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# THE CONCEPT OF THE MONOPOLE. A HISTORICAL AND ANALYTICAL CASE-STUDY

THE FACT that there are no magnetic counterparts in nature to the isolated electric charges, has puzzled physicists for about a century. The idea that this asymmetry is only apparent and that there do, in fact, exist magnetic charges or monopoles, has gained a great deal of popularity in recent physics. For present-day high energy physicists, magnetic monopoles are almost as familiar as other respectable elementary particles. Still, the monopole is a curious kind of a particle. For it has never been detected, despite a large number of attempts to do so.

In this paper I wish to analyse the concept of the magnetic monopole from a historical and metaphysical point of view. I shall follow the development of the concept from the days of pre-quantum physics up to about the mid seventies, with special concern directed to Dirac's pioneering theory. The story of the magnetic monopole furnishes, I think, an illuminating example not only for the history and philosophy of recent physics but also for the way in which the community of high energy physicists works and reasons. Therefore, the present study aims at integrating historical, philosophical, and sociological aspects of modern physics within the same subject matter.

## 1. The Magnetic Charge before 1931

Although the idea of magnets as consisting of atoms of magnetic matter is probably very old,[1] for our purpose the question of elementary magnetic poles entered science only with the advent of the electromagnetic theories and the concept of electric charges. With the increasing recognition, in the first half of the 19th century, that electricity and magnetism are interconnected forces, the missing evidence for a magnetic counterpart to the hypothetical 'electron' could have presented a puzzling problem. But it was not regarded so, presumably because the electrodynamic theories of Ampère, Weber a.o.

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[1]Isolated magnetic elements or fluids were considered in many of the 18th century attempts to find the magnetic law of force. See J. L. Heilbron, *Electricity in the 17th and 18th Centuries* (Berkeley: University of California Press, 1979), pp. 87 – 96.

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explained the magnetic properties satisfactorily on the basis of revolving electric charges, without any need to introduce magnetic particles. In the 19th-century literature, magnetic poles were sometimes discussed, but rarely pictured as genuine isolated monopoles. Usually they were conceived to be the ends of very long and thin (dipole) magnets, in the sense applied by Coulomb in his establishment of the law of force between magnetic 'poles.'

This standard view was also adopted by Maxwell who discussed 'magnetic matter' but only, he stressed, in 'a purely mathematical sense.'[2] If magnetic matter or 'fluids' should account for the observed phenomena then, Maxwell argued, one had to introduce as an extra axiom that such matter is confined within the molecules so that macroscopic bodies containing an excess of one magnetic fluid are avoided.

With the success of Maxwell's system, and its incorporation in the electron theory in the latter part of the 19th century, the asymmetry between electricity and magnetism was given a striking expression through the fundamental field equations of electromagnetism:

$$\nabla \cdot \vec{\mathbf{E}} = 4\pi\varrho, \qquad \nabla \times \vec{\mathbf{B}} = \frac{4\pi}{c}\vec{\mathbf{j}} + \frac{1}{c}\frac{\partial \vec{\mathbf{E}}}{\partial t}, \tag{1}$$

$$\nabla \cdot \vec{\mathbf{B}} = 0, \qquad \nabla \times \vec{\mathbf{E}} = -\frac{1}{c}\frac{\partial \vec{\mathbf{B}}}{\partial t}.$$

Since $\nabla \cdot \vec{\mathbf{B}} = 0$ there are no magnetic poles in the Maxwell – Lorentz theory. This result, however, is a mathematical description of the experimental fact that magnetic charges have never been found, not a necessary prerequisite for the theory. There is nothing in the foundation of the Maxwell – Lorentz theory that precludes that magnetic monopoles might exist. This possibility was not widely discussed, however, and there were no experimental searches for monopoles in the period, as far as I know. To be sure, 'magnetic particles' or 'elementary magnets' were often discussed in classical electromagnetic theory, but usually they referred to infinitesimal magnet dipoles, not monopoles.

Two of the greatest scientists of the period, Henri Poincaré and J. J. Thomson, both examined the motion of electrical particles in fields generated

[2] J. C. Maxwell, *Treatise on Electricity and Magnetism,* third edn. (Oxford: Clarendon Press, 1891), art. 380.

by magnetic monopoles.[3] But apparently they had no confidence in the physical reality of these entities. As a curious incident one might mention that Friedrich Engels was interested in the question of isolated magnetic poles, which he used as a case in his unpublished notes on a dialectics of nature.[4]

In early Maxwellian electrodynamics, the magnetic vector potential, defined by $\vec{\mathbf{B}} = \nabla \times \vec{\mathbf{A}}$, was used as a convenient parameter in calculations but played no important role as an independent quantity. It was known, of course, that the vector potential precludes magnetic poles, since $\nabla \cdot (\nabla \times \vec{\mathbf{A}})$ is identically zero. This obvious fact that magnetic poles are irreconcilable within an electrodynamics based on the electromagnetic potentials, virtually excluded the hypothetical monopoles from the physics of the early 20th century. For the modern Hamiltonian or Lagrangian formulation of electrodynamics depends crucially on the existence of the electromagnetic potentials, not on the field quantities themselves. With the emergence of quantum mechanics, Hamiltonian and Lagrangian formulations turned out to be necessary instruments for a quantum description of the electromagnetic field and its interaction with matter. The existence of magnetic charges was therefore thought to be irreconcilable with quantum electrodynamics. This conviction was probably held generally, although not explicitly formulated. Actually, the question of monopoles was not a subject at all in quantum theory prior to 1931. The quantum physicists were not interested in monopoles and they had indeed no reason to be so: there was no evidence, either from theory or experiment, that demanded consideration of these entities. So Dirac's contribution of 1931 was not merely the offer of a solution to the monopole problem, it was to pose the problem as such in the first place.

## 2. Dirac's Theory

Dirac objected to the conventional, though tacit, assumption that magnetic monopoles are not allowed in quantum mechanics. The issue was raised in his important paper 'Quantized Singularities in the Electromagnetic Field,' dating from May 1931.[5] This paper is not only a milestone in the progress of

[3] H. Poincaré, 'Remarques sur une expérience de M. Birkeland,' *Comptes Rendus* **123** (1896), 530–533. J. J. Thomson, *Elements of the Mathematical Theory of Electricity and Magnetism* (Cambridge: Cambridge University Press, 1900), p. 396.

[4] F. Engels, *Dialectics of Nature* (New York: International, 1940), pp. 38–39. Engels argued that according to the dialectical laws all polar opposites only exist within their unity and interconnection and hence absolute isolation of opposite poles is impossible. 'Although, however, the impermissibility of such assumptions follows at once from the dialectical nature of polar opposites, nevertheless, thanks to the prevailing metaphysical mode of thought of natural scientists, the second assumption [that dividing a magnet should ultimately produce isolated north and south poles] at least plays a certain part in physical theory.' Engels' rejection of magnetic poles, first published in 1925, dates from about 1878.

[5] *Proc. R. Soc. A* **133** (1931), 60–72.

theoretical physics, it is also highly interesting from the viewpoint of the theory of science. Quite unusually for publications in physics, Dirac did not start with a presentation of the problem he was to investigate but with a long and general methodological introduction. In this introduction he discusses at length the relationship between mathematics and physics, pointing out the strategy that theoretical physicists ought to follow in their future work. Dirac's message was, briefly, that in the theoretical sciences the development of pure mathematics must take priority over attempts at empirical reasoning and physical understanding. First mathematics, then physics! was Dirac's methodological programme.

As far as new fundamental physics is concerned, Dirac stated, 'it will be beyond the power of human intelligence to get the necessary new ideas by direct attempts to formulate the experimental data in mathematical terms.'[6] Following this declaration of distrust in the inductive – empirical method, Dirac proposed as an alternative what he termed 'the most powerful method of advance that can be suggested at present.' This method was:

> to employ all the resources of pure mathematics in attempts to perfect and generalise the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities.[7]

Dirac viewed his previous theories, such as the relativistic electron theory (1928) and the hole theory (1930 – 31), as partial fulfilments of this programme. Now he wanted to apply it to yet another area, the one of magnetic poles in quantum mechanics.

In accordance with his 'powerful method of advance,' Dirac did not start out with experimental puzzles or empirical data. As mentioned, there was in 1931 no empirical evidence which indicated the possible existence of magnetic poles. Instead of following an empiricist route, he started out with mathematical deductions to which a physical meaning was to be assigned afterwards. That is, by a purely mathematical generalization of the quantum mechanical formalism he deduced that magnetic monopoles can perfectly well be included consistently in quantum mechanics. The fact that his new theory was firmly based in the fundamental theorems of quantum mechanics, was emphasized by Dirac who always considered the general principles of quantum mechanics as sacrosanct standards for theories in the micro-domain:

> The present formalism of quantum mechanics, when developed naturally without the imposition of arbitrary restrictions, leads inevitably to wave equations whose

[6] *Ibid.*, p. 60.
[7] *Ibid.*, p. 61.

> only physical interpretation is the motion of an electron in the field of a single pole. This new development requires *no change whatever* in the formalism when expressed in terms of abstract symbols denoting states and observables, but is merely a generalization of the possibilities of these abstract symbols by wave functions and matrices.[8]

Dirac's generalization was to consider the effect of introducing a non-integrable phase factor, $\gamma$, in the wave function. Although $\psi$ and $\Psi = \psi e^{i\gamma}$ give the same probability distribution (since $|\psi|^2 = |\Psi|^2$), they obey different wave equations; and Dirac showed that this difference amounts to the same effect as introducing a magnetic field. For certain cases (namely, for vanishing $\psi$ functions) he was able to relate the 'unphysical' phase factor $\gamma$ to singularities in the magnetic field and these singularities were shown to give rise to a non-vanishing flux through a closed surface. This magnetic flux was calculated to be:

$$\iint \vec{\mathbf{B}} \cdot \vec{\mathbf{n}}\, \mathrm{d}S = n \frac{hc}{e}, \tag{2}$$

where $n$ is an integer. Compared with Gauss' law in electrostatics, this was interpreted as a magnetic point charge. The strength of the magnetic charge is then quantized according to:

$$g = n \cdot g_o, \qquad \text{with} \qquad g_o = \frac{hc}{4\pi e} = \frac{\hbar c}{2e}. \tag{3}$$

The essential things in Dirac's argumentation were his proofs that (1) quantum mechanics, if only slightly generalized, formally allows magnetic monopoles; and (2) if such entities are postulated, their rule of quantization is given by equation (3), the so-called Dirac condition.

Here it might be well to emphasize that Dirac's theory did not really *predict* the existence of magnetic monopoles. What Dirac's mathematical reasoning proved was solely that monopoles are not *precluded* by quantum mechanics. No more and no less. That is, there is nothing in quantum (electro-) dynamics that *demands* the existence of monopoles in the sense, say, that Dirac's relativistic wave equation demanded the existence of positrons. While the relativistic quantum mechanics is incomplete without the incorporation of negative-energy states, to appear in the form of positrons, quantum electrodynamics is neither more, nor less, complete or consistent whether monopoles are postulated or not. So, if Dirac's 1931 theory 'predicted' the magnetic monopole (as is often stated), it did so only by a negative argument.

[8] *Ibid.*, p. 71.

In Dirac's reasoning for the possibility of monopoles, he made use of the vector potential, as usual defined by $\vec{B} = \nabla \times \vec{A}$. As mentioned, this relation is inconsistent with the assumption of magnetic poles, so Dirac's argument required a modification of electrodynamics. Dirac recognized this problem in 1931 but did not follow it up. It was only in 1948, when he substantially improved his old monopole theory,[9] that he considered the relationship of the theory to classical electrodynamics. In this improved theory, Dirac supposed $\vec{B} = \nabla \times \vec{A}$ to fail at just one point on each of the surfaces surrounding the monopole. This line of points, extending outward from the pole, is the so-called Dirac string. Dirac proposed to modify electrodynamics by adding a term to $\nabla \times \vec{A}$; the new term vanishes everywhere except in regions where the string passes.

## 3. The Arguments for the Monopole

Let us now take a closer view at the arguments which, in Dirac's and other physicists' view, justify the magnetic monopole as more than merely a hypothetical entity. As the monopole concept is not advanced by empirical data, these arguments are bound to be of a theoretical sort and are, it turns out, partly 'aesthetical.' That is, they are trans-logical and not justified according to usual scientific rationality. Largely, the belief in the monopole is motivated by three principles: (1) A principle of symmetry. (2) A wish to explain the atomicity of the electric charge. (3) A principle of plenitude.

Given the wish of bringing symmetry into the description of nature, the magnetic monopole obviously is an attractive concept. For aesthetical reasons — expectations about how the laws of nature ought to be — one would prefer electromagnetism to include monopoles, since in that case the asymmetric Maxwell equations are replaced by the symmetric, and aesthetically more satisfying, set of equations:

$$\nabla \cdot \vec{E} = 4\pi\varrho, \qquad \nabla \times \vec{B} = \frac{4\pi}{c}\vec{j} + \frac{1}{c}\frac{\partial \vec{E}}{\partial t}, \tag{4}$$

$$\nabla \cdot \vec{B} = 4\pi\varrho', \qquad \nabla \times \vec{E} = -\frac{4}{c}\mathbf{j}' - \frac{1}{c}\frac{\partial \vec{B}}{\partial t}.$$

[9] P. A. M. Dirac, 'The Theory of Magnetic Poles,' *Phys. Rev.* **74** (1948), 817–830.

Undoubtedly the conviction of an underlying symmetry in the laws of nature is an important condition for the attractiveness of the monopole concept. This argument did not play a motivating role in Dirac's original introduction of the monopole. It was not mentioned explicitly in 1931, but in the 1948 paper Dirac emphasized as a satisfying feature that 'The theory developed in the present paper is essentially symmetrical between electric charges and magnetic poles.'[10]

One thing about the monopole which definitely appealed to Dirac and motivated his theoretical search, was that the quantized monopole offered an explanation of the atomicity of the electric charge. This phenomenon, over most of its history taken to be a fundamental fact beyond theoretical understanding, Dirac now wanted to explain from deeper reasons:

> The quantization of electricity is one of the most fundamental and striking features of atomic physics, and there seems to be no explanation for it apart from the theory of poles. This provides some grounds for believing in the existence of these poles.[11]

In fact, Dirac was in 1931 not looking for magnetic charges at all. He sought for a quantum explanation of the electric charge and during this work the monopole appeared, rather as an accidental result. The attempt to explain the atomicity of the electric charge as well as the value of the elementary charge (as given by the fine structure constant $1/\alpha = \hbar c/e^2 = 137$) was at the time pursued by Eddington in particular. Although following a different line of argument, Dirac was admittedly inspired by Eddington's research programme. The fact that Dirac's original aim was not concerned with the monopole but with an explanation of the electric charge, is reflected in the title of his paper.

Because of equation (3), established by quantum mechanics, all electric charges will be quantized in units of $\hbar c/2g_o$. Dirac considered this to be a strong argument in favour of the existence of magnetic monopoles. He thought, although erroneously,[12] that his monopole theory offered the first explanation of the atomicity of electricity derived from a fundamental theory. Yet he realized that his theory did not fulfil his ambitions of giving a purely quantum based explanation of the discreteness of charges, electric and magnetic. He first hoped to be able to express $g_o$ by fundamental constants, in the same way that the elementary electric charge is expressed as $e = \sqrt{\alpha\hbar c}$ where $\alpha$ is the fine structure constant. What Dirac deduced was a reciprocal relation between $e$ and $g_o$, a result he found 'rather disappointing.'

Now, it is one thing to argue that the monopole is an attractive concept

[10] *Ibid.*, p. 830.

[11] *Ibid.*, p. 817.

[12] Only five years earlier, Oskar Klein had suggested an explanation of the quantization of electricity in terms of five-dimensional relativity quantum theory. See O. Klein, 'The Atomicity of Electricity as a Quantum Law,' *Nature, Lond.* **118** (1926), 516. Before that time there were several attempts to deduce the elementary electrical charge from the gravitation theories of Gustav Mie, Herman Weyl and Albert Einstein.

which is not precluded by any fundamental theory. It is quite another thing to assert that this theoretical possibility is also realized in nature, that monopoles do actually exist. Dirac *proved* the first thing and he *claimed* the latter. In order to transform the possibility of the monopole — its potential being — into a claim of physical existence, and to do so without empirical evidence, one has to rely on an extra-scientific principle of some sort. Dirac applied a reasoning which is a variant of the so-called *principle of plenitude.*

The principle of plenitude[13] is a metaphysical statement that what is conceived to be possible attempts also somehow to be endowed with physical reality. Through the centuries this principle has played an important role in the histories of idea and science, expounded in a variety of forms. It expresses the conviction that there is an exhaustive link between theoretical being and physical reality and in this way its acts then as a heuristic principle. If the principle of plenitude is to work as a useful scientific instrument, one has, of course, to specify what one is to mean by theoretical or potential being. Surely it should not be identified with imagined existence. Centaurs are imaginable in fantasy, but their existence is not in our physical universe. In the largely implicit use of the principle in modern physics, a phenomenon which *can* exist is preferably to be identified with one which is consistent with the fundamental laws of nature; that is, which is not ruled out by a 'principle of impotence.'[14] Such *allowed* phenomena are then thought to be real, that is, empirically recognizable, and, in a sense, also *demanded.*

This was, in any case, the way in which Dirac used the principle of plenitude about 1930, in his monopole theory as well as in his theory of positrons. At one stage during the development of the latter theory, Dirac thought that the anti-electron was realized in the ordinary proton; he therefore considered the annihilation process between a proton and an electron and argued for this hypothetical process in the following way: 'There appears to be no reason why such processes should not actually occur somewhere in the world. They would be consistent with all the general laws of nature...'[15]

In accordance with his methodology of mathematical reasoning, Dirac was inclined to think that since there is no theoretical reason barring the existence of magnetic monopoles, they would also exist in nature. 'Under these circumstances one would be surprised if Nature had made no use of it' was

[13]A. O. Lovejoy, *The Great Chain of Being* (Cambridge (Mass.): Harvard University Press, 1976). For the role of the principle of plentitude in modern science, see also L. S. Feuer, 'Teleological Principles in Science,' *Inquiry* **21** (1977), 377 – 407.

[14]These principles are general statements, asserting the impossibility of achieving something, *e.g.* the second law of thermodynamics. See E. Whittaker, *From Euclid to Eddington* (Cambridge: Cambridge University Press, 1949), p. 58f. It is a common belief that the basic laws of physics may ultimately all be expressed as postulates of impotence. Thus, 'natural laws might be compared to "proscriptions" or "prohibitions." They do not assert that something exists or is the case; they deny it.' K. R. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959), p. 69.

[15]P. A. M. Dirac, 'The Proton,' *Nature, Lond.* **126** (1930), 606.

Dirac's characteristic argument for the monopole.[16] The principle of plenitude is at the heart of the belief in monopoles as well as in the belief in other non-discovered entities (*cf.* section 10). Dirac only used the principle indirectly but later scientists have been less cautious in expounding it directly. One striking example is furnished by the following statement, due to K. W. Ford in 1963:

> One of the elementary rules of nature is that, in the absence of laws prohibiting an event or phenomenon it is bound to occur with some degree of probability. To put it simply and crudely: anything that *can* happen *does* happen. Hence physicists must assume that the magnetic monopole exists unless they can find a law barring its existence.[17]

Due to its ambiguity, the principle of plenitude has in the course of history been used as an argument for the physical existence of almost everything from mermaids to monopoles. In the 18th century, for example, the French philosopher J. B. Robinet stated it in these words: 'From the fact that a thing can exist I infer readily enough that it does exist.'[18] This, and similar statements were used to support the claimed observations of *e.g.* mermaids and sea-men: as the notion of mermaids was neither intrinsically contradictory nor colliding with current biological laws, these creatures were assumed to exist. Because, why shouldn't they? In essence, Dirac's and Ford's line of reasoning, when conjecturing the existence of magnetic monopoles, does not differ from the 18th-century arguments in favour of mermaids. Although, from one time to another, both mermaids and monopoles have been reported, neither of them have found widespread acceptance among scientists.

## 4. The Early Fate of the Monopole Theory

Dirac's audacious theory caused no widespread interest at its emergence in 1931. The reaction to the physics community was rather one of silence. In regard to the novelty of Dirac's suggestion — opening up quite new avenues in the understanding of electrodynamics and quantum theory — the cool reception was remarkable. In the thirties, the theory does not seem to have attracted much interest: it was not discussed at any of the major physics

[16] *Op. cit.*, note 5, p. 71. When Pauli and Weisskopf in 1934 quantized the Klein – Gordon equation, and showed that it describes hypothetical particles obeying Bose – Einstein statistics, they jocularly paraphrased Dirac in asking why 'Die Natur... keinen Gebrauch gemacht hat' of negatively charged bosons of spin zero. W. Pauli and V. Weisskopf, 'Über die Quantisierung der skalaren relativistischen Wellengleichung,' *Helv. Phys. Acta* 7 (1934), 709 – 731; 713. Only much later did it become known that nature does, in fact, 'make use' of these particles, since the Pauli – Weisskopf theory applies to pions.

[17] K. W. Ford, 'Magnetic Monopoles,' *Scient. Am.* **209** (December 1963), 122 – 131, 122.

[18] Quoted from Lovejoy, *op. cit.* note 13, p. 272.

conferences (such as the Solvay meetings) and it was only scarcely dealt with in the literature. Without having examined the literature in detail, I would estimate that the annual average of papers dealing with the monopole was less than one during the first twenty years of its lifetime.

Following Dirac's 1931 paper, I. Tamm solved the differential equation governing the motion of a non-relativistic electron around a monopole.[19] In Rostock, P. Jordan and his assistant B. O. Grönblom proved that the Dirac condition for monopole strength can also be derived from quantum electrodynamics.[20] Further theoretical work was done by M. N. Saha in 1936 and by M. Fierz in 1944,[21] and in 1946 P. P. Banderet treated the motion of a relativistic Dirac electron in a monopole field.[22] These theoretical examinations of Dirac's theory confirmed that the Dirac condition is a necessary and sufficient criterion for a consistent quantum theory with monopoles and thus supported Dirac's original theory. The scant interest devoted to the theory was, moreover, restricted to a purely technical level, considering it as a mathematical hypothesis rather than a statement of the constituents of nature. In the thirties and forties, Dirac's suggestion of monopoles as existing in nature was not taken seriously. With one exception (see section 5), there were no experimental searches for magnetic charges.

In 1938, Jordan again attempted a generalization of Dirac's theory. 'The assumption that such magnetic poles exist,' Jordan recognized, 'has been received with much doubt.'[23] But he justified his interest in Dirac's theory on the ground that several new elementary particles had recently been found (meson, positron, neutron) and that this would increase confidence also in the still hypothetical monopole. 'In the meanwhile, however, the number of known elementary particles has increased considerably, that one would now rather be inclined to regard the Dirac pole as a possibility worthy of serious investigation.' This argument for taking the monopole seriously was a precise anticipation of the motive which years later, in the sixties, led the physics community to a drastic re-evaluation of the monopole theory (see section 6).

The most important contribution to the theory was no doubt due to Dirac

[19] I. Tamm, 'Die verallgemeinerten Kugelfunktionen und die Wellenfunktionen eines Elektrons im Feld eines Magnetpoles,' *Z.Phys.* **71** (1931), 141 – 150.

[20] P. Jordan, 'Zur Quantenelektrodynamik, II. Eichinvariante Quantelung und Diracsche Magnetpole,' *Z.Phys.* **97** (1935), 535 – 537. B. O. Grönblom, 'Über singuläre Magnetpole,' *Z.Phys.* **98** (1935), 283 – 285.

[21] M. N. Saha, 'On the Origin of Mass in Neutrons and Protons,' *Ind.J.Phys.* **10** (1936), 141 – 153. M. Fierz, 'Zur Theorie magnetisch geladener Teilchen,' *Helv.Phys.Acta* **17** (1944), 27 – 34.

[22] P. P. Banderet, 'Zur Theorie singulärer Magnetpole,' *Helv.Phys.Acta* **19** (1946), 503 – 522.

[23] P. Jordan, 'Über die Diracschen Magnetpole,' *Annln.Phys.* **32** (1938), 66 – 70. 'Die Vermutung, dass es solche Magnetpole gäbe, ist mit grosser Skepsis aufgenommen worden. Inzwischen aber hat sich die Zahl der uns bekannten Elementarteilchen so erheblich vermehrt, dass man vielleicht jetzt eher geneigt sein wird, die Diracpole als eine ernsthafter Prüfung würdige Möglichkeit anzusehen.'

who in 1948 resumed his old interest in the monopole. Dirac's new theory was a generalization of his old one, advancing the idea of the 'Dirac string' and the formulation of the theory in classical electromagnetism by means of a variational principle. But otherwise the new theory rested on the very same assumptions as did the old one, as Dirac was not able to add new evidence for the reality of monopoles. For the sake of completeness it should be mentioned that Dirac returned to the subject of magnetic poles also in his later years. In 1975, 35 years after he first initiated the field, he surveyed his theory in connection with Price's recently announced discovery (see section 7); and in 1976 – 1978 he developed it into a model in which the magnetic charge is considered an extended particle, and deduced a conservation law for the monopole.[24]

The modest interest in the early theory of the monopole was not unnatural. For if a theory is to be considered as interesting and worthwhile to develop, it will usually be expected to fulfil at least one of two criteria: (1) It will point to anomalies within existing theories, that is, it will offer a new way of looking at problems that are not solved in these theories. (2) It will say something new about physical reality, *e.g.* predict observable phenomena outside the range of existing theories. In both of these respects, however, Dirac's monopole theory was weak. As regards the first criterion, the monopole concept proved to be entirely consistent with quantum mechanics and demanded no serious modifications of the electromagnetic formalism. The theoretical power of the monopole theory was therefore limited; it merely invited to juggle with rather special questions, such as: what will the equations of motion look like for a relativistic electron in a monopole field? Whether such questions were answered in one way or another did not really matter. The theory of monopoles seemed to have no testable consequences for other theories. As to the second criterion, the theory seemed to be ambiguous as it was not clear whether magnetic charges should be the usual sort of observable particles that were candidates of detection in practical experiments. Most physicists, probably, did not consider Dirac's entities to be serious candidates for the status of real particles and felt that, all things considered, the theory had very little explanatory power.

In 1948 Dirac repeated his 1931 main argument for the existence of the monopole, namely:

> since electric charges are known to be quantized and no reason for this has yet been proposed apart from the existence of magnetic poles, we have here a reason for

[24] P. A. M. Dirac, 'Theory of Magnetic Monopoles,' *New Pathways in High-Energy Physics,* I A. Perlmutter (ed.) (New York: Plenum Press, 1976), pp. 1 – 14. P. A. M. Dirac, 'The Monopole Concept,' *Int.J.theoretical Phys.* 17 (1978), 235 – 247.

> taking magnetic poles seriously. The fact that they have not yet been observed may be ascribed to the large value of the quantum of pole.[25]

In 1931 and 1948 he ended his papers with brief remarks about the absence of detection of magnetic charges, suggesting that it was rooted in the large numerical value of $g_o$. From equation (3) and $hc/e^2 = 137$ it is seen that $g_o$ is about 70 times the strength of the electrical unit charge, so that two elementary magnetic poles of opposite sign will be very difficult to separate. Supposing the monopoles to be constituents of the proton, Dirac estimated the binding energy of a monopole – monopole system to be of the order of 1 GeV. He also pointed out that monopoles, or particles built up of monopoles, would appear experimentally as heavily ionizing particles whose track would remain roughly constant (in contrast to ordinary charged particles which produce more ionization as they slow down towards the end of the track). This predicted feature was to be the main test in the experimental searches for the monopole which only began a decade after Dirac's second theory.

Even if Dirac thus held his theory to be about real magnetic monopoles, capable of experimental discovery, he never cared much if they were actually discovered. The point in Dirac's theory was not primarily a prediction of a new elementary constituent of our universe, but a prediction of its theoretical existence. Contingency, rather than empirical reality, was Dirac's prime interest (see also in section 7).

## 5. Felix Ehrenhaft's Approach

The only exception, to my knowledge, of an early experimentalist approach to the monopole problem, was the one of Felix Ehrenhaft. His concern with the problem was truly unorthodox and only remotely linked to the monopole *theory*. Interesting as Ehrenhaft's case is, it hardly made any impact upon the development of the monopole theory; except, maybe, in a negative sense as it may have associated the idea of the monopole with a general feeling of pseudoscience.

Felix Ehrenhaft,[26] born in 1879, at the beginning of the century had a very good reputation as a promising physicist. He made important contributions to physics, particularly to Brownian motion in gases and to photophoresis, and in 1920 was appointed director of one of the physics institutes at the University of Vienna. He is mostly known, however, for the unhappy devotion of his talent

[25] *Op. cit.*, note 9, p. 817.

[26] For Ehrenhaft's early career and the controversy concerning subelectronic charges, see G. Holton, 'Subelectrons, Presuppositions, and the Millikan – Ehrenhaft Dispute,' *Hist. Stud. Phys. Sci.* **9** (1978), 161 – 224. See also the same author's 'Electrons or Subelectrons? Millikan, Ehrenhaft, and the Role of Preconceptions,' *History of Twentieth Century Physics*, C. Weiner (ed.) (New York: Academic Press, 1977), pp. 266 – 289.

to the discovery of subelectronic charges. From 1909 onwards he made a large number of experiments on the motion of individual charged particles in electric fields in order to determine the value of the elementary charge. These measurements were made simultaneously with the famous experiments of Robert Millikan, with whom he competed openly. During his experiments, Ehrenhaft found evidence for subelectronic charges (first in 1910) and was soon involved in a controversy with Millikan and other physicists who saw no reason to reject the indivisibility of the electronic charge. As Ehrenhaft's work continued, and the dispute grew more bitter, he was led to reject atomism itself and defend a Machian anti-atomism. Ehrenhaft persisted in believing in subelectrons and insisted that his experiments were conclusive in this respect. Other physicists, however, flatly refused to take his claim seriously with the result that he was regarded by the physics community as a crank. In his lifelong controversy with the physics establishment, Ehrenhaft felt himself to be a victim of speculative standards in the methodology of physics. Always attacking hypothetical theorizing and praising the knowledge gained from direct experimental facts, he rejected quantum mechanics, the theory of relativity and most of electrodynamics.[27]

Ehrenhaft's idiosyncratic preference for the experimentalist method caused him to claim the discovery of magnetic monopoles. The experiments, on which Ehrenhaft based this claim, were of different sorts. During the thirties he made a large number of experiments with submicroscopic particles irradiated by concentrated light and placed in a homogeneous magnetic field. The particles, Ehrenhaft observed, were set in motion in and against the direction of the lines of force of the magnetic field. This phenomenon, which was discovered before the advent of Dirac's theory, he called magnetophotophoresis.[28] The motion of the particles were thus of the kind that one would expect for single magnetic charges, and magnetophotophoresis became Ehrenhaft's main argument for having discovered the monopole. '*We therefore believe that true magnetism exist*,' he concluded in 1941.[29] In other experiments,[30] he used a homogeneous electric field and observed that his test bodies moved in spiral paths, in the way electrodynamics predicts particles bearing electric *and* magnetic charge

[27]See P. Feyerabend, *Science in a Free Society* (London: NLB, 1978), p. 109. Feyerabend attended Ehrenhaft's lectures in Vienna.

[28]F. Ehrenhaft, 'Die longitudinale und transversale Elektro- und Magnetophotophorese,' *Phys.Z.* **31** (1930), 478 – 484; 'Photophoresis and the Influence upon it of Electric and Magnetic Fields,' *Phil.Mag.* **11** (1931), 140 – 146. Ehrenhaft's observations were confirmed and further studied by his assistant Ernst Reeger. See E. Reeger, 'Experimentaluntersuchungen über Magnetophotophoresis,' *Z.Phys.* **71** (1931), 646 – 657. In these early investigations, Ehrenhaft was cautious not to ascribe the observed effect to the existence of individual magnetic poles but merely mentioned that the particles behaved *as if* they were magnetic poles.

[29]F. Ehrenhaft and L. Banet, 'Is there "True Magnetism" or not?' *Phil.Sci.* **8** (1941), 458 – 462, 458.

[30]F. Ehrenhaft, 'The Magnetic Current,' *Nature, Lond.* **154** (1944), 426 – 427; 'New Evidence for the Magnetic Current,' *Phys.Rev.* **67** (1945), 63.

(dyons!) do behave.

After Dirac had put forward his theory in 1931, Ehrenhaft thought that his experiments might be a confirmation of Dirac's monopoles. He tried to obtain support from Dirac who, however, concluded that Ehrenhaft's experiments could not be interpreted as evidence for his theory and that he would not support the outcast Austrian professor. During the war, when Ehrenhaft lived in New York as a refugee, he persistently but unsuccessfully tried to convince the American physicists of his monopoles. Many times he offered to demonstrate the phenomena, revealing the existence of true magnetic charges, to members of the American Physical Society. But his efforts were completely in vain.

Except for letters and brief notices, Ehrenhaft from the mid-thirties was virtually banned from publication in the recognized physics journals. Characteristically, he was only allowed to publish one fuller report of his work on magnetic charges, and this was in a non-physics journal, *Philosophy of Science*.[31] We can get an impression of Ehrenhaft's isolation and frustration from Dirac's recollections:

> I met Ehrenhaft several times on later occasions, at meetings of the American Physical Society. Ehrenhaft was not allowed by the secretaries to speak at these meetings. His reputation had sunk so low, everyone believed him to be just a crank. All he could do was to buttonhole people in the corridors and pour out his woes. He often talked to me like that in the corridors. I formed the opinion that he was in any case sincere and honest, but he must have given the wrong interpretation to his experiments. He kept saying that he had these experimental results and nobody would listen to him.[32]

Despite Ehrenhaft's corridor attempts to associate his observations with Dirac's theory, in his publications he was cautious to avoid any references at all to current theoretical physics. In accordance with his quasi-Machian outlook he presented his claim for magnetic poles in a purely empirical way, not even mentioning Dirac's theory. Dirac, on his side, referred briefly to Ehrenhaft's work in 1948 but only to point out that the energies involved in the experiments were all much too low to have any connection with the monopole appearing in quantum theory.

Ehrenhaft returned to Vienna after the war, continuing his one-man war against the established physics and propagandizing for subelectrons and monopoles. He died in 1952 and thus did not live to experience the renewed interest in monopoles and fractionally charged particles (quarks).

[31] *Op.cit.*, note 29.

[32] P. A. M. Dirac, 'Ehrenhaft, the Subelectron and the Quark,' pp. 290 – 293 in Weiner (ed.), *op.cit.*, note 26, p. 291.

## 6. Increased Interest in the Monopole

From the scant interest devoted to Dirac's hypothetical particle during its first thirty years, the magnetic monopole gradually became a major research area. The growth in physicists' concern with monopoles is mirrored in Fig. 1.

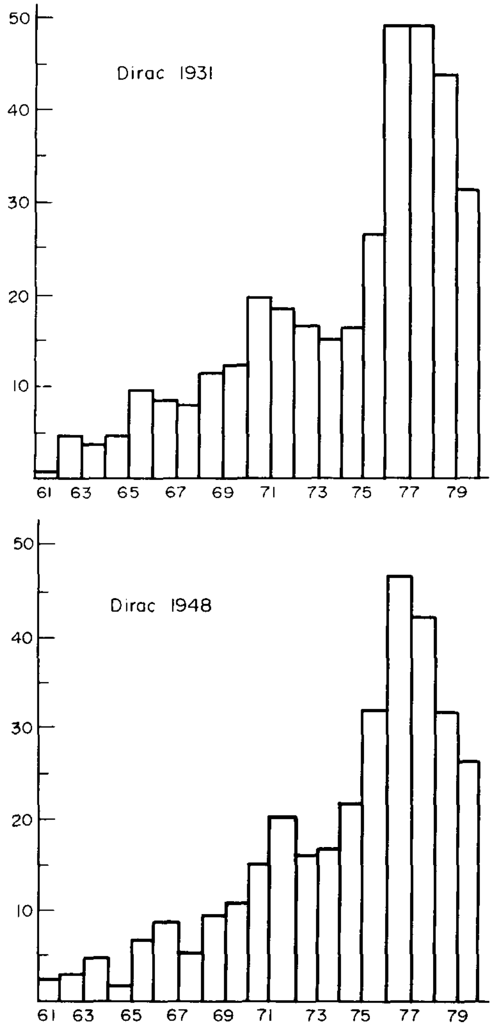


*Fig.* 1. *Number of publications citing Dirac's initial papers on magnetic monopole theory. For* 1961 – 1979, *the data are taken from* Science Citation Index. *Before* 1961, *the citing papers were less than one in annual average. The total number of monopole publications in the course of time is difficult to discover from the* Science Citation Index, *as the entry* monopole(s) *covers also items which have nothing to do with magnetic monopoles; in chemistry, acoustics, nuclear theory and antenna theory 'monopoles' also appear. However, there are only few articles on magnetic monopoles, in Dirac's sense, that do not include references to Dirac's pioneering papers, so the publication structure is probably rather similar to the citation structure, here shown.*

In the field of magnetic monopoles, Dirac's papers were no doubt the main models, serving as exemplars in Kuhn's sense. Exemplars that are characterized by both testability and broad range are supposed to lead to the most substantial growth in the field. According to Kuhn, models that are serving as exemplars are generally rejected if they cannot be tested precisely, or if they are not confirmed. In high energy physics this seems to be confirmed by Diana Crane's study of the growth and decline of the interest in various initial publications.[33] In cases where the basis for a model's appeal is its beauty, range, and internal consistency rather than its testability and contact with the real world, Crane finds that the model will at first attract a great deal of attention but then interest will steadily decline. In the case of Dirac's monopole model, things appear to be different and require some other explanation. Dirac's theory did not have the qualifications that one would expect of a strong exemplar as it was neither characterized by testability nor breadth. This situation did not change significantly during the sixties and seventies but the old model now attracted much more interest, culminating in the years 1976 – 1977. The delayed success for Dirac's theory is in itself an unusual feature, especially in such a fast-growing field as elementary particle physics; the usual pattern is an exponential decline in interest, as measured through the number of citations. The reason for the structure of citations in Dirac's model was, as I will argue, primarily a change in the conception of what elementary particles are and a result of the sensational 1975 discovery of the monopole.

The increased interest in Dirac's elusive particle may be ascribed to various kinds of reasons. Firstly, the improvements in high energy technology, both as regards accelerators and detectors, seemed to offer new possibilities of discovering the monopole. With the recently developed GeV accelerators and the bubble chamber technique, the magnetic monopole was no longer considered to be beyond practical discovery. It was now raised to the status of a 'well-known missing particle,' to be a respectable object to hunt by the fast-growing group of high energy experimentalists.

Secondly, and of course closely related to the first reason, in the fifties there was a marked change in physicists' general outlook as to what elementary particles are. With the proliferation of new particles, the interest in all kinds of exotic particles increased and the standards for accepting evidences as proofs of new particles also changed (see section 8). The explosion-like development of elementary particle physics naturally stimulated interest also in Dirac's particle.

Thirdly, this development also included new theoretical perspectives which brought the magnetic charge in closer connection with the mainstream of

[33] D. Crane, 'An Exploratory Study of Kuhnian Paradigms in Theoretical High Energy Physics,' *Soc.Stud.Sci.* **10** (1980), 23 – 54.

theoretical high energy physics. As mentioned in section 3, the original formulation of the monopole theory, as worked out by Dirac and improved by Jordan and a few others, had a rather isolated and closed character. Being launched as a corollary of general quantum mechanics, it had no specific consequences for other parts of microphysics; whether Dirac's monopole existed or not was largely irrelevant for the theories dealing with the fundamental constituents of matter.

A notable exception to this state of affairs was an idea put forward by the Indian physicist M. N. Saha as early as in 1936.[34] Saha argued that neutrons might be considered as systems of bound pairs of monopoles. This suggestion, however, published in a not widely circulated Indian journal, attracted no interest in the physics centers of Europe and America, if known at all. Saha's suggestion may be seen as an anticipation of the group of theories of hadrons (*i.e.* strongly interacting particles, such as nucleons) in which the strong interaction is ascribed to the large coupling constant of the assumed constituting monopoles. A theory of this kind, in which the monopoles are endowed with electric charges in addition to their magnetic ones, was first proposed in 1962.[35] In the late sixties Julian Schwinger[36] greatly developed the symmetric point of view according to which the basic constituents of hadrons are particles with electric as well as magnetic charge, electrified versions of Dirac's particles. These hypothetical particles were coined *dyons* by Schwinger. The new development of the monopole theory made the monopole concept attractive because it offered a natural explanation of the zero-mass for the photon. What is more important, the dyon theory, as developed by Schwinger and others, furnishes a link with the popular quark model which has served since 1963 as a reliable basis for many subnuclear theories. For according to the dyon theory, while the magnetic charge of the dyon is quantized, its electric charge is only constrained in that it must be a rational fraction of the elementary electric charge *e*. That is, the dyon may be endowed with an electric charge of ⅔ and − ⅓, as assumed in the quark model. Much of the work directed to the understanding of magnetic charges in the last 15 years has concentrated on dyons or similar schemes in which the monopole plays a role as constituent of strongly interacting matter.

The theoretical range of the monopole concept increased substantially with the new insight originating from the works of 't Hooft and Polyakov, who in 1974 suggested a gauge field theory for magnetic monopoles.[37] From this

[34] *Op.cit.*, note 21. Saha later restated his result in 'Note on Dirac's Theory of Magnetic Poles,' *Phys.Rev.* **75** (1949), 1968.

[35] C. J. Eliezer and S. Roy, 'The Effect of a Magnetic Pole on the Energy Levels of a Hydrogen-Like Atom,' *Proc.Cambr.phil.Soc. math.phys.Sci.* **58** (1962), 401 – 404.

[36] J. Schwinger, 'Sources and Magnetic Charge,' *Phys.Rev.* **173** (1968), 1536 – 1544; 'A Magnetic Model of Matter,' *Science, N.Y.* **165** (22 August 1969), 757 – 761.

[37] G. 't Hooft, 'Magnetic Monopoles in Unified Theories,' *Nucl.Phys.* **B79** (1974), 276 – 284. A. M. Polyakov, 'Particle Spectrum in Quantum Field Theory,' *JETP Lett.* **20** (1974), 194 – 195.

approach a wealth of new theoretical work has emerged, bringing the monopole into close contact with other topics in high energy theory. Despite the increase in range, this new avenue has not, however, contributed to new insights into the possible existence of monopoles but has been limited to theoretical speculations. The 't Hooft – Polyakov monopole belongs to a different research tradition than Dirac's particle and seems to have little connection with the magnetic monopoles sought by experimentalists.

Apart from Ehrenhaft's outsider attempts, already mentioned, the first experimental search for the magnetic pole took place, so far as I know, in 1951 when W. Malkus looked for Dirac's particles in cosmic radiation.[38] Since then monopoles have been looked for in a large number of experiments of great ingenuity and sophistication.[39] Many of these experiments have focused on cosmic rays as a possible source of monopoles, others have dealt with the possibility of monopoles in stable matter, including lunar soil and deep sea rocks. Still other searches have been conducted with particle accelerators, the first experiment of this sort taking place in 1959.

Probably the successful detection of the neutrino in 1956, and also of the anti-proton the year before, increased experimentalists' confidence that they would also be able to discover the monopole. The elusive neutrino was for many years thought beyond experimental detectability, if existing at all, and the 1956 discovery was therefore a great triumph for experimental particle physics. Contrary to the neutrino, however, the monopole should be an easy prey for detection. For the monopole theory ascribes some highly specific observable effects to Dirac's particle. As mentioned in section 4, the ionization track of a monopole is different from the ones produced by all other particles. Also in other respects, such as the behaviour under the influence of a magnetic field, monopoles would behave in ways which are not shared by any known particle. Therefore 'detection and identification of a monopole should be easy,'[40] all authors agree.

Despite the predicted easiness in discovering the monopole, it has in practice withstood all attacks and presents a frustrating problem to experimentalists. All that the many experiments have amounted to, is to yield upper limits for the cross-sections produced. One of the major difficulties in analyzing the experiments adequately is the lack of knowledge concerning the mass of the magnetic pole. Current theories do not provide reliable information as to this question. Most physicists believe that the monopole, if existing, is an

[38]W. V. R. Malkus, 'The Interaction of the Dirac Magnetic Monopole with Matter,' *Phys.Rev.* **83** (1951), 899 – 905.

[39]For a review of experimental searches for the magnetic pole up to 1971, see E. Amaldi and N. Cabibbo, 'On the Dirac Magnetic Poles,' *Aspects of Quantum Theory,* A. Salam and E. P. Wigner (eds.) (Cambridge: Cambridge University Press, 1972), pp. 183 – 212. See also note 41, below.

[40]*Op.cit.,* note 17, p. 127.

exceedingly massive particle, probably with a mass many times that of the proton. Another problem is that one does not know how many kinds of monopoles there are. Usually physicists talk of *the* monopole, but perhaps there is a whole family of monopoles, with different masses and charges. The perplexity of the physicists, when confronted by the experimental and theoretical situation in the search for the monopole, is well expressed in the conclusion of a 1978 review of the present knowledge of monopoles and quarks:

> ...there are no reliable calculations of production cross sections. Thus the particles could have large masses, beyond the reach of available energies, or may have extremely small production cross sections or may have been looked for incorrectly, in the wrong reactions, or at too low an energy or in a wrong kinematical region, or with poor sensitivity, or what else? Because of the great physics interest it is easy to anticipate that the search for these 'well known missing particles' will continue with experiments performed at higher energy accelerators and with higher sensitivities.[41]

Furthermore, while the monopole concept can be reconciled with non-relativistic quantum mechanics, there is doubt about the monopole's consistency within relativistic quantum field theory. Some authors claim that the monopole must be accompanied by a failure of Lorentz covariance and that there does not exist a consistent field theory of the monopole. On this view, therefore, an explanation of the apparent non-existence of magnetic charges would be that they are not, after all, permissible.

## 7. The False 1975 Discovery

On 14 August 1975, the scientific world was startled to hear at a press conference that the long-sought magnetic monopole had been discovered in an experiment carried out on 21 July. The claim was made by a team headed by P. Buford Price of the University of California, Berkeley, and the discovery was published very rapidly; submitted for publication on 4 August, the article appeared in the 25 August issue of *Physical Review Letters.*[42] This event is interesting in several respects, not least because it highlights some spectacular trends in the sociology of a 'hot' area in science.

First, what was the evidence found by Price *et al.*? Their experiment consisted in a series of balloon flights in which a detector system, made up of a

[41]G. Giacomelli, 'Searches for Missing Particles,' *Proceedings of the* 1978 *International Meeting on Frontier in Physics* (Singapore 1978), pp. 566 – 602, 599.

[42]P. B. Price *et al.*, 'Evidence for Detection of a Moving Magnetic Monopole,' *Phys.Rev.Lett.* **35** (1975), 487 – 490.

Cerenkov detector, nuclear emulsions and a stack of plastic sheets, was exposed to cosmic rays. Analyzing the tracks in the detecting device, Price *et al.* found a single track which was interpreted as being due to a magnetic (Dirac) monopole. The group found that, to interpret the event consistently with the available information, they had to conclude that a monopole with strength $g = 137e$ and speed about $\frac{1}{2}c$ had traversed the detector. As to the mass of the particle, it was estimated to exceed 200 proton masses, a value which was later changed to 600.[43] The essential point in Price's identification of the event with a monopole was the form of the ionizing track. In accordance with the predicted properties of monopoles, the event showed a heavy ionization at a constant rate as the particle slowed down through the matter. The inferred strength of $137e$ of course fitted in exactly with the Dirac condition ($g = 2g_o$). It was not, on the other hand, consistent with ideas concerning quark-like monopoles.

This, briefly, was the evidence for the discovery of the monopole. How then did physicists react to this 'discovery of the century,' as it was termed in the newspapers?

First it should be noticed that the announcement of the discovery was made very quickly after the detection of the track, and from the very beginning the discovery was turned into a media event. According to the rules of the game played by scientists, publications should not be made until their results have been analyzed carefully and discussed by fellow scientists through seminars, preprints etc. These rules were largely ignored by Price *et al.* who rather rushed for early publication (forced in part by an enterprising newspaper reporter who heard about the discovery at an early stage). The attempt to secure priority and boost the discovery to an event of public importance led to the pre-publication press conference and then to the vivid interest of the mass media. All over the world the monopole appeared in the newspapers, often followed by rather odd comments. In Walter Sullivan's account of the discovery in the *New York Times,* it was asserted that the discovery would probably lead to such remarkable applications as 'new medical therapies in the fight against diseases such as cancer, and new sources of energy.'[44] It appears that this pompous platitude originated from AIP, the *American Institute of Physics.* When trying to discover the basis of the claim, Graham Chedd got into touch with an AIP spokesman. 'His main point was that people expected you to be able to say what use a discovery might be and he was simply doing the best he could.'[45]

Indeed, this exemplifies a trend in current policy of pure science. Of course the hunt for the monopole was not in the slightest affected by potential

[43]P. B. Price, 'The Magnetic Monopole: Fact or Fiction?' *Bull.Am.Phys.Soc.* **21** (1976), 61.
[44]According to G. Chedd, 'Monopole Marvels,' *New Scient.* (11 September 1975), 607.
[45]*Ibid.*

practical applications. It was part of a strictly non-oriented science business, science for the sake of science. The only thing about the monopole discovery which transcended the disinterested pure curiosity aspect, was perhaps the desire of Price *et al.* to gain quick merit and perhaps a Nobel Prize (which, if the discovery had been accepted, no doubt would have been theirs). Therefore, the suggestions of practical applications had solely a political function, namely that of legitimating the research in question and attracting further funds to it and to similar kinds of research. It is well-known that as the pure science activities grow still more expensive and harder to justify politically by purely internal arguments, there is a tendency towards covering the real motives by hinted expectations of future applications. At the press conference following the discovery, Price mentioned (ironically?) that 'you might drive ships across the seas by putting a few monopoles in the ship and having the Earth's magnetic field tug it across the ocean'![46] The case of the magnetic monopole is only one example of the competitive and ideological spirit invading modern particle physics. As shown by Jerry Gaston, this field is heavily mixed up with competition, publicity and money-raising activities.[47]

Despite the sensation caused by the discovery, many physicists soon believed that publication was premature and that Price's monopole was not a monopole after all. Already by the end of August, at an international conference on cosmic rays in Munich, Price's discovery came under heavy attack. During the autumn of 1975 the interpretation of the experiment was severely criticized, the principal antagonists to the monopoles being P. Fowler and L. Alvarez, both leading authorities in cosmic rays and experimental high energy physics. The essence of their criticism was that Price's claim was biased, as the 'monopole' track could reasonably well have been caused by a more conventional entity, such as a platinum nucleus.[48] This alternative was not completely satisfactory; but neither was the monopole interpretation which was, it came to be shown, inaccurate on several points. The result of the controversy was that the credibility of the monopole report quickly diminished

[46] *Ibid.*

[47] J. Gaston, *Originality and Competition in Science* (Chicago: University of Chicago Press, 1973). The sociology of the discovery of the $\Omega^-$ particle in 1963, as referred by Gaston, provides an interesting example which, in several ways, is close to the monopole case. The $\Omega^-$ particle was announced at a press conference prior to its arranged publication in *Phys.Rev.Lett.* and *New York Times*. However, the news of the discovery leaked out, and this, one of the discoverers reports, 'killed our publicity.' The role of publicity in science was clearly exposed by the anonymous physicist: 'We would have got first-page *New York Times* Sunday, which is a very good thing to get. After all, where do we get national money? — from Congress. It makes a great deal of difference if we get first-page in the *New York Times* since most congressmen read the front page. It's a factor, and we can't deny it, so it matters an awful lot to us' (quoted from p. 86). See also J. Gaston, 'Secretiveness and Competition for Priority of Discovery in Physics,' *Minerva* 9 (1971), 472–492.

[48] *Cf.* A. L. Robinson, 'Magnetic Monopole Reconsidered: Another Interpretation,' *Science, N.Y.* **190** (10 October 1975), 137.

and by the end of 1975 most physicists tended to believe that the monopole had returned to its old role of a hypothetical entity. Whether believed or not, Price's report in the sequel led to work on the possible role of monopoles in new fields. The cosmological implications of assuming monopoles in the early universe have, for instance, been discussed by several authors.

The lack of confidence in Price's discovery was more based on feelings than on hard facts. Price's explanation has never been shown to be wrong, in any direct sense, but the lack of further evidence has diminished its credibility. Although many other searches for monopoles have been carried out since 1975, the event occurring in Price's experiment has never been repeated.[49] Taught by previous experience, scientists are naturally disinclined to accept a single event, however convincing it may appear, as sufficient evidence for the discovery of a new entity. Price and his team continued for some time to maintain their claim, but readily accepted that their first publication had been premature and perhaps biased. Thus in January 1976, Price admitted that 'the notoriety and pressure... made it difficult for us to carry out our research with untroubled mind,' a situation which was said to be 'a direct consequence of my own eagerness to announce what I thought was strong evidence for a monopole.'[50] Three years after their claim to the discovery, and after carrying out a new detailed analysis of the crucial event, Price *et al.* admitted that the 1975 identification was erroneous.[51] They now concluded that 'there is no justification for referring to the particle as a "monopole candidate,"' and that 'a monopole is not a good explanation.' Examining the various possibilities, *Price et al.* found that only two particles, both of them very extraordinary, could have produced the strange event. An antinucleus and an ultraheavy nucleus, of suitable charges and masses, were both found to be compatible with experimental data.

One would expect Dirac to have hailed Price's discovery, raising his old favourite invention from the status of a not too convincing hypothesis to a respectable member of the elementary particles of the physical world. That, however, was not Dirac's reaction. Reading Dirac's publications, one rather gets the impression that he never cared too much about experimental confirmation. Although Dirac accepted, as a matter of course, that physical reality can only be truly decided by empirical means, he yet seems to have been quite satisfied with the internal theoretical power of his magnetic theory,

[49]In 1977 reported evidence for the existence of fractional charges on matter was interpreted as being due to pairs of monopoles or dyons. See D. Falik and R. Opher, 'Possible Existence of Fractional Charges and Magnetic Monopoles in Matter,' *Phys. Rev. D* **18** (1977), 2694 – 2697.

[50]P. B. Price, 'Status of the Evidence for a Magnetic Monopole,' in A. Perlmutter (ed.), *op.cit.*, pp. 167 – 214; note 24, p. 212.

[51]P. B. Price *et al.*, 'Further Measurements and Reassessment of the Magnetic-Monopole Candidate,' *Phys.Rev.D* **18** (1978), 1382 – 1421.

whether confirmed or not. When Price *et al.* reported that they had detected the magnetic pole, Dirac welcomed it only rather indifferently. While Price's discovery was considered so sensational in the press and in the physics community, Dirac limited himself to the statement that there was probably a fifty – fifty chance of Price being right. And soon after he told Price that he did not believe in his discovery.[52]

This reaction indicates a relative lack of interest in empirical verification and a preoccupation with the intrinsic qualities of the theory. These characteristic features in Dirac's psychology were also manifest in 1932 when Anderson first detected the positive electron and thereby confirmed Dirac's controversial theory of 'holes.' In an interview of 1963, when T. S. Kuhn asked him whether this discovery generated great immediate excitement and satisfaction, Dirac replied: 'I don't think it generated so much satisfaction as getting the equations to fit.'[53]

## 8. When is a Magnetic Charge?

Much of the confusion of whether the monopole exists or not can be traced back to changing conceptions of which criteria should be adopted in order to state that a particle truly exists. In general, physicists have refrained from being concerned with the difficult question of existence. They have preferred to leave the matter to philosophers and have adopted a pragmatic viewpoint in which 'existence' is identified with 'detectability.'

The physicist, when working *qua* physicist, is usually a practical realist, thinking that he is manipulating real objects such as electrons, neutrons and molecules. When philosophizing, however, he often espouses what Mario Bunge has termed 'the Credo of the Innocent Physicist.'[54] According to this credo, atomic and subatomic particles are transempirical concepts which do not refer to real objects but are rather to be thought of as mathematical auxiliaries invented in order to account for observations. Most physicists would agree that we never observe isolated particles direct. What we observe is the effect of an interaction and from the experimental evidence we infer that elementary particles exist. The standard example is Rutherford's discovery of the atomic nucleus in 1911 when the existence of the nucleus was deduced from the results of scattering experiments.

Another example, of a different sort, is provided by the neutrino. Predicted

[52]P. A. M. Dirac, *Directions in Physics* (New York: John Wiley, 1978), p. 49; and *New Scient.* (21 August 1975), p. 412.

[53]Transcript of interview, *Cf.* T. S. Kuhn, J. L. Heilbron, P. Forman and L. Allen, *Sources for History of Quantum Physics* (Philadelphia: Am.Phil.Soc., 1967).

[54]M. Bunge, *Philosophy of Physics* (Dordrecht: D. Reidel, 1973), p. 2.

by Pauli in the early thirties, it was only 'observed' in 1956. But even without the harder evidence first provided by the experiment of Reines and Cowan, the early scepticism as regards the neutrino had vanished in the late forties. At that time most physicists were convinced that the neutrino existed as a real particle. The difference in opinion as to which criteria should be adopted for accepting a hypothesis as a candidate for a real particle is well illustrated by the following account from a physics colloquium from 1948, referred to by Sidney Drell:

> ...Goldhaber advised caution, ..., and emphasized the importance of looking for evidence of neutrino absorption. After all, we may see it disappear but before all doubts can be removed he advised that we should see the neutrino arrive and hit us over the head — the ultimate litmus test for the full respectability of an elementary particle. To which Dancoff replied, in the spirit of the logical positivist, that we had a respectable wave function, a dignified Dirac wave equation and the unambiguous principles of quantum theory for describing, predicting and analyzing neutrinos in beta processes. What more did we need?[55]

Today Dancoff's view, which was also shared by Dirac,[56] has predominantly been accepted by the physics community (although there are, no doubt, differences in opinion between theorists and experimentalists, the latter group sticking more to Goldhaber's conservative view).

In the course of time, the standards for accepting entities as real particles have become less severe. This change is part of the cause of the proliferation of elementary particles. It is no longer demanded that a particle shall be directly observed, 'arrive and hit us over the head,' in order to be classified as a real particle. If a mathematical entity, conceptualized as a particle, serves a heuristic purpose over a wide front (*e.g.* the hypothesis makes correct predictions, brings theoretical simplification and provides systematic organization of data) and if it is furthermore consistent with the fundamental principles of physics, then it may be elevated from being a 'hypothetical particle' to become a 'real particle.' In the history of science this kind of change often occurs; it corresponds to a change in the research policy, from considering the entities as something that could not be found in nature to something that is worth seeking. (Of course the reverse kind of change also happens, a real entity being

[55]S. D. Drell, 'When is a Particle?' *Physics To-day* **31** (June 1978), 23 – 32, 25.

[56]This view was a general theme in Dirac's thinking, appearing in his monopole theory as well as in his other theories. In 1936, Max Born portrayed what he called Dirac's l'art pour l'art attitude: 'Some theoretical physicists, among them Dirac, give a short and simple answer to this question [concerning the existence of an objective nature]. They say: the existence of a mathematically consistent theory is all we want. It represents everything that can be said about the empirical world; we can predict with its help unobserved phenomena, and that is all we wish. What you mean by an objective world we don't know and don't care.' M. Born, 'Some Philosophical Aspects of Modern Physics,' *Proc.R.Soc.Edinb.* **57** (1936/1937), 1 – 18, 12. For details of Dirac's view on physical theory, see H. Kragh, *Methodology and Philosophy of Science in Paul Dirac's Physics*, Roskilde University Centre, 1980 (mimeographed).

degraded to a mere hypothesis; the heat substance, or caloric, is perhaps an example.) The changes between the status of fictions and of candidates for reality may be due to a general shift in the conception of the possible as well as a shift in the organization of theories. Both mechanisms were effective in the change of the monopole concept, from a hypothetical entity to a serious candidate for reality.

One spectacular illustration of this willingness to interpret useful models as real particles is provided by the quark hypothesis. To the extent that quarks are accepted as real particles this is based on their strong operational credentials. Quarks are not demanded by any fundamental law in the same way that the neutrino was needed in order to account for energy conservation. A similar pattern holds for the magnetic monopole which is justified partly on aesthetical grounds (see section 3), partly on its heuristic value. That physicists have much more confidence in quarks than in monopoles, is largely because the operational credentials of the monopole are less convincing and powerful.

On the whole, there is no accepted definition in the physics community as to what an elementary particle, or just a *particle*, is. There are different views, for instance, on whether entities such as resonances and virtual particles should be classified as 'real particles.' Or, for that matter, whether the particle concept is at all suitable in microphysical theory.[57] Indeed, this paradigmatical unclearness may indicate that the entire field of high energy physics is in need of a conceptual revolution.[58] Take such an entity as the 'instanton,' a mathematical construct introduced in the theory of quarks (quantum chromodynamics, QCD). As the suffix *-on* indicates, instantons are conceived to be particle-like. But they are in a way different entities than *e.g.* protons or electrons. When physicists discuss whether instantons 'exist' or not, what they mean is whether the instanton *method* is a good one or not. Thus, in a recent article entitled 'Are Instantons Found?,' the author's criterion for existence of the instanton is whether computer experiments are in agreement with theoretical predictions.[59]

Gerald Feinberg[60] has proposed a subtle dialectical alternative to the standard criterion for existence, that of a thing existing only if it can be detected by our usual instruments. According to Feinberg, contemporary particle physics indicates the need for a distinction between 'manifestness' and 'existence.' Objects which manifestly exist are those that are observable; such

[57] As forcefully argued by Heisenberg. See his 'The Nature of Elementary Particles,' *Physics today* **29** (March 1976), 32 – 39.

[58] *Cf.* K. Shrader-Frechette, 'Atomism in Crisis: An Analysis of the Current High Energy Paradigm,' *Phil.Sci.* **44** (1977), 409 – 440.

[59] *Phys.Rev.Lett.* **44** (1980), 435 – 438.

[60] G. Feinberg, 'Philosophical Implications of Contemporary Particle Physics,' *Paradigms and Paradoxes*, R. G. Colodny (ed.) (Pittsburgh: University of Pittsburgh Press, 1972), pp. 33 – 46. *Cf.* also the same author's 'On what there may be in the World,' *Philosophy, Science, and Method. Essays in Honor of Ernest Nagel*, S. Morgenbesser *et al.* (eds.) (New York: Saint Martin's Press, 1969), pp. 152 – 164.

objects, like nuclei and neutrinos, are revealed by exchanging energy with the particles which compose our measuring devices. But the properties of manifest objects are affected by, and can only be understood through, *virtually* created and annihilated entities.[61] As a general philosophical moral we learn from modern physics that:

> the behaviour of a given system in quantum physics is influenced not only by its constituents and its surroundings but also by all other physical systems that can exist under some circumstances in this world, that is, by everything that is possible.[62]

The entities whose virtual creation and annihilation affect our ordinary particles are not revealed experimentally but would, following Feinberg, still have to be accorded physical existence. We can see that Feinberg's proposal of non-manifest existence, as 'physical systems that *can* exist,' comes close to the way of thinking involved in the old principle of plenitude (*cf.* section 3). Entities which 'can exist' or are 'possible,' that is, which are allowed by fundamental theory, are to be counted as real, though non-detectable, objects. Complementary to the actual creation and detection of particles it may, on this view, suffice to calculate how a hypothetical particle would affect ordinary particles through its virtual creation. In this way we might find that the introduction of the new particle would be consistent with known data and be valuable in the understanding of hitherto unexplained phenomena. If so, we should say that the new particle actually exists, notwithstanding its lack of manifest detection. Adopting this general viewpoint, the magnetic monopole would exist as a physical entity, quite irrespective of the outcome of experiments of the sort made by Price and other experimentalists.

But, as mentioned, physicists are rarely concerned with questions of ontology. In practice, the apparent absence of magnetic poles in nature has been answered in four different ways:

(1) The magnetic monopole probably exists, we just have to improve our search for it, apply higher energies and more sophisticated methods (*cf.* the quotation at the end of section 6).

(2) The magnetic monopole, if it exists, is endowed with extraordinary properties which make ordinary experimental work for it pointless. Within this group of answers one finds proposals that monopoles have negative mass[63] or

[61]Virtual particles are emitted from and absorbed by 'real' particles but without ever being directly observed. In nuclear forces, for example, virtual pions are exchanged between states of the nucleon, *e.g.* $p \rightleftarrows n + \pi^+$. A process as $p \rightarrow n + \pi^+$ is prohibited by energy conservation but if the pion exists virtually, that is, only in a very small period of time before it is reabsorbed, then the energy violation is justified by the uncertainty relations. For according to $\Delta E \cdot \Delta t \geqslant \hbar$ the energy is only determined within an interval of $h/\Delta t$; energy conservation will therefore not forbid the process if $\Delta t$ is sufficiently small (which is about $10^{-23}$ sec). This period of time is much too small for virtual particles to be detected by counters or track pictures.

[62]Feinberg, 1972, *op.cit.*, note 60, p. 39.

[63]F. Winterberg, 'Quarks, Magnetic Monopoles and Negative Mass,' *Lett.Nuovo Cim.* **13** (1975), 697 – 703.

that they are faster-than-light electrical particles (see section 10). Or that monopoles do not exist in isolation, pairs of monopoles being quark-like.

(3) The magnetic monopole is to be thought of only as a mathematical construct, *cf.* above. If the concept turns out to have heuristic power, then the monopole 'exists.' With this extended meaning of existence the usual attempts to detect the monopole become less interesting, if not irrelevant.

(4) The magnetic monopole does *not* exist, as the experiments have failed to provide any evidence whatsoever. Instead of searching for the monopole one should accept the evidence as a disproof of its existence. This would not lead to a problem *loss* but to a problem *shift:* if monopoles do not exist, why not? The problem shifts from experiment to theory, now demanding some sound theoretical explanation of the non-existence of magnetic poles. One attempt within this framework will be considered in the following section.

## 9. Monopoles in the Theory of Science

In section 3 it was argued that the reasons for adopting the idea of magnetic monopoles are essentially of an 'aesthetic' sort since there is no hard evidence, either experimental or theoretical, that they should actually exist. The aesthetic attractiveness of the monopole is distinctive of Dirac's pioneering works as well as in other variants of the theory. On one occasion Schwinger declared: 'How beautiful it would be if the logically sound concepts of magnetic charge and dyons should prove to be at the heart of the subnuclear world!'[64] And another specialist stated that 'monopoles were reconciled with quantum mechanics in a way so beautiful that one might say that they must be true...'[65]

Aesthetic considerations are, however, subjective by nature and therefore one should not expect the attractiveness of the monopole concept to be shared by all physicists. There are differing views as to what composes a beautiful theory. Indeed, one physicist objected to the 1975 claim of discovery because he felt that the monopole theory was *not* a beautiful theory, as it cannot be formulated as a pure action principle. This physicist, David Rosenbaum, presented the following aesthetic argument *against* the existence of monopoles:

> The arguments for magnetic monopoles are essentially aesthetic. The fact that the electric-charge – magnetic-monopole system, if it existed, would be the only classical system whose dynamical equations could not be derived from an action principle destroys any aesthetic advantage for me, and then any attractiveness to the concepts.[66]

[64] J. Schwinger, 'A Report on Quantum Electrodynamics,' *The Physicists's Conception of Nature* J. Mehra (ed.) (Dordrecht: D. Reidel, 1973), pp. 413 – 429, 426.

[65] A. S. Goldhaber, 'Electric Charge in Composite Magnetic Monopole Theories,' pp. 121 – 133 in A. Perlmutter (ed.), *op.cit.*, note 24, p. 122.

Whether being a beautiful concept or not, it is a fact that the monopole has stubbornly resisted experimental detection. Accepting this as a disproof of its existence has led some physicists to suggest that the non-existence may be due to a 'wrong' paradigm in particle physics. In this area physicists usually work within a dualistic paradigm[67] in which particles and fields are treated on equal footing. There are, however, competing paradigms which do not work with the field concept at all. According to Hoyle and Narlikar's unorthodox theory of direct interaction electrodynamics,[68] the electromagnetic field does not exist, only charged particles which interact by instantaneous action at a distance. In 1975 F. J. Tipler[69] argued that within the framework of direct action electrodynamics, the apparent non-existence of monopoles in nature is no longer a mystery; it is only in field theories of the Maxwell type, where fields are given the same ontological status as particles, that monopoles are permitted. Tipler showed that magnetic monopoles are difficult, if not impossible, to introduce in direct action electrodynamics and he thus turned the problem into a question of the validity of the standard paradigm, governing particle physics: 'If it is observed that the magnetic monopoles do not exist — and so far this is the experimental situation — then this fact suggests that perhaps electromagnetic fields do not exist either!'[70] In Tipler's view, the existence or non-existence of the monopole will provide a test between direct action theory and the usual Maxwell – Lorentz theory.

This example illustrates how a paradigm choice selects the phenomena which are scientific and those which are not. Given the particle – field paradigm, the non-existence of the magnetic pole is a profound mystery, requiring some explanation; given the direct action paradigm, the same fact is not a problem at all. The example also illustrates that paradigms themselves are not testable or subject to the outcome of crucial experiments. For even if most physicists agree that there is heavy evidence against the existence of the monopole, this is not accepted as a sufficient reason to give up or just raise doubt about, the

[66]D. Rosenbaum, *Physics to-day* **29** (December 1976), 47. In his 1948 paper, Dirac formulated monopole electrodynamics in an action principle but had to impose the constraint that nodal lines can never pass through charged particles ('the Dirac veto'). Therefore, Dirac's theory was not given by a pure action principle. See D. Rosenbaum, 'Proof of the Impossibility of a Classical Action Principle for Magnetic Monopoles and Charges without Subsidiary Conditions,' *Phys.Rev.* **147** (1966), 891 – 895.

[67]In Kuhn's scheme of a disciplinary matrix, that is the beliefs, norms, and values shared by members of a research group, the particle – field paradigm enters as an over-all metaphysical model, shared by most or all subgroups in high energy physics. Within the general dualistic paradigm, there are competing theoretical orientations in which the field concept or the particle aspect are viewed as the most important in the conception of elementary particles. For high energy physics in a Kuhnian framework, see Crane, *op.cit.*, note 33, and Shrader-Frechette, *op.cit.*, note 58.

[68]F. Hoyle and J. V. Narlikar, *Action at a Distance in Physics and Cosmology* (San Francisco: W. H. Freeman, 1974).

[69]F. J. Tipler, 'Direct-Action Electrodynamics and Magnetic Monopoles,' *Nuovo Cim.* **28** (1975), 446 – 452.

[70]*Ibid.*, p. 448.

particle – field paradigm, as suggested by Tipler. Thirdly, Tipler's argument is based on the premiss that 'if it is observed that the magnetic monopoles do not exist.' This, however, is unwarranted, for the monopole theory is in fact a non-falsifiable theory.

Dirac's monopoles can of course be verified. But any number of failed attempts to detect them will be insufficient to prove their non-existence. Dirac has never claimed that his monopoles are constituents of usual matter or specified under which conditions they should be observable. He has only argued that they exist 'somewhere in the world.' Furthermore, to satisfy Dirac's main argument for the monopole, that it implies quantization of the electric charge, the existence of only *one* monopole in the whole universe is sufficient. Due in particular to the lack of knowledge concerning the rest mass of the monopole and the kind of interactions in which it participates, the theory is empirically irrefutable. At present there is no way to decide whether the monopole does not exist. There may always be just one monopole which plays hide-and-seek with the physicists behind some distant galaxy.

In short, the monopole theory is a pure *existential statement:* 'there are magnetic monopoles.' Existential statements play an important role in Karl Popper's philosophy of science in which they exemplify non-falsifiable and therefore non-scientific or metaphysical statements. 'There is a magnetic monopole' is in exact agreement with Popper's definition of a strictly existential statement. For it 'applies to the whole universe, and is irrefutable simply because there can be no method by which it could be refuted. For even if we were able to search our entire universe, the strict or pure existential statement would not be refuted by our failure to discover the required pearl [read: monopole], seeing that it might always be hiding in a place where we are not looking.'[71] Other hypothetical particles have an epistemological status similar to the one of the monopole and are thus non-falsifiable. This, however, is seldom recognized by physicists who often argue as if monopoles, tachyons *etc.* could actually be observed not to exist.[72]

If one adopted the Popperian view, the monopole theory should therefore not be considered as a proper scientific theory. On the whole, Popperian methodology cannot account for physicists' occupation with the magnetic pole. Popper's basic rule that, as far as scientific theories are concerned, 'it must be agreed which observable situations, if actually observed, mean that the theory is refuted,'[73] was certainly not followed by Dirac. His theory was not connected with criteria of refutation and also in later developments of the

[71]K. Popper, *Conjectures and Refutations* (London: Routledge & Kegan Paul, 1963), p. 196.

[72]*Cf.* Tipler, as quoted above. Another example is provided by a recent article concerned with the existence of 'axions,' hypothetical long-lived bosons suggested by Weinberg in 1978. Examining the possible ways to detect the axion, the authors speculate on 'what are the theoretical alternatives if axions are actually found not to exist.' See T. W. Donnelly *et al.*, 'Do Axions Exist?' *Phys.Rev.D* **18** (1978), 1607 – 1620, 1615.

theory such criteria are absent. Serious objections against falsificationism have been launched from many sides and the inability of this viewpoint to cover aspects of actual science has often been pointed out. The exclusion of existential statements from the realm of respectable science is, in particular, problematic for falsificationism. To this general critique the theory of magnetic poles contributes a further example. In view of the amount of scientific work that has been directed to the field of magnetic charges, including the works of some of the most eminent physicists of our time, it seems unjustified to classify this field as 'non-scientific.'

## 10. The Tachyon, a Related Case

Many of the characteristic features of the monopole story, as examined in the preceding sections, can also be found in the case of other particles of modern physics. The so-called *tachyon*, in particular, shows a striking analogy to the magnetic monopole.

While faster-than-light particles, or tachyons, could at least be sensibly discussed in pre-relativity physics,[74] the theory of relativity exiled these particles from physics to metaphysics. Tachyons were excluded from respectable physical reasoning because they were considered as conflicting with the sacrosanct principle of relativity. In the sixties, however, physicists reexamined the matter from the viewpoint of special relativity and demonstrated that this theory does not, after all, preclude superluminal particles.[75] These hypothetical particles can be consistently described, not only within the classical theory of relativity but also, as shown by Feinberg,[76] within the framework of quantum theory. So tachyons do not involve mathematical inconsistencies, and may be reconciled with the fundamentals of physical theory, relativity and quantum mechanics.

From the fact that tachyons are not precluded by any fundamental theory, they were then assumed to exist in nature. The justification of tachyons as candidates for reality by virtue of the principle of plentitude was fully recognized by Bilanuik and Sudarshan who first invented/predicted the tachyons. In 1969 they wrote:

[73] *Op.cit.*, note 71, p. 38.

[74] Superluminal particles were discussed within the framework of classical electron theory by Sommerfeld in 1904 – 1905. Although Sommerfeld's analysis showed that particles accelerated beyond the light barrier would behave in a manifestly absurd way, 'tachyons' were not really precluded by classical theory, Sommerfeld, for instance, did not consider the idea of superluminal material bodies as an unphysical concept in itself. For references and some historical details, see L. Pyenson, 'Physics in the Shadow of Mathematics: The Göttingen Electron-Theory Seminar of 1905,' *Archs. Hist. exact Sci.* **21** (1979), 55 – 89.

[75] O. M. Bilanuik, V. K. Deshpande and E. C. G. Sudarshan, 'Meta-Relativity,' *Am.J.Phys.* **30** (1962), 718 – 723. G. Feinberg, 'Possibility of Faster-Than-Light Particles,' *Phys.Rev.* **159** (1967), 1089 – 1105. O. M. Bilanuik and E. C. G. Sudarshan, 'Particles Beyond the Light Barrier,' *Physics to-day* **22** (May 1969), 43 – 51.

[76] *Op.cit.*, note 75.

There is an unwritten precept in modern physics, often facetiously referred to as Gell-Mann's totalitarian principle, which states that in physics anything which is not prohibited is compulsory.[77]

And, since tachyons are not prohibited, nature is supposed to have filled the niche that is offered by theory. Of course this argument is completely analogous to the argument in favour of the monopole (see section 3). As Bilanuik and Sudarshan continued their statement: 'Because theory does not exclude the possibility that a magnetic analog to the electric charge can exist, physicists persist in their quest for the magnetic monopole.'

Since their 'prediction' in the late sixties, tachyons have formed a subspeciality in physics and have been investigated theoretically and experimentally by a number of authors (see Fig. 2). One trend in this discussion has dealt

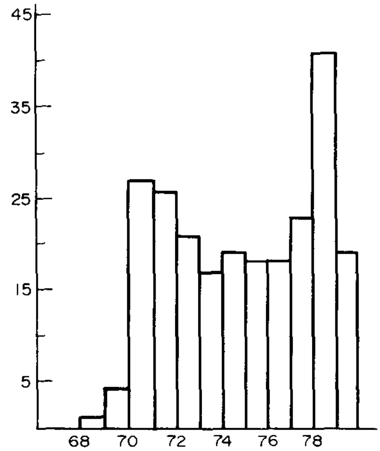


*Fig.* 2. *Number of publications on tachyons. Data taken from* Science Citation Index, *entry 'tachyon' and 'tachyons.' Before* 1968 *there are no publications listed under these entries, although a few publications dealt with the subject of superluminal velocities, initiated in* 1962. *Tachyon physics is, as the diagram shows, a small speciality, attracting only some twenty active physicists.*

with the logical consistency of the tachyon and with its philosophical implications as regards causality and the direction of time.[78] Another trend has been experimentally, in trying to establish the significance in our real world of superluminal particles. As in the case of the monopoles, tachyons are endowed with remarkable properties which *should* make them easy to detect (and, in contrast to the monopole, they should also be easy to produce). They will, if charged, emit Cerenkov radiation even *in vacuo* and will require only low energy for production. But, again, experimental searches for tachyons have

[77]*Op.cit.*, note 75, p. 44.

[78]See, *e.g.* P. Fitzgerald, 'Tachyons, Backward Causation, and Freedom,' *Boston Studies in the Philosophy of Science,* VIII, R. C. Buck and R. S. Cohen (eds.) (Dordrecht: D. Reidel, 1971).

only yielded negative results. Curiously, the detection of tachyons was reported in 1974,[79] one year before Price's false monopole discovery. However, the claim turned out to be too hasty and was soon withdrawn.

The properties of tachyons, if they exist, are even less well-known than those of monopoles. Current theory gives no unequivocal information about their mass, spin, charge, coupling, or of other characteristics and is thus of little help in guiding experimentalists' search. The tachyon theory is another example of a strict existential statement, namely 'there exist particles which move faster than light.' And again this implies that the theory is non-falsifiable. As recognized by Feinberg who first coined the term 'tachyon' '...whereas an experiment with a positive result could establish the existence of tachyons, a negative result could at best establish an upper limit for the rate at which tachyons are produced...'[80]

The similarity between monopoles and tachyons is perhaps not limited to the meta-level. Some authors have speculated that tachyons are in fact magnetically charged. It turns out[81] that, while the special theory of relativity does not explicitly predict the existence of either (subluminal) monopoles or tachyons, it explicitly predicts the existence of tachyonic monopoles. That is, if the special theory of relativity is rebuilt without assuming *a priori* that any material velocity is subluminal, then it can be shown that the electromagnetic field equations are fully symmetric, with superluminal electric charges behaving as magnetic monopoles. If $\varrho^*$ and $\mathbf{j}^*$ refer to the superluminal charges, the 'Maxwell equations' appear in the monopole form (*cf.* equation (4)):

$$\nabla\cdot\vec{\mathbf{E}} = 4\pi\varrho, \qquad \nabla\times\vec{\mathbf{B}} = \frac{4\pi}{c}\vec{\mathbf{j}} \quad \frac{1}{c}\frac{\partial\vec{\mathbf{E}}}{\partial t},$$

$$\nabla\cdot\vec{\mathbf{B}} = -4\pi\varrho^*, \qquad \nabla\times\vec{\mathbf{E}} = \frac{4\pi}{c}\vec{\mathbf{j}}^* \quad \frac{1}{c}\frac{\partial\vec{\mathbf{B}}}{\partial t}.$$

A number of theorists have investigated the idea of tachyon monopoles and several experiments have been performed in order to detect these particles, the first one in 1972.[82] All attempts have however failed.

[79] R. W. Clay and P. C. Crouch, 'Possible Observation of Tachyons Associated with Extensive Air Showers,' *Nature, N.Y.* **248** (1974), 28 – 30. The first systematic search for tachyons took place in 1963, see 1963 *Annual Report of the Nobel Research Institute,* Stockholm, pp. 95 – 97.

[80] G. Feinberg, 'Particles that Go Faster than Light,' *Scient.Am.* **222** (February 1970), 69 – 77, 72.

[81] E. Recami and R. Mignani, 'A New Experimental and Theoretical Outlook on Magnetic Monopoles,' *The Uncertainty Principle and Foundations of Quantum Mechanics,* W. C. Price and S. S. Chiddick (eds.) (London: John Wiley & Sons, 1977), pp. 321 – 324. Not all physicists agree in the conclusion that superluminal electric charges will appear as magnetic monopoles to a subluminal observer. For an opposing view, see L. Marchildon *et al.,* 'Electrodynamics and Tachyons,' *Nuovo Cim.* **53B** (1979), 253 – 280.

[82] D. F. Bartlett and M. D. Lahana, 'Search for Tachyon Monopoles,' *Phys.Rev.D* **6** (1972), 1817 – 1823.