Lochak magnetic monopole as seen by an experimenter

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The story of our acquaintance with Georges Lochak appears to be typical for physicists one being a theoretician and the other being an experimenter. More than 20 years ago, Georges has created a theory that called for experimental verification, while my co-workers and me have conducted a series of experiments that required theoretical reasoning and interpretation. These two facts finally led our fates to cross each other. Although Lochak's magnetic monopole theory had emerged long before the experiments, I will first outline the experimental results, being still an experimenter.

Thus in 1998 our research group was engaged in solving an applied problem and to this end, we studied the mechanism of electric explosion of a titanium foil in water. By chance, in the mass-spectrometric analysis of the titanium powder formed upon the electric explosion, we detected a pronounced distortion of the natural isotopic composition of titanium. It is worth noting that this result was a good luck whose true significance was evaluated much later. Indeed, first of all, the use of mass spectrometry was merely accidental, its purpose being to elucidate the degree to which titanium has been oxidized rather than to study its isotopic composition. In other words, we were interested in the chemical formula of the powder formed after the electric explosion, which was either TiO or TiO_2. Second, titanium happens to have five isotopes, unlike, for example, cobalt (Co), which is monoisotopic. It is clear that with a cobalt foil, it would be impossible to detect any isotope distortions. Third, the magnitude of the effect observed in the first measurements was ten times as high as the possible error of measurements. Thus, the detected ^{48}Ti isotopic shift could not be attributed to an experimental error. It is noteworthy that the distributions of the other four titanium isotopes did not differ from natural values.

Of course, these first results were followed by tens and hundreds of additional experiments, verification and re-verification of the mass-spectrometric procedure, use of other types of mass spectrometers and different procedures. However, everything was in vain — the result fought to the bitter end like Soviet troops near Stalingrad. After half-a-year of intensive struggle against the unexpected result, our acute rejection of this result has gradually vanished and we started asking ourselves questions. If this is not a mistake, then into which have the ^{48}Ti nuclei transformed? The nuclei of what chemical element have formed instead? There we came across one more surprise. It followed from the experiment that ^{48}Ti was not converted into a particular isotope of a chemical element but was expanded into a spectrum of a dozen of other elements. This was incredible from the standpoint of nuclear physics. However, the balance of baryonic numbers of binding energies converged to an accuracy of E_b = ~10 keV/nucleon, which was quite consistent with the accuracy of measurements. Thus, it looked as if, for example, 100 ^{48}Ti nuclei gathered for some reason to form one large "nucleus" and then redistributed their nucleons in such a way as to form both lighter and heavier nuclei. In doing this, they observed also the necessary conservation laws: energy and electric, baryon and lepton charges. Thus, not only do we observe nuclear reactions at such low energies of atoms that they are not expected to occur at all but also they proceed by some channels unknown in nuclear physics. Fantastically, but this looked this way from the experimental standpoint. In addition, this "magic" transformation was not accompanied by any significant radioactivity (neutron or gamma-activity).

However, one type of detector (based on plastic scintillation counters) still did detect some radiation, though we did not know what type of radiation has induced the signals. Then we employed an old nuclear procedure used in the early days of nuclear physics. In particular, this was the nuclear emulsion method. This immediately resulted in broad traces that had not been observed on nuclear emulsions by any of specialists. This result suggested that we came across some new type of radiation.

The next step was prompted by the experiment. As a matter of fact, during these experiments we often noted some strange things with our instrumentation. For example, at high frequency of experiments, the dynamic speaker that hanged in the experimental room and was used to transmit the commands from the diagnostic room lost its "voice". If we terminated the experiment for several days, the volume of sound was restored. This is merely an observation rather than an experimental fact. However, such observations often show the right way. It is noteworthy that a permanent magnet is the key part of the speaker. We suggested that the field of the magnet had changed. This turned out to be not entirely the case, but this observation, together with a number of other observations led us to the idea to apply a magnetic field to the experimental setup.

Everything I am writing about has been stated quite strictly and consistently in our scientific publications. I describe this here only in order to demonstrate how we have arrived to Lochak's magnetic monopole theory. There is one more reason for this account. These experimental hints usually remain beyond the scope of a standard scientific publication. Only bare facts, digits, plots and estimates are to be included. Nevertheless, in my opinion, the emotional aspect of experiments is an important component of any scientific research.

Turning back to the topic of this paper, I would like to note that the application of an external magnetic field to the setup has changed qualitatively the traces that were recorded on nuclear emulsions. Thus, it became clear that "something" which leaves traces on nuclear emulsions interacts with the magnetic field. This gave the first surmise that we were dealing with the magnetic monopole. This hypothesis was attractive for one more reason. In principle, a magnetically charged particle can overcome the Coulomb barrier without having a high energy. Hence, it can interact with atom nuclei. Certainly, it still remained obscure what type of interaction was involved and whether it would be possible to explain the transformation.

This was a depressing fact, although it did not seem to be a crash for the magnetic monopole hypothesis, as I understood that if we are actually dealing with a magnetic charge, we have to study its properties experimentally. I clearly understood that this task cannot be accomplished in a day, not even in a year.

The estimation of the energy needed to generate a monopole–antimonopole pair led immediately to a catastrophe. As I have already mentioned, the observed nuclear transformation effect was macroscopic ($\sim 10^{19}$ nuclei), which required, from common sense considerations, a commensurable number of magnetic particles. However, the energy estimation carried out for classical Dirac monopoles or, even worse, for the heavy Polyakov–t'Hooft monopoles immediately buried this idea.

Thus, by mid-1999 the experiment had come to a crisis. New ideas were required and also money to continue the experiments. Both were lacking. I decided to publish the results.

After my first public reporting of the results of these experiments at a plasma physics conference in Zvenigorod in 1999 and after our first publication on this topic in scientific press, I became a laughing-stock in the scientific circles. The subsequent newsmen's publications in some central newspapers put the lid. I was announced "town's madman" at the Kurchatov Institute. There was gossip that I have run mad.

The situation around me became highly tense. A gap of misunderstanding and ill-feeling arouse also between my colleagues and me. People with whom I had worked for 25 years tried to ignore me. A cold nod — this was the most cordial salutation that I could get from them these days.

At the same time, the research team from Dubna had finished the independent verification of our results. A member of this team (prof. V. M. Dubovik), who knew that I still adhered to the magnetic monopole hypothesis, advised me to read works by G. Lochak on this topic[(1)]. In general terms, he explained me that Lochak considers the magnetic monopole as a sort of neutrino. This approach to the monopole could help me to resolve the energetic contradiction of the magnetic monopole hypothesis. This was the way I obtained the first reference to George's work.

It must be admitted that reading George's paper did not clarify for me the situation with the monopole. The reason was simple: I did not understand anything. By that time, I knew about the existence of such science as quantum electrodynamics, knew that it is based on the Dirac equation, and remembered in general terms what are the Feynman diagrams. But this was all. I had only a slight idea about spinors and bispinors. This made me turn to books. After a while, I managed to move ahead, so that I was able to formulate questions about unclear points. However, there was nobody to answer these questions. At that time (2001), I was an outlaw in the Russian scientific circles and no self-esteemed theoretician would have spoken to me seriously. This was the point where I started thinking how to get in contact with G. Lochak. Fortunately, it turned out that many people in Russia were acquainted with Georges (as he had worked in Dubna) and it wasn't difficult to get his e-mail address. I wrote a message asking Georges to send his papers. After a while, he answered being curious that somebody in Russia got interested in his theory. This was the beginning of our contact, first, only by correspondence. I asked many foolish and naïve questions concerning his theory, while Georges gave very patient and polite answers. In turn, I described him the results of our experiments and details of the procedures

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[(1)] Dubovik himself considered resorting to the magnetic monopole for interpreting the results on the transformation of chemical elements to be, at least, exotic. He is still sure that magnetic monopoles should not exist in nature (perhaps, except relict ones).

used, and made him acquainted with the results, which had not been published, as I was not confident about their validity[(2)].

Initially, I got interested in Lochak's lepton magnetic monopole theory in order to solve the energy contradiction, but I soon understood the strict logics in George's considerations. As the understanding of fine details of mathematical arguments progressed, I was more and more enthusiastic about the strictness and elegance of the theory. The only point that has remained (and still remains) obscure for me is why nobody notices this and nobody is interested in this theory. It is quite understandable that unexpected and sensational results obtained in an experiment would necessarily cast doubt. The validity of the mathematical manipulations made by Georges can be verified very easily. This was feasible even for an experimenter. However, one more aspect is whether the Nature obeys these equations or prefers some other way, unknown as yet. However, this question is not to the theory but to Nature and, hence, to the experiment. From our correspondence, we found out that G. Lochak's position in the French scientific community was better than mine in Russia but only little. French colleagues did not abuse his theory in the public press (as was done to me in Russia) but they simply ignored it. It is difficult to choose which is easier to withstand.

Indeed, if a theoretician says the words "magnetic monopole," everybody only frowns, but if an experimenter does the same, everybody is about to tear him into pieces. Why the magnetic charge hypothesis makes physicists so furious remains obscure (at least, for me).

During exchanging letters, we also found out that we recognize the same scholars of authority and read the same scientific books. We both do not appreciate scientific conferences for the waste of time. In short, we have much in common in the attitude to science. After about half a year of our correspondence, Georges decided to visit Moscow together with his charming wife Michu. I went to meet them at the airport. To my surprise, I saw Georges to be dressed just like me (despite some disparity in years). The style similarity was so striking that I noted that we look like two projections (a Russian and a French one) of something common.

The week Georges spent in Moscow was a delight for me. We spent all the time (including that meant for rest) discussing physical problems. We exchanged opinions on diverse problems of modern physics. Our wives

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(2) By the way, it should be noted that many results still have not been published, because there was no possibility to turn back to these measurements and verify the results once again.

often had to separate us because Georges needed some rest. It is noteworthy that they did it very tactfully at that time (several years ago), however, today they are much less ceremonious with us. Nothing to say — I must admit that they are right.

After the first Moscow meeting, we have met several more times but this first Moscow visit has etched in my memory most strongly. That time we chose the line of research along which we are now moving both in the theory and in the experiment.

Of course I do not think that Lochak's theory is perfect (as any other theory). Any theory is a mathematized scheme of considerations based on analysis of a finite number of observations and experimental facts. If new facts appear and require generalization, the scheme of considerations can markedly change. A vivid example is the evolution of the views on the wave and corpuscular nature of light. In this sense, all theories are local, no matter how all-embracing they seem in a given period of historical development of science.

The Lochak magnetic monopole is massless. It must be admitted that this fact causes some dissatisfaction (the author himself feels the same). However, the situation with the lepton monopole mass is in the same situation as the problem of the mass of neutrino. In my opinion, this is the central issue of the whole modern physics.

The question of whether a massless particle with a magnetic charge can exist still remains open. It is fair to say that the notion of mass does not appear to date to be ultimately solved.

I would like to discuss here one more issue. This question is often raised by the physicists who get acquainted with Lochak's theory for the first time. If everything is so simple why the Lochak monopole had not been discovered in experiments. Indeed, the search for magnetic monopoles was rather thorough. Currently, it is difficult to give an exhaustive answer to this question. Nevertheless, some considerations can be presented.

The fact that the Lochak monopole is a sort of magnetically excited state of neutrino suggests that it can originate, for example, in the beta-decay in a strong magnetic field. Why nobody detected the Lochak monopole? In my opinion, the paradox is as follows: since everybody knows that neutrino has a very small interaction cross-section, it makes no sense to measure their number from a source equal to, say, 1 Curie. Therefore, nobody performs such measurements. Neutrino are measured either from reactors or from space. In both cases, the exact number of beta-decays per second is unknown. Thus, if a small portion of neutrino originates in the magnetically excited state, nobody will notice this. G. Lochak believes that this may account for the deficiency of so-called solar neutrino.

The second consideration concerns the experimental search for magnetic monopoles. It was expected that monopole–antimonopole pairs would originate similarly to electron–positron pairs in the presence of a sufficient gamma-quantum energy. However, the result was negative. This may be due to the fact that the Lochak monopole differs from the Dirac monopole in symmetry type, while the experiments were designed for detecting the latter. This makes the negative result understandable.

If we assume that the Lochak monopoles do really arise in our electron explosion experiments, the hypothetical reason may be that their origination requires the presence of strong electromagnetic fields rather than high energies. However, this consideration is only a hypothesis. The further experiments will show whether or not this is true.