groups and moduli spaces. At first physicists (like t'Hooft and Polyakov) were not interested very much in such by-products of their activity. They always said that they were doing physics of the real world, not pure mathematics. However the next wave of brilliant physicists (like Witten, Wafa and others) started to solve problems of pure mathematics. Such purely topological subjects like the Morse theory and cobordism theory associated with action of compact groups on manifolds, were developed in the 80s from the completely new point of view. Symplectic Topology reached a very high level in the late 80s. We are facing now impressive development of Contact Topology.

Certainly Quantum Theory brought new beautiful ideas. Besides that, the fundamental new invariants of knots were discovered in the 80s by the topologists who came from functional analysis and theory of C^* algebras. These invariants also received quantum treatment in the late 80s. The beautiful connection of the specific Feinmann diagrams with surfaces was borrowed from physics literature. It became a very effective tool for the solution of several topological problems. Unfortunately, only a few number of mathematicians learned this technic and started to apply it in topology. I know only Singer, Konzevich and a very small number of others. Even if you will add here the names of pure mathematicians who learned this with the intention to do real physics, this list will increase inessentially. I do not count here people who were trained originally in the physics community. A large number of them moved into pure mathematics with the intention to prove rigorous theorems about the models serving (in their opinion) as an idealization of theoretical physics. They call this area Mathematical Physics, but not everybody agrees with such a definition of mathematical physics. This community does not do topology.

I would like to make a remark here concerning a beautiful work of Konzevich calculating certain Chern numbers on the punctured moduli spaces of Riemann Surfaces through the special solution to the KdV hierarchy. This folmula has been known as a Witten Conjecture. You have to specify for this some compactification of the moduli spaces of punctured Riemann surfaces, otherwise it makes no sense. Konzevich accually proved this formula for one specific (”Strobel-Penner”) compactification in 1991. What about the standard Deligne-Mumford compactification? Konzevich claimed in 1992 in his work in Inventiones that it is true. However, no proof has been presented until now. So this problem is open. There was a mistakable statement about this at the Berlin Congress.

Let me point out that the physics community did not create any informational mess in topology. According to their training tradition, theoretical work produces Conjectures which should be proved only by some kind of experiment. Starting to do beautiful nonrigorous mathematics, they do not claim that they ”proved” something. They are saying that they ”predicted this fact”. In the case of pure mathematics, the final proof done by pure mathematicians these people may treat as an ”experimental confirmation”. In the past ten years several deep results have been obtained in the 4D topology. We cannot say this about 3D topology: quantum invariants here created some sort of ”invariantology”: a lot of people are constructing topological invariants but no one new topological result has been obtained for almost 10 years. Indeed, these ideas look beautiful in some cases. In my opinion, new deep results will appear after better understanding of the relationship of new invariants with classical topology.

Topological Phenomena in Real World Physics

Topological ideas in physics in the period of the early 80s. I spent about 10 years learning different parts of Modern Theoretical Physics in the 60s and 70s. After joining the physics community (i.e. Landau school) in the early 70s I found out that most physicists did not know at all the new areas of mathematics like topology, dynamical systems and algebraic geometry, including analysis on Riemann surfaces. The quantum people knew some extracts from the group theory and representations because they needed it in Solid State Physics as well as in Elementary Particles Theory since the 1960s. A lot of them knew something about Riemannian Geometry because of the Einsteinian General Relativity. However, these people had already heard something about the new mathematics of the XXth century and badly wanted to find its realization in physics. You have to take into account that between them there was a great number of extremely talented people at that time with very good training in practical mathematics. In some cases I was able to help physicists (like Polyakov, Volovic and some others) to learn and to use topology in the 70s. I worked this period in General Relativity (Homogeneous Cosmological Models) and Periodic KdV Theory with my pupils and collaborators. We found completely nonstandard applications of Dynamical Systems and Algebraic Geometry in these areas. However, until the late 70s I did not produce any new topological ideas. My very first topological work in physics was made in 1980 (see [2]). I started to use in the spectral theory of the Schrodinger operators in periodic lattice and magnetic field the idea of transversality applied to the families of Hermitian matrices or elliptic operators on the torus. This idea led to the discovery of the series of topological invariants, Chern Numbers of Dispersion Relations. They are well-defined for the generic operators only. The classical Spectral Theory in mathematics never considered such quantities because they are not defined for every operator with prescribed analytical properties of coefficients. The ideology of transversality is important here. This work was not understood by my colleagues-physicists at that time (the vice-editor of JETP did not want to publish it as "nonphysical", so I published it in the math literature). People thought that the important integer-valued observable quantities in Solid State Physics may come from symmetry groups only. Indeed, the Integral Quantum Hall phenomenon was discovered soon. Some famous theoretical physicists rediscovered my mathematical idea after that. It is certainly a sum of the Chern classes of dispersion relations below the Fermi level.

My next topological discovery was made in the joint work with student I.Schmelzer in 1981, dedicated to the very special problem of classical mechanics and hydrogynamics (see [3]). I immediately realized its value for modern theoretical physics, as well as for mathematics, and developed this idea in several directions in the same year ([4]). The series of work in the Theory of Normal Metals which I am going to discuss today, is also one of by-products of that discovery. Doing the Hamiltonian factorization procedure for the top systems on the phase spaces like $T^*(SO_3)$ by the action of S^1, you are coming to the systems mathematically equivalent to the motion of the charge particle on the 2-sphere. This sphere is equipped by some nontrivial Riemannian metric. What is important and has been missed by the good experts in analytical mechanics like Kozlov and Kharlamov is that the effective magnetic field like Dirac monopole appears here for the nonzero values of the "area integral" associated with S^1-action. It means precisely that the magnetic flux along the sphere is nonzero. The reason for this is that the symplectic (Poisson) structure after factorization is topologically nontrivial. In terms of modern symplectic geometry, the magnetic field is equivalent to the correction of the symplectic structure. This fact is not widely known in the geometric community even now. The appearance of the topologically nontrivial symplectis structures after S^1-factorization of symplectic manifolds was independently discovered and formulated in geometric, nonphysical terminology in 1982 in the beautiful work [6] for different goals (calculating of integrals).

It has been realized in [3, 4] that the action functional for such systems is in fact a closed 1-form on the spaces of loops. These functionals have been immediately generalized for higher dimensions, to the spaces of mappings F of q-manifolds in some target space M where a closed $q + 1$-form is given instead of magnetic field. We are coming finally to the action functional well-defined as a closed 1-form on the mapping spaces F. The topological quantization condition for such actions was formulated in 1981 [4, 5] as a condition that this closed 1-form should define an integral cohomology class in $H^1(F, Z)$. It is necessary and sufficient for the Feinmann amplitude to be well-defined as a circle-map

$$\exp\{iS/h\} : F \to S^1$$

For the case $q = 1$ the original Dirac requirement was based on a different idea: the magnetic field should be a Chern class for the line bundle whose space of sections should serve as a Hilbert space of states for our Quantum Mechanics. Therefore it should be integral in $H^2(M, Z)$.

In pure topology and in the Calculus of Variations these ideas led to the construction of the Morse-type theory for the closed 1-forms on the finite- and infinite-dimensional manifolds. Let me refer to the last publication of the present author (with P.Grinevich) in this direction [7] where the survey of results and problems is discussed. I would like to point out that for the compact symplectic manifolds the action functional for any nontrivial Hamiltonian system is multivalued. The cohomological class of symplectic form cannot be trivial here. I do not know of such cases in real physics where the symplectic manifold is compact. However, even in the community of symplectic geometers nobody paid attention to such properties of action functional until the 90s.

After that I started to think about different aspects of the Hamiltonian Theory where the class of one-valued functions naturally can be extended to the class of all closed 1-forms. For every symplectic (Poisson) manifold M with $H^1(M) \neq 0$ we may consider Hamiltonian Systems generated by the closed 1-form dH where the function H is multivalued. Instead of energy levels $H = const$ we have to consider nontrivial codimension 1 foliation $dH = 0$ with Morse (or Morse-Bott) singularities. We are coming to the topological problems of studying such foliations. It has been posed in [5]. Several participants of my seminar (A.Zorich, Le Tu Thang, L.Alania) have made very important contribution to the study of this subject. Interesting quasiperiodic structure appears here. It is not revealed fully in my opinion (see references and discussions in the article [9]).

Multivalued Hamiltonians in real physics. I started to look around in 1982 asking the following question: can you find such systems in real physics where Hamiltonian or some other important integral of motion is multivalued (i.e. dH is well-defined as a closed 1-form)? Much later people realized that in the theory of the so-called Landau-Lifshitz equation (which is a well-known physical integrable system with zero-curvature representation elliptic in the spectral parameter) the momentum is a multivalued functional.

At that time (1982) I found only one such system describing motion of the quantum ("Bloch") electron in the single crystal D-dimensional normal metal (D=1,2,3) under the influence of the homogeneous magnetic field B. We are working here with one-particle approximation for the system of Fermi particles whose temperature is low enough. For the zero temperature our electrons fill in all one-particle quantum Bloch states ψ_p below the so-called

"Fermi Level" $\epsilon \leq \epsilon_F$. Its value depends on the number of electrons in the system. It is the intrinsic characteristic of our metal. The index p here may be considered finally as a point in the torus T^D defined by the reciprocal lattice dual to the crystallographic one

$$p \in T^D, T^D = R^3/\Gamma^*$$

There is a Morse function $\epsilon(p) : T^D \to R$ (dispersion relation) such that the domain $\epsilon \leq \epsilon_F$ in the torus T^D is filled in by Bloch electrons. Its boundary $\epsilon = \epsilon_F$ is a closed surface $M_F \subset T^D$ for $D = 3$. We call it **Fermi Surface.** It is homologous to zero in the group $H_2(T^3, Z$. For finite but very small temperature all essential events are happening nearby the Fermi Surface.

Add now a homogeneous magnetic field to our system (i.e. put metal in the magnetic field B). Nobody succeeded in constructing a suitable well-founded theory for the exact description of electrons in the magnetic field and lattice. Irrational phenomena appear in the spectral theory of Schrodinger operators and destroy all geometric picture. However, since the late 50s physicists have used some sort of adiabatic approximation which they call "semiclassical". Let me warn you that this approximation has nothing to do with the standard understanding of semiclassical approximation. We take dispersion relation $\epsilon(p)$ as a function on the torus T^3 extracted from the exact solution of the one-particle Schrodinger operator in the lattice without magnetic field. We consider a phase space $T^3 \times R^3$ with coordinates $p_i, x^j, i, j = 1, 2, 3$ and Poisson bracket of the form:

$$\{x^j, x^k\} = 0, \{x^j, p_k\} = \delta^j_k, \{p_j, p_k\} = B_{jk}$$

where $B_{jk}(x)$ are components of the magnetic field B treated as a 2-form. Our space R^3 is Euclidean, so we can treat magnetic field as a vector B with components B^j.

Take now the function $\epsilon(p)$ as a Hamiltonian. It generates through the Poisson structure above a Hamiltonian system in the phase space $T^3 \times R^3$. For the homogeneous (i.e., constant) magnetic field we can see that our phase space projects on the torus T^3 with Poisson bracket $\{p_j, p_k\} = B_{jk}$. This Poisson bracket has a Casimir (Annihilator) $C_B(p) = \epsilon^{ijk} p_i B_{jk} = B^i p_i$. This Casimir is multivalued: it is defined by the closed 1-form $\omega_B = \sum_i B^i dp_i$ on the torus. As you will see, this is the main reason for the appearence of nontrivial topological phenomena in this problem.

Our Hamiltonian $H = \epsilon(p)$ depends on the variable p only. Therefore all important information can be extracted from the Hamiltonian system on the 3-torus with Poisson bracket defined by the magnetic field. The electron trajectories for the low temperature can be described as a curves in this torus such that

$$\epsilon(p) = \epsilon_F, C_B(p) = const$$

However, the levels of the Casimir on Fermi surface are in fact leaves of foliation given by the closed 1-form restricted on the Fermi surface

$$\omega_B|_{M_F} = \sum_i B^i dp_i|_{M_F} = 0$$

Some people in ergodic theory studied in fact the most generic ergodic properties of "foliations with transversal measure" on the Riemann surfaces. In a sense, our situation is a partial case of that. However, our picture in 3-torus is nongeneric in that sense. We cannot apply any results of that theory. We have to work with foliations obtained in the 3-torus by this special procedure only. Our use of word "generic" here is resticted by that reqiurement. As we shall see, ergodicity is a nongeneric property within this physically realizable subclass of foliations 2-surfaces given by the closed 1-form. What is interesting is that ergodic examples exist in our picture but they occupy a measure zero subset on the sphere of directions of the magnetic fields (if generic Fermi surface is fixed).

As I realized in 1982 (see [5]), this picture leads to nontrivial 3-dimensional topology, and I posed it as a purely topological problem to my students. The first beautiful topological observation was made by A.Zorich [8] for the magnetic fields closed to the rational one. After new discussion and reconsidering all conjectures (see [9]), I.Dynnikov made a decisive breakthrough in the topological understanding of this problem for the generic directions of magnetic fields (see [10]). S.Tsarev constructed in 1992 the first nontrivial ergodic examples, later improved by Dynnikov see [14]).

However, several years passed before some physical results were obtained (see the first remark about the possibility of that in my article [11]). We made a series of joint works with A.Maltsev (see [12, 13]) dedicated to physical applications. Essentially, we borrowed topological results from the works of Zorich and Dynnikov. However, the needs of applications required that we not apply their theorems directly, but extract the key points from the proofs and reformulate them. So the modern topological formulations of these results are by-products of these works with applications (see the most modern survey in [14]). Let me formulate here our main physical results and after that explain the topological background and generalizations.

This picture has been extensively used in solid state physics since the late 50s. The leading theoretical school in that area has been the Kharkov-Moscow school of I.Lifshitz and his pupils, like M.Azbel, M.Kaganov, V.Peschanski, A.Sludskin and others. You may find all proper quotations to physics literature in the survey article [13]. The following fundamental **Geometric Strong Magnetic Field Limit** was formulated by that school (and fully accepted later by the physics community):

All essential phenomena in the conductivity of normal metals in strong magnetic field should follow from the geometry of the dynamical system described above.

How to understand this principle? You have to take into account that this picture certainly will be destroyed by the "very strong" magnetic field where quantum phenomena (of the magnetic origin) are important. It should happen for such magnetic fields that magnetic flux through the elementary lattice cell is comparable with quantum unit. However, the lattice cell in solid state physics is so small that you need for that magnetic field the order of magnitude $B \sim 10^8 Gauss$ or $B \sim 10^4 t$ where 1t is equal to $10^4 Gauss$.

Therefore we are coming to the conclusion that even for the "real strong" magnetic fields like $10^2 t$ this picture still works well.

For our goal we need to consider such metals that Fermi surface is topologically nontrivial. It means precisely that the imbedding homomorphism of fundamental groups

$$\pi_1(M_F) \to \pi_1(T^3) = Z^3$$

is onto. As people have known already for many years, the noble metals like copper, gold, platinum and others satisfy to this requirement. Probably the very first time this property was found was by Pippard in 1956 for copper. Many other materials with really complicated Fermi surfaces are known now.

By definition, the electron orbit is **compact** if it is periodic and homotopic to zero in T^3. Therefore it remains compact on the covering surface in R^3, where R^3 is a universal covering space over the torus T^3. All other types of trajectories will be called **noncompact**.

Normally all pictures in physics literature are drawn in R^3, but everybody knows that quasimomentum vectors $p_1, p_2 \in R^3$, such that $p_1 - p_2$ belongs to the reciprocal lattice Γ^*, are physically identical.

The Lifshitz group started to study this dynamical system about 1960 and made the first important progress. For example, Lifshitz and Peschanski found some nontrivial examples of noncompact orbits stable under the variation of the direction of magnetic field. It looks like nobody could understand them properly in the physics community at that time. It was several decades before this community started to understand the geometry of dynamical systems. The Lifshitz group was ahead of its time. They made some mistakes leading to wrong conclusions and investigations were stopped. You may find the detailed discussion in our survey article [13]. Their mistakes have been found only now because they contradicted our final results describing the conductivity tensor.

Our main results:

Consider projection of the conductivity tensor on the direction orthogonal to magnetic field. This is a 2×2 tensor σ_B. Applying any weak electric field E orthogonal to B, we get current j. Its projection $\sigma_B(E)$ orthogonal to B is only what is interesting for us now. We claim that for the strong magnetic field $|B| \to \infty$ of the generic direction in S^2 only two types of asymptotics are possible:

Topologically Trivial Type:

$$\sigma_B \to 0, |B| \to \infty$$

More exactly, we have $\sigma_B = O(|B|^{-1})$ for the topologically trivial type. All directions with trivial type occupy a set U_0 of measure equal to μ_0 on the two-sphere $U_0 \subset S^2$.

Topologically Nontrivial Type:

$$\sigma_B \to \sigma_B^0 + O(|B|^{-1}$$

Here 2×2 tensor σ_B^0 is a nontrivial limit for the conductivity tensor. We claim that it has only one nonzero eigenvalue on the plane orthogonal to B. Let us describe the topological properties of this limiting conductivity tensor. It has exactly one eigen-direction $\eta = \eta_B$ with eigenvalue equal to zero. Consider any small variation B' of the magnetic field B. For the new field B' we have an analogous picture if perturbation is small enough. We have a new 2×2 tensor $\sigma_{B'}$ with one zero eigen-direction $\eta_{B'} = \eta'$. Our statement is that the plane $a\eta + b\eta', a, b \in R$, generated by this pair of directions, is locally stable under the variations of magnetic field. This plane is integral (i.e. generated by two reciprocal lattice vectors). It contains zero eigen-directions $\eta_{B''}$ for all small variations of the magnetic field B. It can be characterized by 3 relatively prime integer numbers $m = (m_1, m_2 m_3)$. This triple of integer numbers is a measurable topological invariant of the conductivity tensor. An open set of directions $U_m \subset S^2$ with measure μ_m corresponds to this type. The total measure of all these types is full:

$$\mu_0 + \sum_{m \in Z^3} \mu_m = 4\pi$$

We started to look in the old experimental data obtained in the Kapitza Institute in the 60-s by Gaidukov and others (see references in [13]). They measured resistance for the single crystal gold samples in the magnetic field about 2t-4t following the suggestion of Lifshitz. Confirming the ideas of the Lifshitz group, several domains with nonisotropic behavior of conductivity were found and many suspicious "black" dots (maybe domains of small size) on the sphere S^2. It is not hard to see even now that several larger domains in these data with nonisotropic conductivity should correspond to the simplest stable topological types like $(\pm 1, 0, 0), (\pm 1, \pm 1, 0), (\pm 1, \pm 1, \pm 1)$ up to permutation in the natural basis of this cubic lattice. However, for good checking it would be nice to increase magnetic field to 20t-40t for a more decisive conclusion. The black dots either correspond to the smaller domains with larger values of the topological integers or to some ergodic regimes occupying measure zero set on the sphere. For the final decision these experiments should be repeated and increased about 10 times magnetic field and smaller temperature like $10^{-2}K$.

Let me explain now the topological background of these results. Consider the generic Morse function $\epsilon : T^3 \to R$ and its generic nonsingular level $M_F \subset T^3, \epsilon = \epsilon_F$ in the torus and in the covering space $M' \subset R^3$. We call the surface $M' \subset R^3$ **a periodic surface**. Apply now generic magnetic field B and make the following construction:

Remove all nonsingular compact trajectories (NCT) from the periodic surface M' and its image M_F in the torus. The remaining part is exactly some surface with boundary if it is nonempty:

$$M_F \backslash (NCT) = \bigcup M_i$$

(i.e. Fermi surface minus all NCT is equal to the union of surfaces with boundary). We call these surfaces M_i and their closure below **the Carriers of Open Trajectories**. All boundary curves are the separatrix type trajectories homotopic to zero in T^3. They bound 2-discs in the corresponding planes orthogonal to magnetic field B. Let us fill them by these discs in the planes. We get closed piecewise-smooth surfaces $\bar{M}_i$. We denote their homological classes by $z_i \in H_2(T^3, Z)$.

We use the following extract from the proofs of the main theorems of Zorich and Dynnikov (see [8, 10]; their theorems have not been formulated in that way, but you may extract these key points from the proofs):

In the generic case all these homology classes are nontrivial and equal to each other up to sign $0 \neq z_i = \pm z \in H_2(T^3, Z)$ where z is some indivisible class in this group. All these closed surfaces have a genus equal to 1.

As you may see, this statement means in fact some kind of the "Topological Complete Integrability" of our systems on the Fermi surfaces for the generic magnetic field.

For obtaining our final result on the conductivity tensor, we need to use the Kinetic Equation for the quasiparticles based on Bloch waves nearby the Fermi level. This equation has been used a lot by solid state physicists for the past 30 years. For the small (but nonzero) temperature, strong magnetic field and apropriate general assumptions on the impurities, the motion of quasiparticles concentrates along the electron trajectories above. This fact leads to our conclusions. Despite the fact that this theory is considered a well established one already for many years in the physics community, any attempt to prove such things as the rigorous mathematical theorems would be a huge mess. As we see, our final conclusion is separated from all theorems by some gap which cannot be eliminated. Let me point out that it is always so. "Rigorous proofs" in mathematical physics never prove anything in real world physics.

What about nongeneric trajectories? Tsarev and Dynnikov constructed very interesting examples where genus of carriers of the open trajectories is larger than 1 (see[14]). We call such cases **stochastic**. Sometimes we call them **ergodic**. There were some attempts to extract from their properties highly nontrivial asymptotics of the conductivity tensor in the strong magnetic field [15]. However, these attempts need a better understanding of the properties of such trajectories. We have to answer the following questions:

1.How many directions of the magnetic field on the sphere S^2 admit ergodic trajectories?

According to my conjecture, for the generic Fermi surface, this set of directions has a Hausdorf dimension not greater than some number $a < 1$ on the sphere S^2. For the special Fermi surfaces $\epsilon = 0$ of the even functions like $cosp_1 + cosp_2 + cosp_3 = 0$, we expect to have ergodic trajectories for the set of directions with Hausdorf dimension like $1 < a < 2$. Dynnikov started to investigate this example in his Thesis and proved several general properties. Recently R.Deleo investigated such kinds of examples more carefully and performed more detailed calculations ([16]). His results confirm our conjectures. However, the Hausdorf dimension of this set has been unknown in this example until now.

2.Which geometric properties does "typical" ergodic trajectory have?

According to the conjecture of Maltsev, these trajectories are typically the "asymptotically self-similar" plane curves in the natural sense. His idea (if it is true) leads to the interesting unusual properties of the asymptotic conductivity tensor. Anyway, this problem is very interesting.

Dynnikov investigated also the dependence of these invariants on the level ϵ_F of the dispersion relation (see [14]). These results are useful for the right understanding of our conjectures.

Multidimensional Generalizations.

Consider the following **problem**: What can be said about topology of the levels $f(x, y) = const$ of the quasiperiodic functions with m periods on the plane x, y?

For the case $m = 3$ this problem exactly coincides with our subject above: By definition, quasiperiodic function on the plane is a restriction on the plane $R^2 \subset R^m$ of the m-periodic function. Our space R^3 was a space of quasimomenta (more precisely, its universal covering). Our plane was orthogonal to the magnetic field. Can this theory be generalized to the case $m > 3$? According to my conjecture, it can be generalized to the case $m = 4$. I think that for small perturbations of the rational directions this theory can be generalized to any value of m. We consider now any 4-periodic function $f : R^4 \to T^4 \to R$ and pair of the rational directions l_1^0, l_2^0 corresponding to some lattice Z^4 in R^4.

Let me formulate the following theorem.

Theorem. There exist two nonempty open sets U_1, U_2 on the sphere S_3 containing the rational directions l_1^0, l_2^0 correspondingly such that:

For every plane $R_l^2 \subset R^4$ from the family given by 2 equations $l_1 = const, l_2 = const$, the quasiperiodic functions f_l have only the following two *types of connectivity components of the levels $f_l = const$ on the plane R^2_l. 1.The connectivity component of the level is a compact closed curve on the plane. 2. The connectivity component of the level is an open curve lying in the strip of finite width between 2 parallel straight lines with the common direction η. This situation is stable in the following sense. After any small variations of the directions $l_1 \in U_1, l_2 \in U_2$, of function f on T^4 or the level we still have such open component with direction η'. For all possible perturbations this set of directions $\eta, \eta', ...$ belong to some integral 3-hyperplane in R^4.*

This property can be formulated in terms of the integral homology class in the group $H_3(T^4, Z)$ and of the torical topology of the carriers of the open trajectories. The idea of the proof was recently published by the author in [17].

We may reformulate this problem in terms of Hamiltonian systems. Let the constant Poisson Bracket B_{ij} be given on the torus T^m whose rank is equal to 2. Any Hamiltonian f generates such systems whose trajectories are equal to the levels of f on the planes. Our theorem means that in these cases this Hamiltinian system is Completely Integrable in the specific topological sense described above.

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