Stefan Marinov

THE THORNY WAY
OF TRUTH

Part VIII

Documents on the violation of the laws
of conservation

⊂EST-OVEST→
Editrice Internazionale

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THE THORNY WAY OF TRUTH

OF TRUTH

Part VIII

Documents on the violation of the laws of conservation

ЕСЛИ ТЕБЕ, ПОЭТ, ГОЛОС ДАН -
ОСТАЛЬНОЕ ВЗЯТО.

Марина ЦВЕТАЕВА

ПОКА ВЫ, АБСОЛЮТИСТИКИ, НА НАРАХ
ОТЛЕЖИВАЛИСЬ, МЫ ГОТОВИЛИ ПЕРЕСТРОЙКУ.

Релятивист-либерал, уверовавший
в эфир, после того как ОБСОЛЕННЫМ
пальцем ткнул в раны Спасителя

OGNI TEORIA COMPLICATA È SBAGLIATA.

ВСЕ МЕНЯЕТСЯ, ТОЛЬКО УСПЕВАЯ ПОВОРАЧИВАТЬСЯ.

Релятивист-либерал, после подписания
указа о помиловании узников эфира

СКОЛЬКО ВЕРЕВОЧКЕ НЕ ВИТЬСЯ, А КОНЦУ БЫТЬ.

Из сентенций высказанных
Альбертом Эйнштейном на смертном ложе

ДРУГ ДРУГА ЕДИМ - И ТЕМ СЫТЫ.

DIE ERKENNTNIS EINER EINZIGEN TATSACHE NACH
IHREN URSACHEN ERÖFFNET UNS DAS VERSTANDNIS
ANDERER ERSCHEINUNGEN OHNE ZURÜCKGREIFEN
AUF DIE ERFAHRUNG.

Galileo Galilei

ОДНАЖДЫ Я ПРОЧЕЛ ВИКТОРУ МАКСИМОВИЧУ НОВОЕ
СТИХОТВОРЕНИЕ. ВЫСЛУШАВ МЕНЯ, В. М. ПРОСТО-
ДУШНО СКАЗАЛ: "ИСТИНА НЕ МОЖЕТ БЫТЬ СТОЛЬ
ДЛИННОЙ."

Фазиль Искандер

Il Signor GENIO TEORICO e la Signorina ESPERIMENTALINA.

ПЕРДИСЛОВИЕ

(FARTWORD)

> Tuttavia, poichê non voglio fidarmi
> troppo di me stesso, io non asserisco
> nulla qui, e sottometto tutte le mie
> opinioni al giudizio dei più saggi ed
> all'autotitã della chiesa. Anzi prego
> i lettori di non prestare nessuna fede
> a tutto quanto troveranno, e non amet-,
> tere che quanto la forza e l'evidenza
> della ragione li potrã forzare a cre-
> dere.
>
> Cartesio
>
> Le ultime parole nel suo saggio:
> "I principi della filosofia"

Although the eighth part of THE THORNY WAY OF TRUTH (TWT) is dedicated predominantly to the Ampere-Grassmann controversy, I think that the dispute on this controversy must be closed, as my magnetic experiments with interrupted circuits, which violate the angular momentum conservation law (the Bul-Cub Machine without Stator - TWT-III, and the Rotating Ampere Bridge with Displacement Current - TWT-IV), have shown that Ampere's formula is WRONG, taking into account that this formula does not allow a violation of Newton's third law.

Nevertheless the experimental and theoretical study of the magnetic interactions (i.e., the interactions between moving charges) remains further a highly interesting topic. Let us not forget that the interactions in two of the machines violating the energy conservation law which were constructed by me (the machine ADAM - TWT-II, i.e., the cemented Faraday disk, or the so-called N-machine of Bruce de Palma, and the machine MAMIN COLIU - TWT-III) were magnetic. The latest information which reached me from Bill Müller from the Canadian British Columbia (I met Bill and his wife in Hannover in 1987 and his charming daughter Carmen in Einsiedeln in 1989, where she reported on the work of her father) is that Müller is on the threshold of closing the energetic circle in his over-unity converter and thus to run it as a perpetuum mobile. The inter-actions in Müller's machine are also magnetic.

In the present volume I reprint my paper "Absolute and relative Newton-Lorentz equations" which was recently published in PHYSICS ESSAYS (this paper was published first in TWT-IV, p. 101). This theoretical paper and my experimental paper "Action of constant electric current on electrons at rest due to the absolute velocity of the Earth", published in TWT-IV, p. 110, are decisive for recognizing and accepting the absolute character of the magnetic phenomena, i.e., that the magnetic effects depend on the absolute velocities of the electric charges.

After the publication of Prof. Rindler's paper in the AMERICAN JOURNAL OF PHYSICS, 57, 993 (1989), reprinted in TWT-VII, p. 219, and especially after the publication of Dr. Maddox' conundrum in NATURE on the 12 July 1990 (see it reprinted in this volume), the problem about the absolute character of the magnetic phenomena and of the violation of the principle of relativity in electromagnetism reached wide circles of the scientific community (let me note that Dr. Maddox' conundrum had to appear in NATURE in December 1988 as a "Christmas puzzle").

The reaction of the "relativists" to this conundrum is presented by Dr. Bunting in NATURE, 23 August 1990 (see this note reprinted in this volume). I should like to point out here the reaction of the "anti-relativists", presented by Prof. Petr Beckmann.

I publish here the note which Prof. Beckmann wrote in the November /December issue of his journal GALILEAN ELECTRODYNAMICS:

DISSIDENT NEWS

pace-Time Conference in Leningrad: :all for papers

The Technical University of Leningrad, the Academy of Civil ,viation (also in Leningrad), and the Geographical Society of the ،cademy of Sciences of the USSR are sponsoring an International :onference "Problems of Space and Time in Natural Science."

The conference is to be devoted to the following issues on suc-essive days:

• Coordinates and time in astrometry, celestial mechanics nd mathematics;

• Coordinates and time in physics, astrophysics, and cosmol-gy;

• (2 days) Coordinates and time in physics, geophysics, and eology; critical analysis of 20th century theories; experimental est;

• general discussion; scientific eth.ics.

Historians of science and philosophers interested in these topics re also welcome.

Prospective speakers should submit a summary in English or lussian to the

> Local Organizing Committee
> Intl. Space-Time Conference 1991
> P.O. Box 16
> Leningrad 198097, USSR

The Local Organizing Committee is chaired by Prof. P.F. Par-;hin, head of the Physics Department of the Academy of Aviation ;n Leningrad. Other Soviet organizers include physicist Prof. 3.I. Peshchevitsky (Novosibirsk) and astronomer Dr. S.A. Tol-:hélnikova (Leningrad); all of these have published papers critical)f modern physics. At press time, an International Scientific Drganizing Committee was also being set up.

Please watch this column for further details as they emerge. Scientists wishing to participate, and especially prospective speakers should not delay preparations, as communications with he USSR are still extremely slow — in our experience, about one nonth for an air mail letter to be deliv.red.

The Alleged Conundrum

In a page-long article "Stefan Marinov's seasonal puzzle," in *Nature* (12 July 1990), the editor of that journal John Maddox reports on what he considers a conundrum: (essentially) a magnet rotating about the axis of symmetry of its field with a conductor in that field stationary in the lab, as against the case when the mag-net is stationary and the conductor rotating. In the present case, dissident physicist Stefan Marinov has chosen the configuration so cleverly that he need only ask whether a voltage will be induced. Both "yes" and "no" will contradict Einstein.

Maddox correctly comments that [Einstein's] relativity theory predicts no difference, but then says "What is the truth? Nobody is quite sure, for nobody has done the experiment — not even Marinov." But unless he refers to that particular configuration of the general setup for unipolar induction, this is quite incorrect.
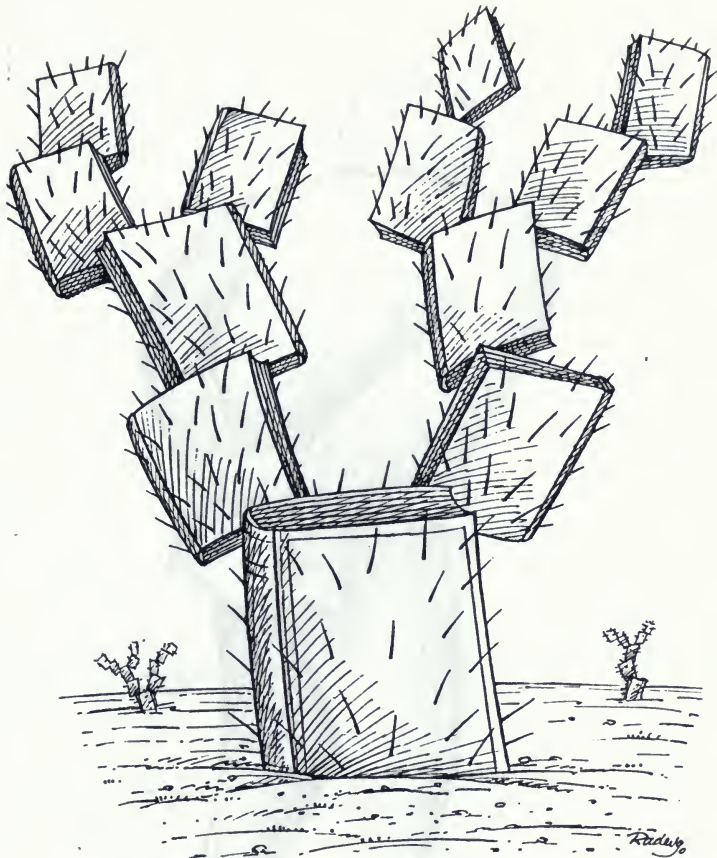
Though Maddox is by training a physicist, he can hardly be blamed for not knowing about F. Müller's unipolar induction experiments, described in the May/June issue of this journal, and probably the most complete series on the subject. But he does not know about Faraday's experiments on unipolar induction in 1831, either. Familiarity with either would quickly tell him what is going on and what the answer to the "conundrum" is. All he need have done was to ask whether the field rotates with the mag-net when the latter rotates about its axis of symmetry.

Faraday's 1831 experiment. is not all that well known, either, but the unipolar induction effect in general is common knowledge, and when a physicist who is editor of one of the world's major scientific journals writes an editorial on the sub-ject, one would expect him to use its considerable advisory resources.

Even more embarrassing, perhaps, is the fact that Maddox has fallen victim to the baseless dogma that Einstein's observer-based relativity and absolute space are the only possible alternatives. Here I do not refer to any particular contemporary dissident theory, but to the fact that for unaccelerated frames the Principle of Relativity was known to Galileo, was quantitatively formulated by Newton in the *Principia* (Book 1, Corollary 5), and was never doubted by any classical physicist, whether he believed in absolute space or not. (A rotating magnet is not an unaccelerated frame — though Müller has shown the effect for uniform motion also; but Maddox invokes the Relativity Principle anyway.)

It is remarkable to what extent this fundamental principle of physics has been replaced by the Einstein-or-absolute-space dichotomy in the consciousness of physicists in general, many of them working much closer to the subject than an editor. But then, it is remarkable to what extent experimental evidence by such classics as Faraday or Michelson (I refer to the 1924 Michelson-Gale experiment) has simply disappeared from basic (and often even specialized) textbooks when it refutes the dogmas of ortho-doxy.   *P.B.*

With. this note I reprinted also the information on the Soviet anti-relativity Confe-rence presented in GALILEAN ELECTRODYNAMICS, so that the readers of TWT may become aware of this important scientific meeting which, together with the conference organi-zed by Prof. Bartocci in May 1991 on the Italian island Ischia,will sign, I hope, the death act of the theory of relativity.

This drawing, as well as all drawings in the present volume signed by "Radev", are of my Sofia neighbour and friend, Milen Radev, one of the most talented young Bulgarian cartoonists. - Accidentally I counted the number of the "thorny books" in the above "book cactus". It was 12. It became clear for me that the volumes of THE THORNY WAY OF TRUTH will be also 12. That's a good number!

To a certain extent Prof. Beckmann derides Dr. Maddox of being ignorant about certain elementary physical facts, as Dr. Beckmann writes:

> Though Maddox is by training a physicist, he can hardly be blamed for not knowing about F. Müller's unipolar induction experiments, described in the May/June issue of this journal, and probably the most complete series on the subject. But he does not know about Faraday's experiments on unipolar induction in 1831 either. ... when a physicist who is editor of one of the world's major scientific journals writes an editorial on the subject, one would expect him to use its considerable advisory resources.

I should like to emphasize that Dr. Maddox learned about F. Müller's experiments in 1984, i.e., five years before Prof. Beckmann has heard about them, as Dr. Maddox is a passionate reader of TWT. Moreover, in 1988 Dr. Maddox paid my trip to London, so that we can calmly and in detail discuss Faraday's , Müller's and my experiments (which we have largely discussed during my previous visits in 1985, 1987 and 1988). Finally, the RIGHT
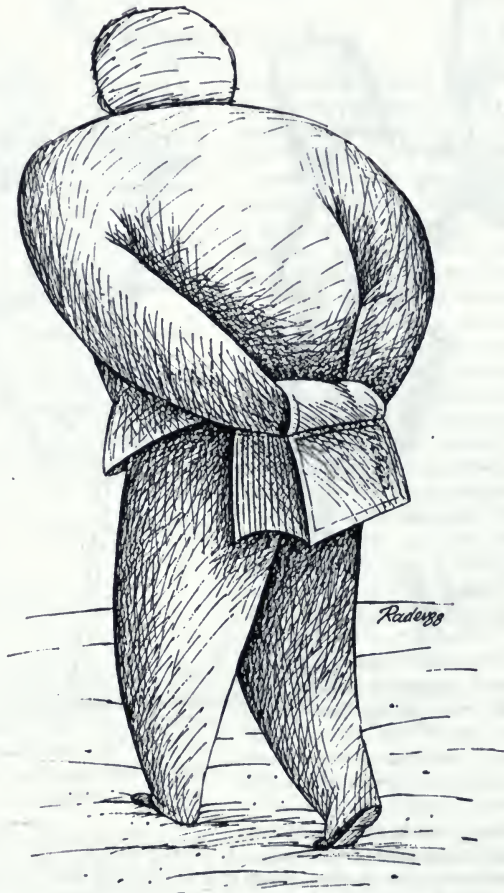
answers of Dr. Maddox about the effects in the rotational Kennard experiment (i.e., in the rotational variation of the conundrum, which is the only one discussed and analyzed by Prof. Beckmann) are given in his letter to me of the 8 March 1985 and are published on p. 296 of TWT-II.

Dr. Maddox is an old fox and he is not as ignorant as Prof. Beckmann thinks. And it sounds somewhat comical when Prof. Beckmann tries to explain the solution of the ROTATIONAL conundrum by the following words:

All he (Dr. Maddox) need have done was to ask whether the field rotates with the magnet when the latter rotates about its axis of symmetry.

During our long conversations Dr. Maddox has perfectly well understood that there is NO field. Dr. Maddox has very well understood that in electromagnetism there are only electric charges, their absolute velocities and their distances to the reference point at the moment of reference which determine their electric and magnetic potentials. And Dr. Maddox has understood that there is NOTHING ELSE.

Because aware of all of this, Dr. Maddox asks VERY CAUTIOUSLY what will occur if one cuts a long quasi-rectangular loop from the double circular wires when assuming their radii to be big enough. Then at a motion of the loop also "its magnetic field" has to move, and   the effects will be not the same as for the rotational case. And as Dr. Maddox has realized that it is IMPOSSIBLE to bring these two controversial conclusions under one hat, he concluded: "There is a puzzle!" Thus the real puzzle is not to give predictions to the rotational variation but in the impossibility to bring by the most simple logic the results of the inertial and rotational variations under one hat without violating the principle of relativity. In his letter of the 8 March 1985 Dr. Maddox gave his prediction also about the inertial Kennard experiment (carried out by me!) which was WRONG. As I have derided him in my letter of the 15 March 1985 (see TWT-II, p. 304) for his stupid answer   on which any CHILD will laugh, now Dr. Maddox does not give a prediction and leaves this task to the readers of NATURE.

I am sure that Prof. Beckmann also has posed the question about the effects in the inertial Kennard experiment, hasn't he. And as Prof. Beckmann also was afraid to give his prediction, he did not mention the inertial variation of Kennard's experiment in his note. Thus, Dr. Maddox, the editor of one of the most CONSERVATIVE scientific journals in the world, is hardy enough and POSES the unpleasant question about the effects in the inertial Kennard experiment, while Prof. Beckmann, the fighter against the Gods, puts this question under the rug. So, I am asking Prof. Beckmann: Which will be HIS prediction for the effcts in the inertial Kennard experiment, but I am afraid that he will not dare to give his answer on the pages of GALILEAN ELECTRODYNAMICS, as he, surely, will realize that by giving the only logically possible answer, he will be impelled to recognize the failure of the principle of relativity and he will be impelled to accept that the Newton-Lorentz equation in a laboratory moving in absolute space must have the form given by me in the mentioned above paper published in PHYSICS ESSAYS.

The unshakable belief of Prof. Beckmann in the validity of the principle of relativity is expressed in his words from the above note:

> I refer ... to the fact (my emphasis - S.M.) that for unaccelerated frames the Principle of Relativity was known to Galileo, was quantitatively formulated by Newton in the *Principia* (Book 1, Corollary 5), and was never doubted by any classical physicist, whether he believed in absolute space or not.

At the present time there are only two physicists in the whole world who believe in absolute space but who not only that do not believe in the principle of relativity, but show by experiments that this principle is WRONG. These two physicists are Prof. God of the Champs-Elysees University and the Oberstallknecht Stefan Marinov from the stable for saddle-horses in Niederschöckl.

There are a couple of physicists who also assert that the principle of relativity is wrong (Wesley, Parshin, Efimov, Tolchelnikova) but nobody of them has done experiments IN THE LABORATORY for demonstrating its invalidity. So Efimov and Spitalnaia in thier article "Об анизотропии вспышечной и пятнообразовательной деятельности Солнца в инерциальном пространстве" /ФИЗИЧЕСКИЕ АСПЕКТЫ СОВРЕМЕННОЙ АСТРОНОМИИ, издание Главной Астрономической Обсерватории, Ленинград, 1985, стр. 147/ report on their registration of the absolute motion of the Sun system; for the equatorial coordinates of the apex of the Sun's absolute velocity they have obtained $\delta = -3^o$, $\alpha = 11^h$ (compare these figures with the equatorial coordinates of the apex of the Earth's absolute velocity in February obtained by the help of my "coupled shutters" experiment, $\delta = -24^o$, $\alpha = 12.5^h$ (TWT-II, p. 68), and with the right ascension of the apex of the Earth's absolute velocity in January obtained by the help of my inertial Kennard experiment, $\alpha = 11.8^h$ (TWT-IV, p. 110)). However, the "laboratory" of Efimov and Spitalnaia was the whole Sun system.

The only other man who has claimed of having observed violation of the principle of relativity by having done experiments in the laboratory was E. W. Silvertooth with his quasi-Wiener experiment (Spec. Sc. Techn., **10**, 3 (1987); Electronics and Wireless World, p. 437, May 1989). By duplicating this experiment in the proposed by me quasi-Michelson variation, I found null result and I drew the conclusion that also Silvertooth was UNABLE to measure the absolute laboratory's velocity by THIS kind of experiment.

I wish to conclude by turning reader's attention to my letter to Prof. Beckmann of the 2 August 1990 (published in this volume).
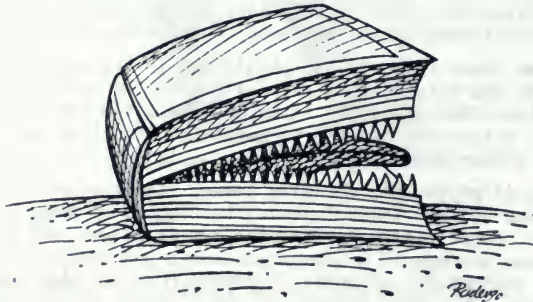
This letter remained unanswered as, obviously, Prof. Beckmann did not wish to publish my comments on Dr. Maddox' conundrum entitled "How Dr. Maddox blabbed out the secret about the goat's ears of King Albert", where I point out certain unpardonable LIES printed in Dr. Maddox' article.
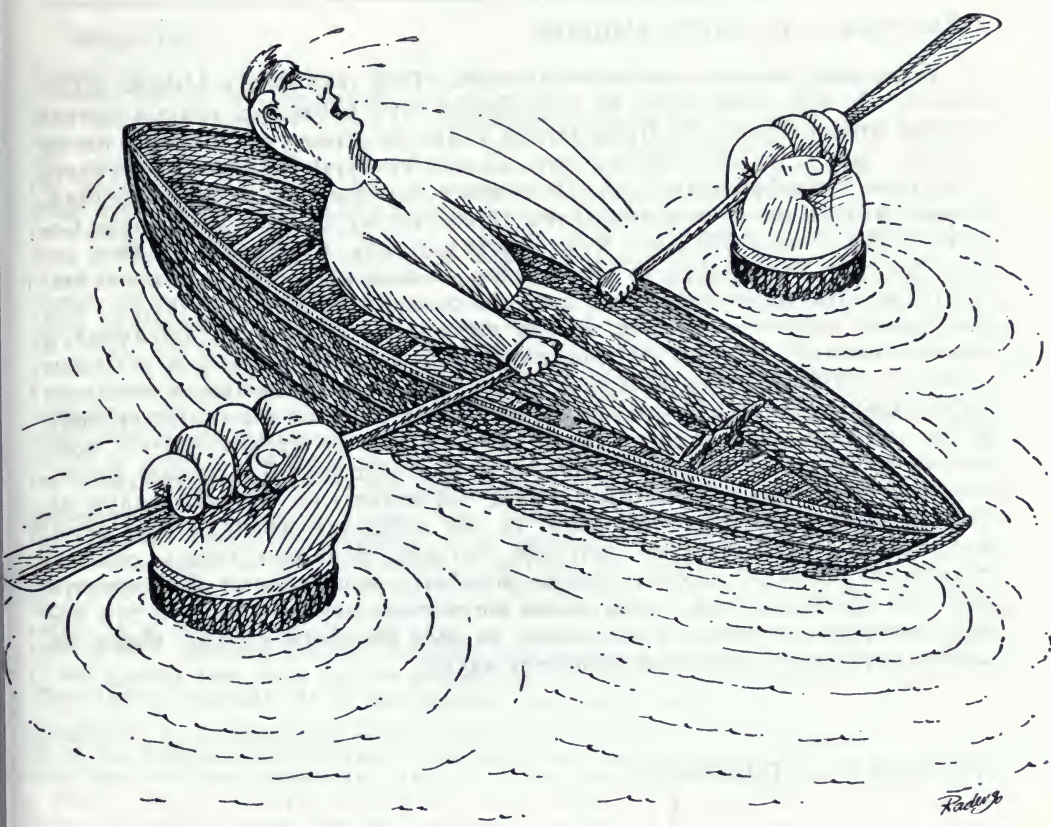
Surely, it became clear to Prof. Beckmann that by publishing my comments on the pages of his journal, he will be impelled to start not only with the discussion about the invalidity of the principle of relativity, but also with the discussion about the invalidity of the laws of conservation. One has to consent that the discussion of this topic on the pages of GALILEAN ELECTRODYNAMICS will be a too hard nut even for the teeth of Prof. Beckmann.

However, as Prof. Beckamnn has raised on the banner of his journal the saint and eternal Galilean principle "dubitare, sperimentare, provare", I should suggest to him to visit the Christian community METHERNITHA in the village Linden in Switzerland and to see with his own eyes the first perpetuum mobile on this planet. Unfortunately, as his journal has not the circulation of NATURE, if then he will publish a photograph of himself in front of the machine TESTATIKA with the declaration "TESTATIKA is not a perpetuum mobile", I shall not pay to him Ł 10,000.

Graz, 20 November 1990   Stefan MARINOV

ВЕСТНИК 7

Radu jo

ВЕСТНИК 7
АКАДЕМИИ НАУК СССР

НАУЧНЫЙ И ОБЩЕСТВЕННО-ПОЛИТИЧЕСКИЙ ЖУРНАЛ ПРЕЗИДИУМА АН СССР

СОДЕРЖАНИЕ

Годичное Общее собрание АН СССР

Академик А. Д. АЛЕКСАНДРОВ

Надо защитить академию от нападок. Возьмите газету «Наука в Сибири». В ней, вероятно, по невежеству публиковались статьи против теории относительности. Посредством таких печатных органов мы науку ставим под удар. Другой пример. В «Литературной газете» появилось интервью с профессором А. А. Денисовым, который, по моим сведениям, является чуть ли не председателем Комиссии по этике в Верховном Совете СССР. Это интервью — чудо безграмотности и безобразия. Оно демонстрирует, что профессор совершенно не понимает теорию относительности, о которой говорит. При этом он ссылается на Л. Д. Ландау, как на своего предшественника. Много рассуждает на тему плюрализма и борьбы мнений. Но как не понимать того, что плюрализма в обычном смысле в науке быть не может, а может быть борьба мнений, основанная на различной трактовке фактов. В науке есть знания, и бессмыслен спор о том, что дважды два — четыре. То, что в «Литературной газете» появилось интервью, а давший его человек занимает пост — вызывает беспокойство. И я что-то не слышал, чтобы представители Академии наук на Съезде народных депутатов СССР (а мы выбирали от Академии наук, например, В. Л. Гинзбурга, который, надеюсь, понимает теорию относительности) заявили протест против подобных выступлений. Это чревато тем, что провоцируется новый поток поношения науки. А ведь и так положение трудное, только и слышишь: во всем виноваты ученые. Наша же задача — утвердить высокий авторитет науки.

Академик В. Л. ГИНЗБУРГ

Действительно, председателем Комиссии по этике избран профессор Денисов, который является врагом теории относительности. И когда мне предложили войти в состав этой комиссии, я отказался, узнав, что профессор Денисов будет ее возглавлять. Я известил руководство Верховного Совета о том, что выбирать председателем Комиссии по этике человека, который является в каком-то смысле врагом науки, занимает столь лженаучные позиции, недопустимо.

Будет большим счастьем, если в этических вопросах профессор Денисов поведет себя не столь некомпетентно, как в научных. Пока оснований для оптимизма мало. Прочтите интервью Денисова в «Литературной газете»: оно не выдерживает критики и с моральной точки зрения.

Академик В. И. ГОЛЬДАНСКИЙ

Мне представляется, что пункт 35 следовало бы целиком исключить. В этой связи я вспоминаю один случай, возможно, что это легенда. Однажды П. И. Мусхелишвили спросили, правда ли, что он бывший князь? Николай Иванович сказал: «Как может быть бывший князь? Ведь не может же быть бывший пудель!» Этот ответ целиком можно применить к нам: как это может быть бывший академик?

TRANSLATIONS

Academician A. D. Alexandrov. We must defend the Academy of attacks. Take the journal "Science in Siberia". Because of ignorance, articles against the theory of relativity have been published there. Via such printed organs (vehicles) we set the science under strokes. Another example. "The Literary Journal" published an interview with Prof. A. A. Denissov, who, according to my knowledge, is, it seems, a president of the comission on ethics in the Soviet Supreme of USSR. This interview is a blatant alanphabetism and mischief. It shows that the professor does not understand at all the theory of relativity, about which he speaks. Moreover, he refers to L. D. Landau as to his predecessor. He speculates too much on the topic of pluralism and the encounter of opinions. But how can one not understand that a pluralism in the common sense cannot be in science; it can be there only encounter of opinions based on different treatment of the facts. There is a body of knowledge in science, and the dispute whether two plus two are four is there senseless. The fact that "The Literary Journal" has published this interview and that the person who has given it is at a high position gives rise to concern. And I have not heard that the deputies at the Soviet Supreme from the Academy of Sciences have presented protests against such interventions (we have elected from the Academy, for example, V. L. Ginsburg who, I hope, understands the theory of relativity). This is a precedent which provokes new defamation of science. The situation is in any case difficult. One hears from everywhere: the scientists are guilty for all. Our scope is to strengthen a high authority of science.

Academician V. L. Ginsburg. Indeed, Prof. Denissov, who is an enemy of the theory of relativity, was elected for president of the Commission on ethics. When I have been invited to become a member of this Commission, I declined the invitation, by learning that Prof. Denissov will be the head. I informed the ruling body of the Soviet Supreme that it is inadmissable to elect as a president of the Commission on ethics a man who, in a certain sense, is an enemy of science and stays on anti-scientific positions. It will be a great luck if Prof. Denissov will act in ethics not as incompetently as he acts in science. But at the present time there are few grounds for optimism. Read the interview of Denissov in "The Literary Journal"; it is unacceptable from a moral point of view.

Academician V. I. Goldansky. I think that article 35 must be thoroughly canceled (article 35 of the Academy statute states that a member of the Academy who has lost Soviet citizenship loses also his academician status). In this connection I remember a story, perhaps a legend. Once N. I. Muskelishvili was asked, whether it is true that he is an ex-prince. Nicolai Ivanivitch said: "How can be there an ex-prince? Is there an ex-poodle?" This answer can be attributed thoroughly to us: is it possible to be an ex-academician?

Marinov's question: Is it possible to be there an ex-relativist?
                    Oppure: È possible incontrare un relativista pentito?

THESE ARE THE INTERVENTIONS OF ACAD. ALEXANDROV, THE EX-PRESIDENT OF THE SOVIET ACADEMY OF SCIENCES, AND OF ACAD. GINSBURG, THE TOP-THEORETICIAN IN SOVIET PHYSICS, AT THE ANNUAL MEETING OF THE SOVIET ACADEMY OF SCIENCES 1990. WHEN ONE HEARS GINSBURG'S CATEGORIZATION "ENEMY OF SCIENCE", ONE THINKS ON STALIN'S "ENEMY OF PEOPLE AND... МУРАШКИ ПО КОЖЕ ПРО-БЕГАЮТ.

Stefan Marinov with father Marin (born 1896) and son Marin Jr. (born 1958)
in front of his native house in Sofia, Bulgaria, 1973.

SCIENTIFIC   PAPERS

EXACT CALCULATION OF THE PUSHING FORCE WHICH ACTS ON THE AMPERE BRIDGE

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Morellenfeldgasse 16
A-8010 Graz, Austria

Abstract. When calculating in Ref. 1 the pushing force which acts on an Ampere bridge, a diverging integral has been obtained and only an approximate value could be given. In the present paper I show how the calculation of the pushing force can be carried out mathematically exactly, but the result, again, remains dubious.

I have calculated in Ref. 1 the pushing force which acts on a U-form Ampere bridge with infinitely long legs (Fig. 1). I showed that this force is independent of the radius of curvature, R.

However, the rather simple calculation led to an integral which was divergent. I was unable to solve the problem exactly and I found for the pushing force the approximate value

$$f_A > 0.34 \ \mu N/A^2. \tag{1}$$

Now I found an elegant way to carry out an exact calculation of this pushing force.

First I shall briefly remember the way of calculation followed in Ref. 1.

The potential magnetic force with which a current element Idr' acts on another current element Idr, according to Grassmann's formula,[2] is

$$df = (\mu_0/4\pi)I^2 dr\times(dr'\times r)/r^3 = 10^{-7} \ I^2\{dr'(dr.r) - r(dr.dr')\}/r^3, \tag{2}$$

where r is the oriented distance from dr' to dr.

It is easy to see that the pushing force will be the projection of the net force along a direction parallel to the legs, while the net projection of the force along a direction perpendicular to the legs, because of the symmetry, will be zero. Thus, denoting the parallel and perpendicular components of dr, dr' and r respectively by $dr_\parallel$, $dr_\perp$; $dr'_\parallel$, $dr'_\perp$; and $r_\parallel$, $r_\perp$, we can write for the component of the elementary force (2) along a direction parallel to the legs, by taking $I = 1$ A,

$$df_A = 10^{-7} \ \frac{dr'_\parallel(dr_\parallel r_\parallel + dr_\perp r_\perp) - r_\parallel(dr_\parallel dr'_\parallel + dr_\perp dr'_\perp)}{(r_\parallel^2 + r_\perp^2)^{3/2}}. \tag{3}$$

Let us calculate first the force with which the currents in both legs act on the current in the half-circular shoulder. For this case we have to put in (3)

$$dr = Rd\beta, \qquad dr_\| = R\cos\beta\,d\beta, \qquad dr_\perp = R\sin\beta\,d\beta,$$

$$dr' = (R/\cos\alpha)d\alpha, \qquad dr_\|' = Rd\alpha/\cos^2\alpha, \qquad dr_\perp' = 0, \qquad (4)$$

$$r_\| = R\sin\beta + R\tan\alpha, \qquad r_\perp = R - R\cos\beta.$$

Putting (4) into (3), integrating for $0 < \alpha < \pi/2$, $0 < \beta < \pi$, and taking into account that there are two legs, we obtain

$$f_A' = 2\times10^{-7}\int_0^{\pi/2}\int_0^{\pi}\frac{\cos\alpha\,\sin\beta(1 - \cos\beta)\,d\alpha\,d\beta}{(1 + 2\sin\alpha\cos\alpha\sin\beta - 2\cos^2\alpha\cos\beta + \cos^2\alpha)^{3/2}}, \qquad (5)$$

and we thus see that the pushing force acting on a U-form Ampere bridge does not depend on the radius R of the half-circular shoulder.

This integral has been calculated on a computer[1] and its value is

$$f_A' = 0.1222 \ \mu N/A^2. \qquad (6)$$

Let us now calculate the pushing force caused by the action of the current elements in the shoulder on the current elements in the shoulder. Taking (Fig. 2) the line elements dr' and dr at two arbitrary positions of the half-circle, we have to put in (3) the following expressions for the parallel and perpendicular components of dr, dr' and r, and then integrate formula (3) for $0 < \alpha < \pi$, $0 < \beta < \pi$,

$$dr = Rd\beta, \qquad dr_\| = dr\cos\beta = R\cos\beta\,d\beta, \qquad dr_\perp = dr\sin\beta = R\sin\beta\,d\beta,$$

$$dr' = Rd\alpha, \qquad dr_\|' = dr'\cos\alpha = R\cos\alpha\,d\alpha, \qquad dr_\perp' = dr'\sin\alpha = R\sin\alpha\,d\alpha, \qquad (7)$$

$$r_\| = R(\sin\beta - \sin\alpha), \qquad r_\perp = R(\cos\alpha - \cos\beta).$$

Dividing the domain of integration into four domains, we shall have

$$f_A'' = \int_0^\pi\int_0^\pi = \int_0^{\pi/2}\int_0^{\pi/2} + \int_0^{\pi/2}\int_{\pi/2}^\pi + \int_{\pi/2}^\pi\int_0^{\pi/2} + \int_{\pi/2}^\pi\int_{\pi/2}^\pi = 2\int_0^{\pi/2}\int_0^{\pi/2} + 2\int_0^{\pi/2}\int_{\pi/2}^\pi =$$

$$\times10^{-7}\int_0^{\pi/2}\int_0^{\pi/2}\frac{\cos\alpha\{\cos\beta(\sin\beta - \sin\alpha) + \sin\beta(\cos\alpha - \cos\beta)\} - (\sin\beta - \sin\alpha)(\cos\alpha\cos\beta + \sin\alpha\sin\beta)}{\{(\sin\beta - \sin\alpha)^2 + (\cos\alpha - \cos\beta)^2\}^{3/2}}\,d\alpha\,d\beta +$$

$$2\times10^{-7}\int_0^{\pi/2}\int_0^{\pi/2}\frac{\cos\alpha\{-\cos\beta(\sin\beta - \sin\alpha) + \sin\beta(\cos\alpha + \cos\beta)\} - (\sin\beta - \sin\alpha)(-\cos\alpha\cos\beta + \sin\alpha\sin\beta)}{\{(\sin\beta - \sin\alpha)^2 + (\cos\alpha + \cos\beta)^2\}^{3/2}}\,d\alpha\,d\beta$$

$$= (10^{-7}/\sqrt{2})\int_0^{\pi/2}\int_0^{\pi/2}\frac{\sin\beta\,d\alpha\,d\beta}{\{1 - \cos(\alpha-\beta)\}^{1/2}} + (10^{-7}/\sqrt{2})\int_0^{\pi/2}\int_0^{\pi/2}\frac{\sin\beta\,d\alpha\,d\beta}{\{1 + \cos(\alpha+\beta)\}^{1/2}} = f_{A1}'' + f_{A2}''. \qquad (8)$$

The second integral has been calculated on a computer and its value is

$$f''_{A2} = (10^{-7}/\sqrt{2}) \int_0^{\pi/2}\int_0^{\pi/2} \frac{\sin\beta \, d\alpha \, d\beta}{\{1 + \cos(\alpha+\beta)\}^{1/2}} = 0.2493 \; \mu N/A^2. \quad (9)$$

However the first integral

$$f''_{A1} = (10^{-7}/\sqrt{2}) \int_0^{\pi/2}\int_0^{\pi/2} \frac{\sin\beta \, d\alpha \, d\beta}{\{1 - \cos(\alpha-\beta)\}^{1/2}} \quad (10)$$

is improper as for $\alpha = \beta$ it has a peculiarity and thus it can be not calculated on a computer. Moreover, I shall now show that the first integral is converging to infinity.

Indeed, let us choose some specific value for α (α = Const) and let us calculate the integral depending on β, denoting it by $\Phi(\alpha)$. For simplicity let us assume $\alpha = \pi/2$. In such a case the integral depending on β will be

$$\Phi(\pi/2) = \int_0^{\pi/2} (1 - \sin\beta)^{-1/2}\sin\beta \, d\beta = \quad (11)$$

$$\sqrt{2}\ln\{\sqrt{2} + (1+\sin\beta)^{1/2}\} - (\sqrt{2}/2)\ln(1-\sin\beta) - 2(1+\sin\beta)^{1/2}\bigg|_0^{\pi/2}.$$

The last integration is complicated but the reader can easily persuade himself that the integration is right, as if he will differentiate the expression on the right of (11), he will obtain the integrand in the integral on the left.

All terms in the expression on the right are limited when the limits 0 and $\pi/2$ will be substituted except for the term $\ln(1-\sin\beta)$; by substituting here $\beta = \pi/2$, we obtain $\ln(1 - 1) = \ln 0 = -\infty$, so that the integral $\Phi(\pi/2)$ is tending to $+\infty$.

After considering the case from a critical <u>physical</u> point of view, I found the following way for calculating the pushing force f''_{A1}:

Let us put two concentric half-circles from both sides of the initial half-circle drawn in Fig. 2, respectively with radii $R + xR$ and $R - xR$, where x is a quantity small with respect to 1. Let currents $I/2$ flow along any of these half-circles in parallel to the current I flowing in the initial half-circle. One can easily realize that the force pushing the U-form Ampere bridge will be the <u>sum</u> of the forces with which the currents $I/2$ in any of the secondary half-circles act on the current I in the initial half-circle when x tends to zero.

Making calculation similar as above, we find that the force f_{A1}'' is the sum of the following two integrals, for the case when x tends to zero,

$$f_{A1}'' = 10^{-7} \int_0^{\pi/2} \int_0^{2\pi/2} \frac{(1+x)\sin\beta\{(1+x) - \cos(\alpha-\beta)\}d\alpha\,d\beta}{\{1 + (1+x)^2 - 2(1+x)\cos(\alpha-\beta)\}^{3/2}} +$$

$$10^{-7} \int_0^{\pi/2} \int_0^{2\pi/2} \frac{(1-x)\sin\beta\{(1-x) - \cos(\alpha-\beta)\}d\alpha\,d\beta}{\{1 + (1-x)^2 - 2(1-x)\cos(\alpha-\beta)\}^{3/2}} = (f_{A1}'')' + (f_{A1}'')''. \qquad (12)$$

The integrals have been calculated on a computer and the results are presented in the following table

TABLE 1

x	$10^7 \times (f_{A1}'')'$	relative error	$10^7 \times (f_{A1}'')''$	relative error	$10^7 \times f_{A1}''$
10^{-1}	3.5061390851	3.6×10^{-10}	- 1.9021946978	5.0×10^{-10}	1.6039443873
10^{-2}	3.8599812048	5.1×10^{-9}	- 2.2886125609	9.6×10^{-9}	1.5713686439
10^{-3}	3.9179560837	9.5×10^{-9}	- 2.3471516064	1.9×10^{-8}	1.5708044773
10^{-4}	3.9258568313	6.6×10^{-11}	- 2.3550602887	1.6×10^{-10}	1.5707965426

We thus see that when $x \to 0$ the sum of the integrals $(f_{A1}'')'$ and $(f_{A1}'')''$ tends to a limit value which, obviously, is the number $\pi/2 = 1.5707963$. I must however emphasize that for $x = 0$ the integrals (12) are equal to the integral (10) and their value is $+\infty$.

Thus, for the force f_{A1}'' we can accept the number

$$f_{A1}'' = 0.1571 \ \mu N/A^2, \qquad (13)$$

and taking into account (6), (13) and (9), we conclude that the pushing force acting on a U-form Ampere bridge with infinitely long legs does not depend on the radius of curvature and is equal to

$$f_A = f_A' + f_A'' = f_A' + f_{A1}'' + f_{A2}'' = 0.5286 \ \mu N/A^2 \cong 0.5 \ \mu N/A^2. \qquad (14)$$

Now the problem is to be posed: can we consider the above calculation of the integrals as exact? The answer is: The calculation, surely, is exact and it is admissible that the limit of the sum of the integrals (12) for $x \to 0$ will be the number (13), but whether this calculation will correspond to the physical reality can be established only by the experiment, as I have used an artificial model which may lead to wrong

physical conclusions.

All experimentalists who have measured the pushing force acting on a Π-form Ampere bridge with different character and geometry of the sliding contacts (Moyssides and Pappas[3], P. Graneau and P. N. Graneau,[4] Peoglos[5], Marinov[6]) have received figures higher than 0.5 μN/A^2. The most careful experiment, according to me, has been carried out by Peoglos[5] and his figure, if extrapolated to a Π-form bridge with very long legs, is $f_A = 1 \pm 0.05$ μN/A^2.

My experiment was done very quickly with a primitive technique and its eventual measuring error was pretty big[6]; the figure obtained by me for the pushing force acting on my Π-form bridge was $f_A = 5 \pm 2.5$ μN/A^2.

With my Rotating Ampere Bridge with Sliding Contacts[6,7], I measured the pushing force acting on a Π-form Ampere bridge indirectly and the figure obtained was a little bit less but near to $f_A = 5$ μN/A^2.

Now the following very important question is to be answered: Will the force pushing an Ampere bridge with very long legs depend on the form of the bridge?

If we would assume that the above calculation of the force acting on a U-form Ampere bridge corresponds to physical reality, then the answer will be: yes. Indeed, let us make a Π-form bridge with length of the shoulder, a, connecting it to the two very long legs with two fourth-circular arcs of small radii R. The pushing force acting on this bridge will be bigger than the force with which the legs act on the shoulder; the last force, for $I = 1$ A, is[6,7]

$$(f_A)_{shoulder} = (\mu_0/2\pi)\ln(a/R). \tag{16}$$

It is clear that, at suitably small R, one can make always

$$(f_A)_{\Pi-form} > (f_A)_{shoulder} > (f_A)_{U-form} = 0.5 \text{ μN/A}^2. \tag{17}$$

For example, if $R = e^{-5}a = 6.7 \times 10^{-3}a$, what is an easily realisable bridge, say $a = 1000$ mm, $R = 6.7$ mm, we shall have

$$(f_A)_{shoulder} = 2 \times 10^{-7}\ln(e^5) = 1 \text{ μN/A}^2. \tag{18}$$

Thus, by making a rectangular loop, the one end of which has a U-form, we should be able to set this loop in motion by the action of its internal forces, as the pushing forces on its Π- and U-form extremities will be not equal.

I did such an experiment (figs. 3, 4). 1000 windings of a wire with diameter of 0.6 mm have been wound on a square frame with a side a = 34 cm, the one part of which had a U-form with radius of curvature R = 17 cm. By applying the mains, an alternating current of about 4 A flew through the windings.

The calculation which I shall make is very approximate, but there is no necessity to make a more exact calculation. Assuming that current of 4000 A flows in a single-wire Π-U-form circuit, we shall have for the force acting on the U-form "bridge" (see formula (14)) $F_{U-form} = I^2 f_A = 8$ N. For the force acting on the Π-form "bridge", we shall have, assuming that the middle radius of curvature of the fourth-circular arcs at the angles is $R_o = 1$ cm (see formula (16)) $F_{\Pi-form} = 2\times10^{-7} I^2 \ln(a/R_o) = 11.3$ N. Thus, there must be, according to this calculation, a net force acting in the Π-form direction $F_{net} = F_{\Pi-form} - F_{U-form} = 3.3$ N.

The frame was suspended by strings on the ceiling, but not the slightest motion could be observed although, by applying a force even of milli-Newtons during the half periods of the own oscillations of the system, pretty big oscillations could be observed.

Thus, according to my opinion, the calculation (12), leading to the result (13), can be not considered as corresponding to physical reality.

The following speculation, too, makes the computer calculatios in table 1 very suspicious: It seems that, at $x \rightarrow 0$, not only the sum of $(f_{A1}'')'$ and $(f_{A1}'')''$ tends to the limit $(\pi/2)10^{-7}$, but also the single forces $(f_{A1}'')'$ and $(f_{A1}'')''$ converge to certain final numbers, as their increase (in absolute value) is very slow with the decrease of of x. On the other hand, however, we have

$$\int_0^{\pi/2}\int_0^{\pi/2} \frac{(1+x)\sin\beta\{(1+x) - \cos(\alpha-\beta)\}}{\{1 + (1+x)^2 - 2(1+x)\cos(\alpha-\beta)\}^{3/2}} > \int_0^{\pi/2} \frac{\sin\beta\{1 - \cos(\alpha-\beta)\}}{2\sqrt{2}\{1 + x - \cos(\alpha-\beta)\}^{3/2}} > \int_0^{\pi/2} \frac{\sin\beta}{2\sqrt{2}\{1 - x - \cos(\alpha-\beta)\}^{1/2}}$$
$$(19)$$

and the limit value to which the last integral converges, at $x \rightarrow 0$, surely is $+\infty$. We have then further to conclude that the limit of the integral $(f_{A1}'')''$ will be $-\infty$. Their sum may be the finite number $\pi/2$ but this, according to me, is not sure.

Thus the result (13) remains physically highly suspicious and the different limits of the integrals (12) calculated by computer and by the speculations (19) are to be considered as results of mathematical jugglery with expressions which have peculiarities.

Thus I am inclined to conclude that, neither the pushing force acting on a Π-form bridge, nor the force acting on a U-form bridge can be calculated mathematically. And taking into account the negative result of my experiment presented in figs. 3 and 4, I make the general conclusion: the pushing force acting on the Ampere bridge does not depend on its form. This conclusion, of course, has to be confirmed by careful experiments with U-form and Π-form bridges with very long legs, to see whether, indeed, the pushing forces will be equal. Then I think that the pushing force will depend neither on the size of the bridge.

The pushing force acting on the Ampere bridge can be found also proceeding from the formula for the inductance of the loop: If L is the inductance of the circuit, then the magnetic energy of the circuit when current I flows through it is[8] (p. 363)

$$W = (1/2)LI^2. \tag{20}$$

The force acting on the circuit at change of its characteristic size, a, will be

$$f = \partial W/\partial a = (1/2)I^2(\partial L/\partial a). \tag{21}$$

The inductance of a square loop with side a and radius r of the wire is[5]

$$L_{square} = 8 \times 10^{-7} a\{\ln(2a/r) + r/a + \sqrt{2} - \ln(1 + \sqrt{2}) - 7/4\} = 8 \times 10^{-7} a\{\ln(2a/r) + r/a - 1.217\}, \tag{22}$$

while the inductance of a circle with diameter a and radius of the wire r is[8] (p.361)

$$L_{circle} = 2\pi \times 10^{-7} a\{\ln(2a/r) + \ln 2 - 7/4\} = 6.28 \times 10^{-7} a\{\ln(2a/r) - 1.057\}, \tag{23}$$

and we can accept $\partial L_{square}/\partial a \cong \partial L_{circle}/\partial a \cong$ Const. This conclusion can serve as a mathematical confirmation of the assertion that the pushing force on the Ampere bridge depends neither on its form nor on its size. (N.B. According to me, the term "r/a" in formula (22) is due to wrong calculation.)

I should like to note the way on which one comes to formula (21):

The magnetic flux Φ produced by a circuit with inductance L is[8] (p. 354)

$$\Phi = L I, \tag{24}$$

and as at the motion of the Ampere bridge the induced back tension will be $U_{back} = d\Phi/dt$, the power taken from the battery generating the current I will be

$$P_{batt} = U_{back}I = I^2 dL/dt. \tag{25}$$

As the power increase of the magnetic energy of the loop is

$$P_{magn} = dW/dt = (1/2)I^2(dL/dt), \qquad (26)$$

we have to conclude that the mechanical power acquired by the Ampere bridge will be

$$P_{mech} = P_{batt} - P_{magn} = (1/2)I^2 dL/dt. \qquad (27)$$

This formula leads to formula (21), as $f = (P_{mech}dt)/da$.

Here, however, I have a very serious objection: When the radius of the wire, r, tends to zero, L must tend to infinity and thus P_{magn} and P_{mech}, consequently also f, must tend to infinity, too. The last conclusion can be not reconciled with Grassmann's formula and with the results presented in table 1 which give a finite limit for f_A when $r \to 0$ (i.e., when $x \to 0$). Careful experiments are needed to establish whether at $r \to 0$ there is also $f_A \to 0$. On the other hand, if it will be true that the Ampere force f_A depends on the wire's radius r, then making an experiment similar to that shown in figs. 3 and 4, where the wires at the one end of a prolongated loop are thin and at the other thick, one will set it in motion, violating thus Newton's third law.

Moyssides and Pappas[3] have tried to establish an experimental relation between r and f_A but their results are too crude and indefinite, and no conlcusion can be made. I consider the experimental answer for the behaviour of f_A when $r \to 0$ as very important.

At any rate the following general conclusion can be made: Nature surely will to con- lead tradicting predictions if the   integral (10) and the expression (16) at $R \to 0$ will be finite numbers. Thus Nature "saves" Grassmann's formula by the mathematical jungles to which it leads when one tries to make exact mathematical calculations.

And another remark: In this paper I speak only about Grassmann's formula without mentioning the rival Ampere's formula[6] (p. 322). The reason is that my Bul-Cub Machine without Stator[6] (p. 322) has demonstrated that Ampere's formula is wrong.

At the end I have to add that, perhaps, not only the magnetic (Grassmann) forces act on the Ampere bridge but also the introduced by me[2] "current jet forces", whose essence is the following: The electrons flowing in an Ampere bridge change their momenta from $m_e v$ to $-m_e v$, where m_e is the electron's mass and v is their energy velocity[2]. If a current of $I = 1$ A flows through an Ampere bridge, i.e., if 1 C of electricity traverses the bridge in 1 sec, taking into account that the electron charge is $q_e = 1.6 \times 10^{-19}$ C, one sees that $N = 1/q_e$ 6.25×10^{18} electrons will traverse the bridge in a second. Consequent-

ly the pushing force due to the "current jet" effect will be (for a current of I = 1 A)

$$f_{cur.jet} = 2Nm_e v. \tag{28}$$

Putting here $m_e = 9.11\times10^{-31}$ kg, $v = c = 3\times10^8$ m/sec, we obtain $f_{cur.jet} = 3416$ μN/A, noting that this force is proportional not to the square of the current but to the current. If the energy velocity will be less than c, respectively $f_{cur.jet}$ will be less.

In Ref. 2 I proposed an experiment to establish whether there is a current jet effect acting on the Ampere bridge. However the strong dependence of the force acting in the Ampere bridge on the square of the current established by all experimentalists[3-6] says that, very probably, there are not current jet forces acting in the Ampere bridge.

The most well-done experiment which has an impeccable geometry is Peoglos experiment[5] with his L-frame (where the action on a straight wire is observed). Peoglos experiment[5] with his D-frame (where the action on a Π-form Ampere bridge is observed) can be not mathematically predicted, if proceeding from Grassmann's formula, as there are singularities at the angles and Peoglos' theoretical prediction is based on formulas (21) and (22). Peoglos obtains figures almost 5 times less than the figures given by the indirect measurement with my Rotating Ampere Bridge with Sliding Contacts[6,7]. Further investigations have to establish which are the reasons for these experimental discrepancies.

It is of interest to note that in Refs. 6 and 7 I obtained the following relation between the induced back tension, U_{back}, the current in the rotating Ampere bridge, I, the pushing force, f, and the rotational velocity v (i.e., the velocity of the rotor's points at   which the pushing force is applied)

$$IU_{back} = fv, \tag{29}$$

which formula is obvious, as it gives the equality between the power lost by the source of electric tension driving the rotating Ampere bridge and the mechanical power acquired by the bridge. The measurement of f by measuring U_{back}, I and v in the method developed by me[6,7] is very pure, elegant and exact.

A similar method was used by P. T. Pappas[6] (p. 169) who measured the back tension induced in the Ampere-Faraday disk (name introduced by me). Proceeding from Pappas figures, I found[6] (p. 178) for the pushing force acting on the Ampere bridge again a number near to 5 μN/A^2.

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FIGURE CAPTIONS
Fig. 1. U-form Ampere bridge.
Fig. 2. Half-circular shoulder of the U-form Ampere bridge.
Fig. 3. The Π-U-circuit.
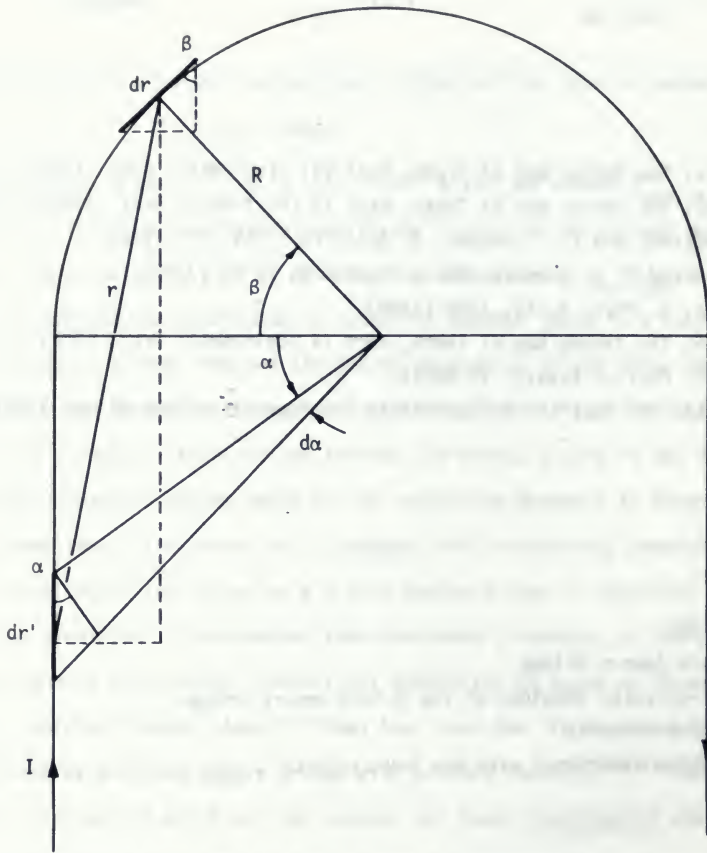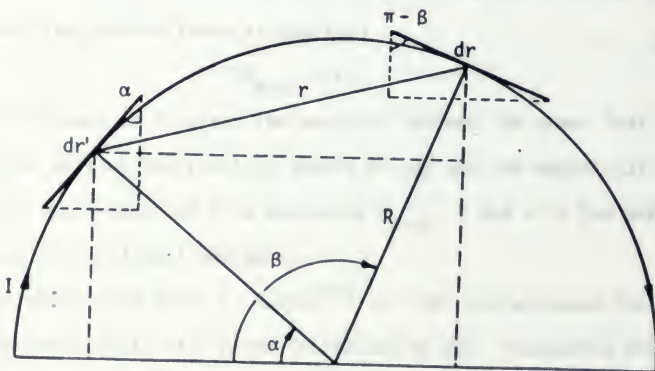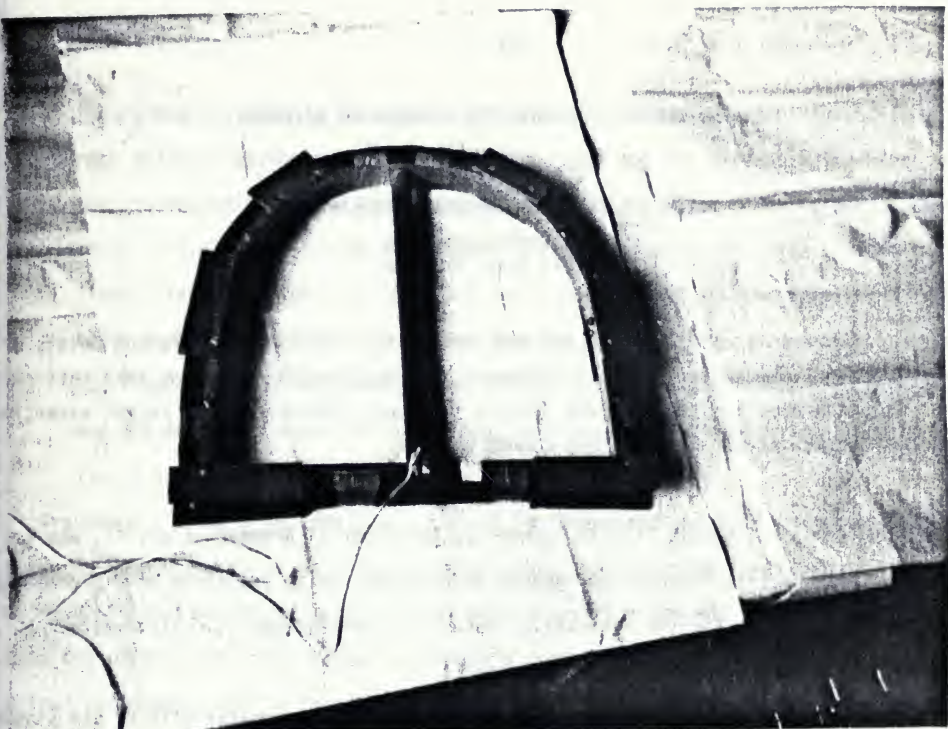Fig. 4. Pendulum experiment with the Π-U-circuit.

Fig. 1

Fig. 2

Fig. 3

Fig. 4

ON THE ELECTRIC INTENSITIES INDUCED IN RAILGUNS

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Abstract. I calculate the electric intensities induced at different points of railguns, proceeding from the fundamental Lorentz equation. I show that this calculation predicts the observed induced tensions, contrary to the recent assertion of Graneau et al.[1] that this cannot be done.

With this paper I should like to comment on the paper of Graneau et al.[1], who assert that the electric intensities induced in railguns can be explained only proceeding from Ampere's formula for the interaction between current elements, while Grassmann's formula, i.e., the Lorentz force formula, fails.

In my firm opinion the formula which explains all electromagnetic effects (of single charges and of current elements) is Grassmann's, i.e., Lorentz', formula. As it is well known, experimenta crucis that can choose which of these two formulas is the true one can be done only by observing the interaction between non-closed circuits (for closed circuits both formulas predict equal effects). At the present time such effects have been observed only by Graham and Lahoz[2] and by me[3,4]. My experiments have led to violation of the angular momentum conservation law (it is well known that Grassmann's formula violates Newton's third law), what cannot occur if Ampere's formula is the true one, as it preserves Newton's third law. All proponents of Ampere's formula (the two Graneaus, Pappas, Wesley, Assis, the recently converted Vigier[5] etc.) are very well acquainted with my Bul-Cub Machine without Stator[3] and Rotating Ampere Bridge with Displacement Current[4] which have violated the angular momentum conservation law, but nobody has commented on the effects observed by me. Only the critical analysis of these machines can resolve the Ampere-Grassmann controversy, which humanity failed to resolve in the last 150 years, although the way for solving this controversy has been indicated by Grassmann himself, who wrote in his historical paper[6] (p. 14):

Oberhaupt ist klar, dass eine Entscheidung zwischen beiden Theorien (Ampere's

theory and Grassmann's theory), da die Wirkung , welche geschlossene Ströme üben, nach beiden dieselbe ist, nur möglich ist, wenn man die Wirkung betrachtet, welche ein begränzter Strom übt... Der begränzte Strom würde daher so hervorzurufen sein, dass man zuerst etwa zwei Kugeln mit entgegengesetzten Elektrizität möglichst stark lüde, und sie dann nach der Ladung (nicht während derselben) in leitende Verbindung brächte. Dann hätte man die Wirkung dieses begränzten Stromes auf irgend einen elektrischen Strom oder besser auf einen Magneten zu beobachten, und die Anordnung dabei so zu treffen, dass die Wirkung nach beiden Theorien möglichst verschieden erfolgten.

Grassmann's program was realized at first by Graham and Lahoz[2] and by me[3,4], however, as Graham and Lahoz have not suspended their whole system freely, they could not observe a violation of the angular momentum conservation law and thus their experiment does not offer a clear refutation of Ampere's formula, as Pappas asserts[7] that a cylindrical coil can be set in rotation about its axis, if acting on it with non-closed circuits.

The proponents of Ampere's formula consider the explosion of wires, along which strong currents flow, as a confirmation of this formula, treating the explosive forces as the Ampere forces of repulsion between collinear currents. As a matter of fact, the explanation of these explosive forces is quite different:

Sansbury has observed[8] that when stationary current flows along a wire, a positive electric charge is repulsed from the wire, independently of the direction of the current. Thus a wire along which current flows becomes charged positively. Why? Because the positive electrode of the battery "sucks" the electrons from the wire in its immediate neighbourhood, this wire's domain deprived of electrons "sucks" the electrons from the wire in its neighbourhood, and the process goes on with a velocity near to c (or, as Milnes[9] and Pappas[10] have recently announced of having measured, with a velocity higher than c). When the current is higher, the depletion of electrons in the wire is higher, and the repulsive forces between the positively charged ions of the metal lattice provoke explosions.

Now I shall calculate the electric intensities induced at the different points of

a railgun, proceeding form the Lorentz equation which gives the <u>global</u> electric intensity

$$E_{glob} = - grad\phi - \partial A/\partial t + v \times rotA, \tag{1}$$

i.e., the force acting on unit positive electric charge crossing with a velocity v a reference point where the electric and magnetic potentials generated by the surrounding system are

$$\phi = \sum_{i=1}^{n} q_i/4\pi\varepsilon_0 r_i, \quad A = \sum_{i=1}^{n} \mu_0 q_i v_i/4\pi r_i = \sum_{i=1}^{n} \mu_0 I_i dr_i/4\pi r_i, \tag{2}$$

where q_i is the ith electric charge of the system moving with a velocity v_i whose distance from the reference point is r_i and $I_i dr_i$ is the respective current element.

Let us consider the railgun analysed by Graneau et al.[1] (fig. 1), in which the armature branch AB, under the action of the propulsive magnetic force F, moves upwards by making sliding contacts with the rails DE and CF, and noting that, according to Grassmann's formula, the same force applied to the branch DC acts in the opposite direction.

Let us choose a reference frame with origin at point A, with x-axis along AB, y-axis along AE and z-axis pointing to the reader. We shall assume the wires DA = CB = L long enough, so that the action of the current in the wire DC can be neglected, and let us denote AB = a.

Proceeding from formula (2), we obtain for the magnetic potential generated by the current wire DA at a point of the wire AB distant x from the frame's origin, assuming that the current flows in the direction DA,

$$A = (\mu_0 I/4\pi)\int_0^L (x^2 + y^2)^{-1/2}dy \,\hat{y} = (\mu_0 I/4\pi)Arsinh(L/x)\hat{y}. \tag{3}$$

The electric intensity induced at that point when AB moves with the velocity $v = v\hat{y}$ will be   the motional electric intensity

$$E_{mot} = v \times rotA = - (\mu_0 vI/4\pi)\hat{y} \times \{\hat{y} \times grad(Arsinh\frac{L}{x})\} = - (\mu_0 vI/4\pi x)L(x^2 + L^2)^{-1/2}\hat{x}. \tag{4}$$

Graneau at al.[1] assert that, if proceeding from the Lorentz formula (1), this must be the only electric intensity induced in the circuit, as they write (p. 397):

> If we believe in the Lorentz formula (1), then no electric intensity should be induced in the rails.

This assertion of Graneau et al. is not true, as they (as well as conventional physics) do not take into account the transformer induction which may appear because of the availability of the term $-\partial A/\partial t$ in the Lorentz formula (1).

Indeed, at the different points of the wire DE (as well as CF) two kinds of transformer electric intensities will be induced (the theory and the terminology of the different kinds of transformer induction are given by me[11,12]):

1. The motional-transformer electric intensity

$$E_{mot-tr} = (\mathbf{v}.grad)A, \tag{5}$$

where $\mathbf{v} = v\hat{\mathbf{y}}$ is the velocity of the wire AB and (compare with formula (3))

$$\mathbf{A} = (\mu_0 I/4\pi)\int_0^a (x^2 + y^2)^{-1/2} dx\hat{\mathbf{x}} = (\mu_0 I/4\pi)Arsinh(a/y)\hat{\mathbf{x}} \tag{6}$$

is the magnetic potential generated by the current in the wire AB at a point of the wire DE with ordinate y, so that

$$\mathbf{E}_{mot-tr} = (\mu_0 vI/4\pi)\{\partial(Arsinh\frac{a}{y})/\partial y\}\hat{\mathbf{x}} = -(\mu_0 vI/4\pi y)a(y^2 + a^2)^{-1/2}\hat{\mathbf{x}} \tag{7}$$

will be in parallel to the x-axis and thus will not generate induced tension (I dislike the term electromotive _force_) in the wire DA or AE.

2. The rest-transformer electric intensity

$$E_{rest-tr} = -\partial A(t)/\partial t, \tag{8}$$

where

$$\mathbf{A}(t) = (\mu_0 I/4\pi)\int_0^{vt}(y-y')^{-1}dy'\hat{\mathbf{y}} = -(\mu_0 I/4\pi)\ln(1 - vt/y)\hat{\mathbf{y}} \tag{9}$$

is the magnetic potential generated by the _additional_ vertical current appearing in the rail DA at time t as a result of the motion of the wire AB with a velocity $\mathbf{v}$, and y is the ordinate of the reference point in the wire AE (for a reference point in the wire DA one must write in formula (9) $y'-y$ instead of $y-y'$), so that

$$\mathbf{E}_{rest-tr} = -(\mu_0 vI/4\pi)(y - vt)^{-1}\hat{\mathbf{y}}. \tag{10}$$

The calculation of the rest-transformer electric intensities induced in the wires CB and BF is similar and thus we see that the polarities of the electric tensions induced in the wires AB, DA, AE, CB, BF are as indicated in fig. 1 which were obtained also by Graneau et al.[1]. Let us now calculate these tensions.

For the   motional electric tension induced in the wire AB we shall have by

integrating formula (4) and taking into account also the tension induced as a result of the magnetic action of the current wire CB (as the tensions act on <u>linear</u> wires, we can consider them as <u>vectors</u>)

$$\mathbf{U}_{mot} = - (\mu_0 vI/2\pi)\int_0^a (L/x)(x^2 + L^2)^{-1/2} dx\,\hat{\mathbf{x}} \cong - (\mu_0 vI/2\pi)\int_0^a (dx/x)\hat{\mathbf{x}} = - (\mu_0 vI/2\pi)\ln(a/a_0)\hat{\mathbf{x}},$$

(11)

where a_0 is a <u>small</u> length, as for $a_0 = 0$ the integral on the right becomes illimited. The mathematical determination of the small quantity a_0 is impossible at this level of mathematical calculations (in Ref. 13 I give the exact mathematical calculation of the pushing forces - and consequently of the induced tensions - in Ampere bridges of different forms). As I have shown in Ref. 4 (p. 140), for AB/AD < 0.33, the approximation taken in formula (11), i.e., the cancelation of the factor $L(x^2 + L^2)^{-1/2}$, leads to an error not bigger than 5%.

For the rest-transformer electric tension induced in the wire AE we shall have by integrating formula (10), for the moment $t = 0$,

$$\mathbf{U}_{rest-tr} = - (\mu_0 vI/4\pi)\int_0^y (dy'/y')\hat{\mathbf{y}} = - (\mu_0 vI/4\pi)\ln(y/a_0)\hat{\mathbf{y}},$$

(12)

where a_0 is again a <u>small</u> length, as for $a_0 = 0$ the integral becomes illimited.

If we assume $y = a$, we shall have, as shown in fig. 1,

$$(1/2)U_{AB} = U_{AE} = U_{DA} = U_{BF} = U_{CB}$$

(13)

with the indicated in the figure directions.

Graneau et al.[1] have pointed out at some experimental observations which seem to have confirmed the relations (13). I have, however, to note that the railgun is not the appropriate machine for measuring induced tensions, as the phenomena there are not stationary. An appropriate machine is the Rotating Ampere Bridge with Sliding Contatcs constructed recently by me (Ref. 4, p. 136 and Ref. 14); the numerical results of the induced tensions have confirmed the above theory.

At the end I should like to note that if proceeding from Ampere's formula, one is unable to introduce the notion magnetic potential **A**, and consequently also the notion magnetic intensity B = rotA. Thus, if Grassmann's formula, i.e., the Biot-Savart formula, for the magnetic intensity generated by a current element I**dr** at a distance r from it

$$B = \text{rot}A = \text{rot}(\mu_0 I d\mathbf{r}/4\pi r) = (\mu_0 I/4\pi)(d\mathbf{r} \times \mathbf{r}/r^3) \qquad (14)$$

was not introduced into physics, humanity would be <u>unable</u> to calculate a good deal of its electromagnetic machines.

In Ref. 7, p. 183, I announced a prize of $ 1000, addressed to Prof. Pappas, if he will be able to write a magnetic potential generated by a current element by proceeding from Ampere's formula. Now I announce this prize to all supporters of Ampere's formula.

Let me further note that the notion motional-transformer induction and the relevant formula (formula (5)) have been <u>discovered</u> by me and announced in two <u>paid</u> advertisements[11,12], as <u>almost all</u> physical journals of the world rejected my papers (see the relevant documentation in my series   THE THORNY WAY OF TRUTH), realizing that the motional-transformer induction which is <u>not</u> opposite to the motional induction (see the last term in formula (1)) kills the principle of relativity. Recently Maddox[15], although with a tongue in the cheek, has recognized the failure of relativity. Rindler[16], without mentioning my priority, consented that the motional-transformer induction is to be calculated by the help of formula (5) and not by the help of the relativistic formula

$$E_{\text{relativistic}} = - \mathbf{v} \times \text{rot}A. \qquad (15)$$

I show in Ref. 4, p. 101 and in Ref. 17 that the <u>relative</u> Lorentz equation, which is to be used in a laboratory moving with a velocity V in absolute space (the space in which velocity of light is isotropic), has a form different from the form of the <u>absolute</u> Lorentz equation (1), and this relative Lorentz equation explains the "strange" non-reciprocity of the motional and motional-transformer inductions.

I have, however, to note that Maxwell[18] has turned the world's attention to the motional-transformer induction with the following words:

> Another part of the electromotive force depends on the time-variation of the magnetic field. This may be due either to the time-variation of the electric current in the primary circuit (i.e., rest-transformer induction - S.M.), or to the motion of the primary circuit (i.e., motional-transformer induction - S.M.).

Then Maxwell gives the Lorentz equation <u>exactly</u> in the form (1), so that the attribution of Lorentz' name to it is historically <u>unwarranted.</u>

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FIGURE CAPTION

Fig. 1. Distribution of the motional and rest-transformer tensions induced in the railgun (the tensions are designated not by "U" as in the paper but by "e").
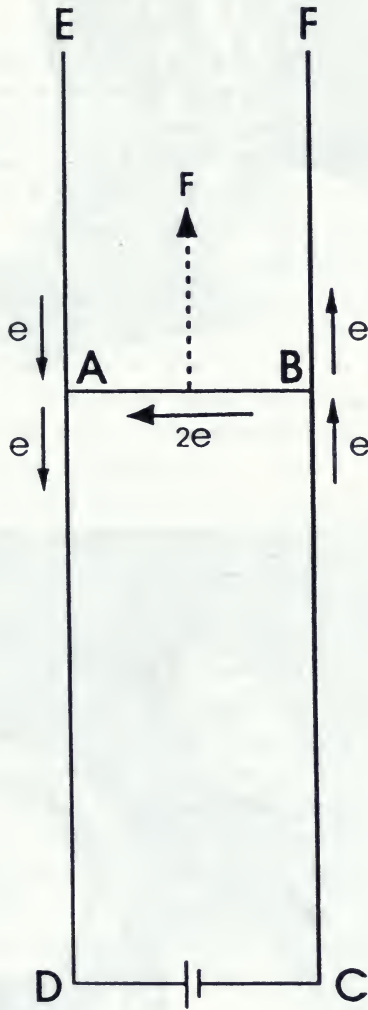
Fig. 1

A WIMSHURST MACHINE COUPLED WITH A CORONA MOTOR

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In TWT-VII, p. 235 I presented the Wimshurst machine   and in TWT-V, p. 8 (section 5) the electrostatic (corona) motor constructed by me.

In the present paper, I shall present a Wimshurst machine coupled (mechanically and electrically) with a corona motor which I recently constructed (figs. 1 and 2).

The two disks with the metal sectors have been taken from a standard Wimshurst machine purchased for 6000 shillings from the company Leybold-Heraeus (Nr. 54165). These two Wimshurst disks can be seen at the middle of the machine.

The two corona motors are at the left and right sides of the apparatus. Every one of them consists of six spherical electrodes (three such electrodes can be seen in the left corona motor in fig. 1). Three of the electrodes are connected to the positive output electrode of the Wimshurst machine and three to the negative, so that every positive motor electrode has two negative neighbours and vice versa. The motor electrodes which are solid to the laboratory are put between two solid one to another plastic disks covered with a rough plastic fabric (for enhancing the driving torque of the corona motor)

The left corona motor is fixed to the left axle to which also the left Wimshurst disk is fixed, while the right corona motor is fixed to the right axle to which also the right Wimshurst disk is fixed. Both these axles can be rotated by one's fingers by grasping the two metal cylinders at the ends of the two axles (in figs. 1 and 2 only the left one of these metal cylinders can be seen).

The idea is to set the machine in motion by applying one's fingers' torque, and then to let it rotate eternally if the current produced by the Wimshurst machine will drive the two corona motors with mechanical power higher than the electro-mechanical braking power of the Wimshurst machine (accepting the mechanical friction power of being substantially lower than the electro-mechanical braking power of the Wimshurst generator).

Unfortunately, this was not the case. The electro-mechanical braking power of the Wimshurst generator was at least 5 times bigger than the electro-mechanical driving power of the corona motor.

Thus, my conclusion is that the machine TESTATIKA has certain sophistications which make its driving motor power bigger than its braking generator power. My Wimshurst generator coupled with the two corona motors works in conformity with the energy conservation law, showing that the efficiency of the Wimshurst generator is very low (in TWT-V, p. 8 I showed that the efficiency of the corona motor is pretty high). The explanation for the low efficiency of the Wimshurst machine is the following: a big part of the electrical charges separated by the machine goes lost by discharges in the air without coming to the electrodes of the corona motors to be discharged between them by putting the motors' disks in rotation.
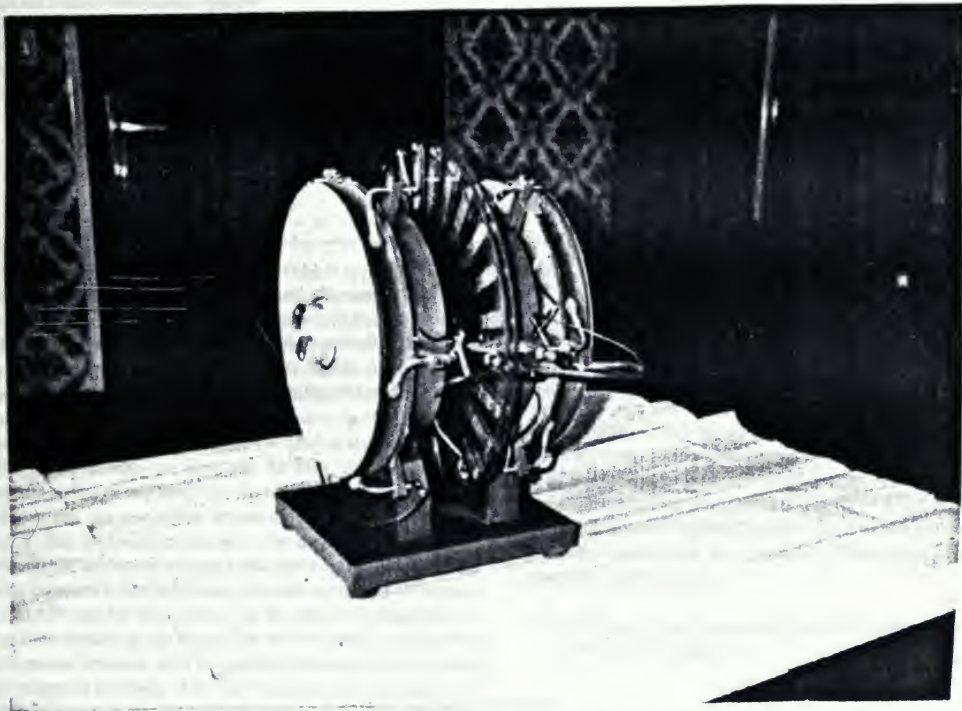
Fig.

Fig.

Absolute and Relative Newton-Lorentz Equations

Stefan Marinov

Abstract

The fundamental electromagnetic Lorentz equation is shown to have its well-know form (which I call the absolute Newton-Lorentz equation) only if the chosen reference fram rests in absolute space. If the chosen reference frame moves in absolute space, this equation has a different form, which I call the relative Newton-Lorentz equation. Rece experiments have confirmed the validity of the relative Newton-Lorentz equation.

In electromagnetism there are only two fundamental equations that cannot be deduced from other simpler postulates. Those are the laws of Coulomb and Neumann, which assert that if there are two electric charges q_1, q_2 moving with velocities v_1, v_2, then they have two kinds of energies, called electric (*space*) and magnetic (*space-time*) energies (the italicized words here and below are my terms),

$$U = q_1 q_2/r, \quad W = q_1 q_2 v_1 \cdot v_2/c^2 r, \tag{1}$$

where r is the distance between the charges, and the formulas are written in the cgs system of units.

Using the law of superposition (the energy of a system of more than two charges is the sum of the energies of all its pairs), and putting (1) in the law of conservation of energy $dE_0 + dU + dW = 0$, where E_0 is the sum of the *time* energies $e_0 = mc^2(1 - v^2/c^2)^{-1/2}$ of any of the particles of the system, m being the respective particle's mass and v its velocity, I showed,[1],[2] by rigorous mathematical arguments, that one can obtain the fundamental equation in electromagnetism (which I call the *Newton-Lorentz equation*) and from it all electromagnetic "laws." I obtain this equation in the form

$$(d/dt)(\mathbf{p}_0 + q\mathbf{A}/c) = -q\nabla(\Phi - \mathbf{v}\cdot\mathbf{A}/c), \tag{2}$$

where $\mathbf{p}_0 = m\mathbf{v}(1 - v^2/c^2)^{-1/2}$ is the momentum of a particle with electric charge q at a reference point where the electric and magnetic potentials of the surrounding system of n particles (summation from 1 to n)

are

$$\Phi = \sum \frac{q_i}{r_i}, \quad \mathbf{A} = \sum \frac{q_i \mathbf{v}_i}{cr_i}, \tag{3}$$

so that $q\Phi$ and $(q\mathbf{v}/c)\cdot\mathbf{A}$ are the electric and magnetic energies in which charge q takes part.

Since $d\mathbf{A}/dt = \partial\mathbf{A}/\partial t + (\mathbf{v}\cdot\nabla)\mathbf{A}$, we can reduce Eq. (2) to its usua form (known as the Lorentz equation):

$$d\mathbf{p}_0/dt = -q(\nabla\Phi + \partial\mathbf{A}/c\partial t) + (q\mathbf{v}/c) \times \mathrm{rot}\,\mathbf{A}. \tag{4}$$

The above formulas are written in a reference frame attached to absolut space (the space in which light velocity is isotropic), and I call (2) and (4 the *absolute* Newton-Lorentz equation.

Now I shall show which will be the form of the *relative* Newton-Lorent equation, i.e., when working in a frame moving with a velocity V in absolut space. As demonstrated with the "rotating axle" experiments reported in Ref 1 to 5, the Earth moves in absolute space with a velocity of about 350 km/ and during a year this velocity suffers changes of about ± 30 km/s because o the Earth's revolution around the Sun.

Thus let us suppose that the velocities of the test charge and of the charge of the surrounding system in the laboratory are, respectively, v' and v_i'. shall obtain the relative Newton-Lorentz equation within an accuracy of *firs order* in V/c, so that the Galilean formulas for velocity addition $v = v' + V$, $v_i = v_i' + V$ can be used. If working with a higher accuracy, th

formulas for velocity addition indicated in Refs. 1, 2, and 6 are to be used. Taking into account the Galilean formulas, we shall have

$$\Phi - \frac{\mathbf{v} \cdot \mathbf{A}}{c} = \sum \frac{q_i}{r_i} - \frac{\mathbf{v}' + \mathbf{V}}{c} \cdot \sum \frac{q_i(\mathbf{v}'_i + \mathbf{V})}{cr_i}$$

$$\cong \Phi' \left(1 - \frac{\mathbf{v}' \cdot \mathbf{V}}{c^2} \right) - \frac{\mathbf{v}'}{c} \cdot \mathbf{A}' - \frac{\mathbf{V}}{c} \cdot \mathbf{A}', \qquad (5)$$

where $\Phi' = \Phi$ is the relative electric potential which is equal to the absolute electric potential, because the electric potential is not velocity dependent, $\mathbf{A}' = \sum q_i \mathbf{v}'_i / cr_i$ is the relative magnetic potential, and the expression on the right side is written within an accuracy of first order in V/c.

One should take into account two substantially different invariances [1],[2]: the Lorentz invariance and the invariance proposed by the present author. One works with the Lorentz invariance when an observer considers the motion of a particle that first moves with a velocity $\mathbf{v}$ in absolute space and then with *another* velocity $\mathbf{v}'$, while one works with the other invariance when the motion of a particle moving always with the *same* velocity $\mathbf{v}$ is considered by an observer who first is at rest in absolute space and then moves with a velocity $\mathbf{V}$. Thus the Lorentz invariance is to be applied when the *observed particle* changes its character of motion with respect to distant matter, while the other invariance is to be applied when the *observer* changes his character of motion with respect to distant matter. For the Lorentz invariance there is a change in the momentum and energy of the observed particle and it involves four-dimensional invariants, while for the other invariance there is no change in the momentum and energy of the observed particle and it involves three-dimensional invariants. For the theory of relativity it is of no significance whether the observed particle or the observer changes its (his) character of motion; however, unfortunately, these two cases are *physically substantially different*. [1]-[10]

The "total" time derivatives of the absolute and relative magnetic potentials must be equal, i.e., $d\mathbf{A}/dt = d\mathbf{A}'/dt$, because $d\mathbf{A}/dt$ depends only on the changes of the *relative* velocities of the charges of the system with respect to the test charge and on the changes of the distances between the former and the latter which are also "relative." Thus putting the above equality and (5) into (2), we obtain

$$\frac{d}{dt} \frac{m(\mathbf{v} + \mathbf{V})}{[1 - (\mathbf{v} + \mathbf{V})^2/c^2]^{1/2}} = -q \left(\nabla \Phi + \frac{1}{c} \frac{\partial \mathbf{A}}{\partial t} \right)$$

$$+ \frac{q}{c} \mathbf{v} \times \mathrm{rot}\, \mathbf{A} + \frac{q\mathbf{v} \cdot \mathbf{V}}{c^2} \nabla \Phi + \frac{q}{c} \mathbf{V} \times \mathrm{rot}\, \mathbf{A} + \frac{q}{c} (\mathbf{V} \cdot \nabla) \mathbf{A}, \qquad (6)$$

where the space and time derivatives are taken with respect to the laboratory, as we work only within an accuracy of first order in V/c, [1],[2],[6] and, for brevity, we write all laboratory quantities in the last equation (and further in this paper) without primes.

Comparing formulas (6) and (4), we see that the "potential" (right) parts of these equations differ with the last three terms in Eq. (6). Since the electric (i.e., related to Φ) absolute effects are proportional to v/c, they are small, if $v \ll V$, with respect to the magnetic (i.e., related to $\mathbf{A}$) absolute effects, which are not only comparable with the relative magnetic effects, but, for $V \gg v$, are even much bigger.

In Refs. 5 and 7 to 10 a third type of electromagnetic induction was presented which was called the *motional-transformer induction*. I see three reasons why this fundamental kind of induction was not derived before:

(1) The acceptance of what I believe to be *wrong* "intensity" and "flux" interaction concepts of Faraday-Maxwell (as opposed to the "potential" and "point-to-point" interaction concepts of Weber-Riemann).

(2) The acceptance of what I believe to be *wrong* principle of relativity of Lorentz-Einstein [as opposed to the absolute (or ether) concepts of Newton].

(3) The fact that for *closed* wires the motional and motional-transformer inductions lead to the *same* induced *circular tension*, as shown in Ref. 5.

The motional-transformer induction can, on the other hand, be deduced by simply applying the basic rules of mathematics when contemplating the Newton-Lorentz Eq. (4).

First let me note that $d\mathbf{p}_0/dt$ is called *kinetic force* of the charge q. The kinetic force of a unit charge $\mathbf{E}_{glob} = (d\mathbf{p}_0/dt)/q$ is called now *global electric intensity*. Considering an electrically neutral system of charges where the electric action of the positive charges is neutralized by the electric action of the negative charges (such is a system of closed wires along which direct and alternating currents flow), we shall have $\Phi = 0$, and this assumption will be held further throughout this paper. Let us assume that the surrounding system represents only one current loop. There are three possible *fundamentally different* cases:

(1) The loop is at rest, the current is constant, the test charge is moving. Then Eq. (4) reduces to

$$c\mathbf{E}_{mot} = \mathbf{v} \times \mathrm{rot}\, \mathbf{A}, \qquad (7)$$

and I (as well as conventional physics) call this the *motional electric intensity (motional induction)*.

(2) The loop is at rest, the current is alternating, the test charge is at rest. Then Eq. (4) reduces to

$$c\mathbf{E}_{tr} = -\partial \mathbf{A}/\partial t, \qquad (8)$$

and I (as well as conventional physics) call this the *transformer electric intensity (transformer induction or, more precisely, resttransformer induction)*.

(3) The loop is moving, the current is constant, the test charge is at rest. Then Eq. (4) reduces to the following, if taking into account that in this case $\mathbf{A}$ is a *composite* function of time through the distances r_i of the n current elements of the loop to the reference point:

$$c\mathbf{E}_{mot\text{-}tr} = -\frac{\partial \mathbf{A}}{\partial t} = -\sum \frac{\partial \mathbf{A}_i [r_i(t)]}{\partial t}$$

$$= -\sum \left(\frac{\partial \mathbf{A}_i}{\partial x_i} \frac{\partial x_i}{\partial t} + \frac{\partial \mathbf{A}_i}{\partial y_i} \frac{\partial y_i}{\partial t} + \frac{\partial \mathbf{A}_i}{\partial z_i} \frac{\partial z_i}{\partial t} \right)$$

$$= \sum (\mathbf{v}_i \cdot \nabla) \mathbf{A}_i, \qquad (9)$$

where $\mathbf{v}_i = -\partial \mathbf{r}_i/\partial t$ is the velocity of the ith current element of the loop, so that $-\mathbf{v}_i$ is the velocity of the test charge in the *moving* inertial frame attached to the ith current element of the loop. If the *whole* current loop moves with the velocity $\mathbf{v}$, formula (9) reduces to

$$c\mathbf{E}_{mot\text{-}tr} = (\mathbf{v} \cdot \nabla) \mathbf{A}. \qquad (10)$$

I call this the *motional-transformer electric intensity (motional-transformer induction)*. Conventional physics, unfortunately, seems

to deny the existence of formulas (9) and (10) and, proceeding from the principle of relativity, asserts that in the third case the induced electric intensity must be calculated according to the formula

$$cE = -v \times \text{rot } A. \qquad (11)$$

i.e., it reduces the third case to the first one, considering them *identical*.

I should like to emphasize that for the third case formula (10) and *not* formula (11) is adequate to physical reality. The experiment that has verified this is the following (Fig. 1): Along the rectangular loop with d much larger than b, a constant current I flows in the indicated direction. If moving the vertical wire with a velocity v to the right, between the extremities of the wire an induced motional electric tension will appear, which according to formula (7), will have the indicated polarity and the magnitude $U_{mot} \cong (4vI/c^2)$ $\ln(2b/b_0)$, where $b_0/2$ are the distances between the end points of the vertical wire and the horizontal wires of the loop. If, however, the vertical wire is kept at rest and the loop is moved with the same velocity to the left, an induced motional-transformer electric tension will appear, which according to formula (10), will have the same polarity and the magnitude $U_{mot-tr} = 8vIb^2/c^2d^2 \cong 0$, the approximation being valid for $b \ll d$.

Experiments confirming these formulas were carried out first by Kennard[11] for rotational motion and by F. Müller[5],[8] for inertial motion. With the help of the quasi-Kennard experiment in January 1989, I established[12] that the right ascension of the apex of the Earth's absolute velocity is $\alpha = 11^h8 \pm 1^h$ (the "coupled shutters" experiment in February 1984 gave $\alpha = 12^h5 \pm 1^h$).

Now I shall show that the effects observed by Müller are to be predicted not only when working with the absolute Newton-Lorentz Eq. (4), but also when working with the relative Newton-Lorentz Eq. (6). Indeed, let us suppose that the system originating the magnetic field (the loop in Fig. 1) is at rest in the laboratory and the latter moves with the constant velocity V in absolute space. The test charge (the vertical wire in Fig. 1) is also first at rest in the laboratory and then is moved with the velocity v with respect to the laboratory. The induced electric intensities in these two cases, according to formula (6), will be

$$cE = V \times \text{rot } A + (V \cdot \nabla)A,$$
$$cE' = v \times \text{rot } A + V \times \text{rot } A + (V \cdot \nabla)A, \qquad (12)$$
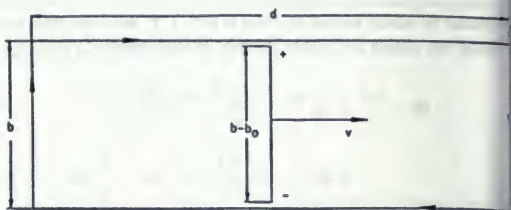
Figure 1. Experiment revealing the difference between the motional and motional-transformer inductions.

and for the difference $E' - E$ (which was the effect measured), one obtains the result (7).

Let us then suppose that the test charge is always at rest in the laboratory and the loop originating the magnetic field is also first at rest in the laboratory and then is moved with the velocity v with respect to the laboratory. The induced electric intensity for the first case will be as above. For the second case we have to write the relative Newton-Lorentz equation in a frame moving with a velocity $V + v$ in absolute space as only in this frame the originated laboratory magnetic potential will be as in the first case, and because in this frame the test charge will have a velocity $-v$, we obtain

$$cE'' = -v \times \text{rot } A + (V + v) \times \text{rot } A + [(V + v) \cdot \nabla]A. \qquad (13)$$

Thus for the difference $E'' - E$ (which was the effect measured), one obtains the result (10).

It is interesting to note that Rindler has recently asserted[13] that for the case of a moving magnet, the force acting on a unit charge at rest must not be (11) but (10), without noting that the first introduction of formula (10) and the demonstration of the incorrectness of formula (11) was done in Refs. 9 and 10. Rindler's derivation of formula (9) is believed to be obtained by a "trick," whereas it is known that by differentiation of a composite function, one inevitably gets formula (9). Having therefore recognized that the forces acting on a unit charge, for the cases a) magnet at rest, charge moving, and b) charge at rest, magnet moving, are different, it is believed that relativity begins to be in serious trouble.

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Résumé

On montre que l'équation fondamentale de l'électromagnétisme de Lorentz ne prend sa forme bien connue (que j'appelle l'équation absolue de Newton-Lorentz) que dans un référenciel immobile dans l'espace absolu. Si l'on se situe dans un référenciel en mouvement par rapport à l'espace absolu, cette équation prend une forme différente que j'appelle l'équation relative de Newton-Lorentz. De recents expériments ont confirmé la validité de l'équation relative de Newton-Lorentz.

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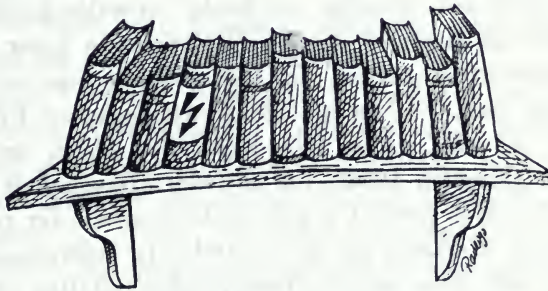
I. *Neue Theorie der Elektrodynamik;*
   von Hermann Grafsmann.

Es ist bekannt, dafs sich die bewegenden Wirkungen, welche elektrische Ströme oder Magnete auf einander oder die einen auf die andern üben, so weit sich bisher unsere Beobachtungen erstrecken, aus Einer Voraussetzung ableiten lassen. Das Gebiet, auf welchem sich jene Beobachtungen bewegen, läfst aber noch, wie ich hernach zeigen werde, für die Annahme der gegenseitigen Einwirkung zweier Stromtheile einen freien Spielraum übrig. Indem ich nun die Ampère'sche Annahme, nach welcher, wie es seyn mufs, die gegenseitige Einwirkung zweier unendlich kleinen Stromtheile zu Grunde gelegt wird, einer genaueren Prüfung unterwarf, so ergab sich mir dieselbe als höchst unwahrscheinlich; und indem ich zunächst das Willkührliche in jener Annahme fortzuschaffen suchte, so bot sich mir eine andere Annahme dar, welche die elektrodynamischen Erscheinungen, so weit sie in den Kreis der bisher angestellten Beobachtungen fallen, mit gleicher Genauigkeit darstellt, welche aber sowohl durch die Einfachheit der zu Grunde gelegten Formel, als auch durch die vollkommene Analogie mit allen andern bewegenden Kräften den höchsten Grad der Wahrscheinlichkeit besitzt. Ich habe schon angedeutet, dafs diese neue Annahme, auf alle bisher beobachteten Erscheinungen angewandt, dieselben Resultate liefert, wie die Ampère'sche; hingegen giebt es ein Gebiet der Erscheinungen, auf welchem nach beiden Annahmen oft gerade die entgegengesetzten Erfolge eintreten müfsten, und welches daher von Seiten der Er-

fahrung her die einzige Entscheidung über die Richtig-
keit der einen oder der andern Annahme liefern würde.
Es ist diefs, wie ich am Schlusse dieser Abhandlung zei-
gen werde, das Gebiet der Strömungen, welche durch
freie, an den Enden einer Leitung aufgehäufte (entge-
gengesetzte) Elektricitäten hervorgebracht werden, also
das Gebiet der durch Maschinenelektricität hervorgeru-
fenen Strömungen. Die Versuche, welche man bisher
auf diesem Gebiete angestellt hat, um elektrodynamische
Wirkungen, wie z. B. die Ablenkung einer Magnetnadel,
nachzuweisen, sind sehr weit davon entfernt, die Diffe-
renz beider Hypothesen irgend wie hervortreten zu las-
sen. Auch stellen sich solchen Versuchen, welche diefs
leisten können, bisher noch bedeutende Schwierigkeiten
entgegen. Dennoch scheint es mir wichtig, eine Hypo-
these als wahrscheinlich nachzuweisen, welche die Erfolge
vorhersagen würde, die bei feineren Instrumenten und
genaueren Beobachtungen eintreten müfsten. Eine sol-
che Annahme würde ein leitendes Princip werden, wo-
nach von geübter Hand vielleicht bald entscheidende Ver-
suche angestellt werden könnten. Es sey mir daher er-
laubt, hier diese neue Annahme abzuleiten, und geübte-
ren Physikern zur Prüfung vorzulegen.

1) Alle Versuche, welche bisher in Bezug auf elek-
trodynamische Erscheinungen angestellt sind, sind ent-
weder mit geschlossenen Strömen angestellt, oder doch
mit solchen Strömen, die wie geschlossene angesehen wer-
den können [1]. Diese Versuche bestehen darin, dafs
man entweder die gegenseitigen Einwirkungen zweier ge-
schlossener Ströme beobachtet, oder dafs man einen Theil

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1) Dahin gehören die Ablenkungen der Magnetnadel durch Entladun-
gen einer Batterie, wobei einestheils die zahlreichen Umwindungen
des Multiplicators, anderentheils die Nähe der nur durch die Dicke
des Glases getrennten Elektricitäten, welche ausgeglichen werden, die
Ströme, ihren beobachtungsfähigen Wirkungen nach, den geschlossenen
Strömungen gleich machen.

eines geschlossenen Stromes beweglich macht, und theils die Einwirkung beobachtet, welche er durch den ganzen Strom, dem er angehört, erleidet, theils die Aenderung dieser Einwirkung, welche erfolgt, wenn noch ein anderer geschlossener Strom hinzutritt. Da nun dadurch, dafs man einen Theil eines Stromes beweglich macht, die Wirkung, welche der ganze Strom hervorruft, nicht geändert wird, so erstrecken sich die bisher angestellten Versuche nur auf die Wirkungen, welche geschlossene Ströme üben, sey es nun auf andere geschlossene Ströme oder auf Stromtheile. Hingegen hat man keinen Versuch angestellt, um die Wirkung eines Stromtheils zu prüfen, weder die, welche er auf einen geschlossenen Strom, noch die, welche er auf einen andern Stromtheil übt.

2) Daher mufste Ampère, um zu seiner Formel zu gelangen, mit den Ergebnissen der Beobachtung eine willkührliche Annahme verbinden. Die Annahme, welche er zu diesem Ende macht, ist für den ersten Anblick eine höchst einfache und naturgemäfse, nämlich dafs zwei unendlich kleine Stromtheile längs der ihre Mitten verbindenden geraden Linie auf einander wirken, also entweder anziehend oder abstofsend im eigentlichen Sinne. Vermöge dieser Annahme gelangt nun Ampère von den Ergebnissen der Beobachtung aus mit Nothwendigkeit zu seiner Grundformel; nach welcher die Kraft, mit der ein unendlich kleiner Stromtheil a auf einen anderen b anziehend wirkt, proportional ist dem Ausdrucke:

$$\frac{ab}{r^2}(2\cos\varepsilon - 3\cos\alpha\cos\beta) \ldots \ldots \ldots 1$$

wo a und b die Stromelemente, d. h. die mit den Strömungsintensitäten multiplicirten unendlich kleinen Linientheile sind, in welchen sich die Ströme bewegen, r die Entfernung der Mittelpunkte dieser Linientheile von einander, ε den Winkel zwischen beiden Stromtheilen, α und β die Winkel bedeuten, welche diese Stromtheile

a und *b* beziehlich mit dem Strahle bilden, welcher von
dem Mittelpunkte eines dieser Stromtheile durch den des
andern gezogen werden kann.

3) Schon die verwickelte Gestalt dieser Formel mufs
einen Verdacht gegen sie erregen. Dieser Verdacht mufs
noch gesteigert werden, wenn man sie anzuwenden ver-
sucht. Betrachtet man z. B. den einfachsten Fall, dafs
beide Stromtheile parallel, also ε = 0, α = β seyen, so
geht der Ampère'sche Ausdruck über in:

$$1* \quad \ldots \ldots \ldots \ldots \frac{ab}{r^2}(2 - 3\cos^2 \alpha),$$

woraus hervorgeht, dafs wenn $\cos^2 \alpha$ gleich $\frac{2}{3}$, oder,
was auf dasselbe hinauskommt, wenn $\cos 2\alpha$ gleich $\frac{1}{3}$
ist, d. h. wenn der Mittelpunkt des angezogenen Elemen-
tes auf einer Kegeloberfläche liegt, deren Spitze in dem
anziehenden Elemente, und deren Winkel an der Spitze
zum Cosinus $\frac{1}{3}$ hat, keine Einwirkung erfolgt, innerhalb
derselben Abstofsung, aufserhalb Anziehung stattfindet.
Diefs Ergebnifs hat in der That zu wenig Wahrschein-
lichkeit, als dafs man nicht gegen die Annahme, aus wel-
cher es hervorgeht, einen Verdacht schöpfen sollte, so
sehr dieselbe auch dem Scheine nach durch die Analo-
gie aller übrigen Kräfte vertreten seyn mag. Dazu kommt,
dafs die Anwendung jener Analogie auf unser Gebiet
als eine wenig begründete erscheint. Denn bei allen
anderen Kräften sind es ursprünglich punktartige Ele-
mente, d. h. Elemente ohne bestimmte Richtungen, wel-
che auf einander wirken, und bei diesen läfst sich die
Nothwendigkeit der gegenseitigen Wirkung längs ihrer
Verbindungslinie sogar *a priori* ableiten; was berechtigt
uns aber, diese Analogie auf ein ganz fremdartiges Ge-
biet, auf welchem die Elemente mit bestimmten Richtun-
gen begabt sind, zu übertragen? Auch spricht die For-
mel selbst, welche keineswegs etwa der Formel für die
Anziehung durch Gravitation ähnlich ist, es deutlich genug
aus, dafs die Analogie in dieser Weise nicht stattfindet.

4) Ich gehe daher, ohne zunächst eine willkührliche Voraussetzung zu machen, davon aus, das Willkührliche der Ampère'schen Hypothese auszuscheiden, wobei ich, wie es geschehen mufs, annehme, dafs diese Hypothese, so weit sie durch Versuche bisher geprüft ist, d. h. so weit sie sich auf die Anziehungen bezieht, welche geschlossene Ströme auf andere Ströme oder Stromtheile üben, vollkommen bewährt sey. Es ergiebt sich zuerst leicht, dafs man alle Erscheinungen, welche innerhalb des so eben bezeichneten Gebietes liegen, ableiten kann, wenn man die Einwirkung kennt, welche ein Winkelstrom, d. h. ein unendlicher Strom, welcher einen Winkel durchströmt, auf ein Stromelement übt, dessen Mittelpunkt in der Ebene des Winkels liegt. Denn erstens kann ich jeden geschlossenen oder nicht geschlossenen Strom ansehen als zusammengesetzt aus solchen Stromelementen, und zweitens kann ich jeden geschlossenen Strom als ein von dem Strome durchflossenes Polygon, diese Polygon aber, als zusammengesetzt aus Winkelströmen, welche die Aufsenwinkel desselben bilden, ansehen, wobei ich nur aus der Erfahrung voraussetze, dafs gleich starke einander entgegengesetzte Ströme, welche durch denselben Leiter fliefsen, sich einander aufheben. So z. B. kann ich den Strom *abc* (Fig. 1 Taf. I) ansehen als zusammengesetzt aus den drei Winkelströmen *fad, dbe, ecf.* Endlich kann ich, indem ich von der Mitte des angezogenen Elementes einen Strahl durch den Scheitel des Winkelstromes lege, diesen in zwei Winkelströme zerlegen, deren jeder mit der Mitte des angezogenen Elementes in derselben Ebene liegt. Es kommt also, um aus der Ampère'schen Formel das Willkührliche fortzuschaffen, nur darauf an, aus ihr die Wirkung eines Winkelstromes auf ein derselben Ebene anliegendes Element abzuleiten.

5) Aus der Ampère'schen Formel folgt sogleich, dafs die Einwirkung, welche ein Element durch ein an-

deres erfährt, wenn beide Elemente nicht in derselben
Ebene liegen, gleich ist der Einwirkung, welche die senk-
rechte Projection des ersteren auf die, durch seine Mitte
und das letztere gelegte Ebene durch dasselbe Element
erfährt. Diese Beziehung wird also auch für unseren Fall
fortbestehen; und wir haben somit nur noch die Wir-
kung eines Winkelstromes auf ein Element derselben
Ebene, also zunächst die eines durchströmten Strahles
auf ein solches Element zu suchen. Diese Wirkung kön-
nen wir in eine längs dem Elemente und in eine senk-
recht dagegen erfolgende zerlegen.

6) Für diese Längsbewegung ergiebt sich, dafs sie
von der Richtung des Strahles unabhängig ist [1]), also

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1) Denn man hat aus Ampères Formel, wenn ds ein Element des
Strahles (in der Richtung des Strahles genommen) ist, und i die
Intensität der Strömung ist, welche von dem Anfangspunkt des Strah-
les aus diesen durchläuft, für die Anziehung, welche diefs Element
auf das Stromelement b nach dessen Längsrichtung übt, den Aus-
druck:

$$-\frac{i\,ds\,b}{r^2}\cos\beta\,(2\cos\varepsilon-3\cos\alpha\cos\beta).$$

Wenn ferner l das Loth von der Mitte des angezogenen auf die Li-
nie des anziehenden Elementes ist (siehe Fig. 2 Taf. I) ds gleich
$-d(l\cot\alpha)=\dfrac{l\,d\alpha}{sin^2\alpha}=\dfrac{r^2\,d\alpha}{l}$, während $\varepsilon=\alpha-\beta$, $d\beta=d\alpha$ ist.
Dadurch wird der obige Ausdruck:

$$=-\frac{ib}{l}\,(\cos^2\beta\cos\alpha\,d\alpha-2\sin\alpha\sin\beta\cos\beta\,d\beta),$$

was integrirt giebt:

$$-\frac{ib}{l}\sin\alpha\cos^2\beta.$$

Dehnt man die Integration über den ganzen Strahl aus, und setzt
schliefslich α, β, r insbesondere als die dem Anfangspunkte des Strah-
les zugehörigen Werthe, so erhält man, statt l wieder sein Werth
$r\sin\alpha$ gesetzt, den Ausdruck:

$$\frac{ib}{r}\cos^2\beta$$

für die Längswirkung des Strahles, welche somit von α, also von
der Richtung des Strahles, unabhängig ist.

für einen Winkelstrom eben so grofs ist, als ob beide Strahlen zusammenfielen, d. h. gleich Null ist. Daraus folgt, dafs die Wirkung, welche ein Winkelstrom auf ein seiner Ebene anliegendes Element übt, senkrecht gegen das letztere in dieser Ebene erfolgt, worin, beiläufig bemerkt, liegt, dafs die Wirkung eines beliebigen geschlossenen Stromes auf ein Stromelement stets senkrecht gegen das letztere erfolgt.

7) Die gegen das angezogene Element senkrechte Bewegung, welche ihm nach Ampère's Formel durch einen mit jenem Elemente in derselben Ebene liegenden durchströmten Strahl mitgetheilt wird, ergiebt sich als aus zwei Gliedern bestehend, deren eines von der Richtung des anziehenden Strahles unabhängig ist, und also bei der Annahme von Winkelströmen verschwindet, und deren anderes

$$\frac{ib_i}{r} cot \tfrac{1}{2}\alpha \dots {}^1) \dots \dots 2$$

ist, wo r die Entfernung des Elementes vom Anfangspunkte des Strahles und α der Winkel ist, welchen der

1) Nämlich mit Beibehaltung der obigen Bezeichnung ist die Wirkung eines Elementes ids des den Strahl durchlaufenden Stromes auf das Stromelement b_i nach der gegen das letztere senkrechten Richtung gleich:

$$\frac{idsb_i}{r^2} sin\beta\,(2cos\varepsilon - 3cos\alpha\,cos\beta),$$

was wieder, da $\frac{ds}{r^2}=\frac{d\alpha}{l}$, $d\alpha=d\beta$, $\varepsilon=\alpha-\beta$, also $2cos\varepsilon=cos\varepsilon$ $+cos(\alpha-\beta)$ ist, übergeht in $\frac{ib_i}{l}(cos\varepsilon\,sin\beta\,d\beta-2cos\alpha\,sin\beta\,cos\beta\,d\beta$ $+sin^2\beta\,sin\alpha\,d\alpha)$, und also integrirt giebt:

$$-\frac{ib_i}{l}[cos\varepsilon\,cos\beta + cos\alpha\,sin^2\beta];$$

und diefs liefert, wenn die Integration über den ganzen Strahl ausgedehnt wird, und die Bezeichnungen der veränderlichen Gröfsen (α, β) jetzt auf ihre für den Anfangspunkt des Strahles eintretenden Werthe beschränkt werden, den Ausdruck:

$$\frac{ib_i}{l}[1+cos\varepsilon\,cos\beta + cos\alpha\,sin^2\beta],$$

Strahl mit dem von seinem Anfangspunkte durch das Element gezogenen Strahle bildet, wo b_i die senkrechte Projection des Elementes auf die durch seine Mitte und den Strahl gelegte Ebene ist, i aber die Intensität des den Strahl durchlaufenden Stromes ausdrückt, und wo endlich das Stromelement sich nach seiner rechten oder linken Seite hin bewegt, je nachdem der Strom in dem Strahle demjenigen, welcher, von ihm aus das Element betrachtet, zur rechten oder zur linken Hand fortläuft. Hieraus folgt die Wirkung eines Winkelstromes, dessen Schenkel die Winkel α und α' mit dem durch das angezogene Element geführten Strahle bilden, gleich:

$$\frac{ib_i}{r}(cot \tfrac{1}{2}\alpha - cot \tfrac{1}{2}\alpha') \ldots \ldots \ldots 3$$

Hieraus folgt, beiläufig bemerkt, dafs die Gröfse der Bewegung, welche ein Stromelement von einem in gleicher Ebene mit ihm liegenden Strome erfährt, unabhängig ist von der Richtung dieses Elementes, aber stets senkrecht gegen dasselbe nach derselben Seite hin erfolgt.

8) Der gefundene Ausdruck (3) für die Wirkung eines Winkelstromes enthält nun, da diese Wirkung sich wenigstens annäherungsweise durch Versuche nachweisen läfst, nichts Hypothetisches mehr, zugleich enthält er die Resultate der Beobachtungen, da sie sich alle auf die Wirkung von Winkelströmen zurückführen lassen, vollständig in sich, und kann daher als Grundlage einer jeden Hypothese über die gegenseitige Einwirkung der

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indem im Unendlichen α 180° wird und β in 180° — ϵ übergeht. Setzt man endlich statt l und ϵ ihre Werthe $r\,sin\,\alpha$ und $(\alpha-\beta)$, zieht das dann sich entwickelnde Glied $cos\,\alpha\,cos^2\beta$ mit $cos\,\alpha\,sin^2\beta$ in Ein Glied $cos\,\alpha$ zusammen, und setzt statt $\dfrac{1+cos\,\alpha}{sin\,\alpha}$ seinen Werth $cot\,\tfrac{1}{2}\alpha$, so erhält man:

$$\frac{ib_i}{L}(cot \tfrac{1}{2}\alpha + sin\,\beta\,cos\,\beta),$$

wenn das zweite Glied von α, d. h. von der Richtung des anziehenden Strahles unabhängig ist.

Stromelemente dienen. Da nun dieser Ausdruck aus
zwei Gliedern besteht, von denen das eine durch die
Lage des Einen Strahles und das andere eben so durch
die des andern bedingt ist, so erscheint es durchaus als
das Einfachste, diese Glieder als Ausdrücke für die Wir-
kungen der einzelnen Strahlen zu nehmen, d. h. den Aus-
druck (2) als den wirklichen Ausdruck für die Anziehung
eines durchströmten Strahles zu setzen; in der That bringt
jede andere Annahme etwas Fremdartiges in die Formel
hinein, und erscheint daher als eine erkünstelte. Ich
lege daher jenen Ausdruck (2) nämlich $\frac{ib_,}{r} cot \frac{1}{2}\alpha$ als
Ausdruck für die Wirkung eines Strahles in dem oben
näher dargelegten Sinne für die folgende Entwicklung
zu Grunde.

19.) Von hier aus gelangen wir sogleich zu der ge-
genseitigen Einwirkung zweier Stromelemente, indem wir
das anziehende Stromelement ids als Vereinigung zweier
durchströmter Strahlen auffassen können, welche die Rich-
tung und Intensität (i) dieses Elementes haben, und von
denen der eine in gleicher Richtung mit dem Element
der andere in entgegengesetzter von dem (positiven)
Strome durchflossen wird, während der erste den An-
fangspunkt des Elementes zu seinem Anfangspunkte hat,
der letzte den Endpunkt. Man erhält dann

$$\frac{\dot{a}b_,}{r^2} sin\,\alpha \quad \ldots \ldots \ldots \ldots \ldots 4$$

als Ausdruck der Wirkung, welche ein Stromelement a
auf ein anderes, um r von ihm entferntes b, dessen senk-
rechte Projection auf die durch a und r gelegte Ebene
$b_,$ ist, während α den Winkel darstellt, welchen a mit
dem nach b hin gezogenen Strahle bildet; und zwar er-
folgt die Bewegung senkrecht gegen b (oder $b_,$) in der
durch a und r gelegten Ebene nach derjenigen Seite
hin, nach welcher der Schenkel a des Winkels α von

dem andern Schenkel aus betrachtet liegt (siehe Fig. 2 Taf. I) [1]).

10) Betrachten wir zunächst die gegenseitigen Einwirkungen zweier Stromelemente a und b, deren Verlängerungen sich schneiden, so ist klar, daß man beide Bewegungen, da sie gegen die sich bewegenden Stromelemente senkrecht sind, als durch Schwenkung der beiden geraden Linien, denen die Stromelemente angehören, um den Durchschnittspunkt bewirkt ansehen kann. Dann ist der Winkel, um welchen sich eine der Linien, etwa die, welcher b angehört, schwenkt, gleich der Bewegung des Elementes dividirt durch die Entfernung (B) dieses Elementes von jenem Durchschnitte, also gleich:

$$\frac{ab}{r^2}\frac{\sin\alpha}{B} = \frac{ab\sin\varepsilon}{r^3} \;[2]) \;\ldots\ldots\ldots 5$$

Diese Formel lehrt, daß die Strahlen, in welchen beid Elemente liegen, bei der Bewegung einen gleichen Winkel zu beschreiben trachten, während ihr Durchschnittspunkt derselbe bleibt, und auch die Lage der Elemente in den Strahlen sich nicht ändert; auch sieht man leicht, wie der Winkel beider Strahlen durch die Bewegung vermindert wird, wenn die Elemente beide dem Scheitelpunkte zu- oder von ihm abströmen, hingegen vermehrt, wenn das eine dem Scheitelpunkte sich zukehrt, das andere sich von ihm abwendet. Hierdurch tritt die wahre Gegenseitigkeit in der Bewegung ans Licht, und man sieht wie diese gegenseitige Anziehung zweier Li-

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1) Denn man hat den Ausdruck (2) nur nach $-ds$ zu differenziiren, um die Anziehung des Elementes ids zu finden; man erhält statt r, $\cot\frac{1}{2}\alpha$, $d\alpha$ ihre Werthe $\dfrac{l}{\sin\alpha}$, $\dfrac{1+\cos\alpha}{\sin\alpha}$, $\dfrac{lds}{r^2}$ gesetzt, sogleich durch diese Differenziation den zu erweisenden Ausdruck:

$$\frac{ib_{i}ds}{r^2}\sin\alpha \quad \text{oder} \quad \frac{ab_{i}}{r^2}\sin\alpha.$$

2) Da $\dfrac{\sin\alpha}{B} = \dfrac{\sin\varepsilon}{r}$ ist, s. Fig. 2 Taf. I.

nientheile eben so den Winkel zu vermindern trachtet
bei constantem Scheitelpunkte, wie die gegenseitige An-
ziehung zweier Punkte deren Entfernung bei constan-
ter Linie, in der sie liegen, zu vermindern trachtet. So
zeigt sich hier, statt der erkünstelten und scheinbaren
Analogie der Ampère'schen Annahme, eine naturge-
mäfse und wahre Analogie, indem Linien und Punkte
sich in der Ebene eben so einander entsprechen, wie
Winkel und Entfernung, wie Durchschnittspunkt und um-
fassende Linie.

11) Diese Analogie tritt in ein noch helleres Licht,
wenn ich zeige, wie die elektrodynamischen Anziehun-
gen nach der neuen Theorie und die Anziehungen durch
Gravitation sich durch *dieselbe* Formel ausdrücken las-
sen. Zu dem Ende mufs ich jedoch hier den Begriff ei-
ner Verknüpfung anführen, welche ich in einem kürz-
lich erschienenen Werke [1]) dargelegt habe, und zwar
ehe ich von dieser neuen Theorie eine Ahnung hatte.
Ich habe nämlich dort nachgewiesen, dafs man als das
Product zweier Punkte a und b ihre Verbindungsstrecke,
und eben so als das Product zweier, mit bestimmten In-
tensitäten (Gewichten) behafteten Punkte die mit dem
Producte der Intensitäten multiplicirte Verbindungsstrecke
ansehen *müsse;* hiernach würde, wenn α und β Punkte
wären, $\alpha\beta$ die von α nach β gezogene Strecke, welche
nicht blofs ihrer Gröfse, sondern auch ihrer Richtung
nach aufzufassen ist, vorstellen, und wenn etwa 2 und
3 die Intensitäten wären, und a gleich 2α, b gleich 3β
wäre, so würde jene Strecke, ohne Aenderung ihrer Rich-
tung, sechs Mal zu nehmen seyn, um das Product ab
darzustellen. Ich habe dort gezeigt, wie diefs Product
sich von dem arithmetischen dadurch unterscheide, dafs,
wie man sogleich sieht, $a.b$ gleich $-b.a$ ist. Hiernach
würde die Anziehung, welche ein Punkt a auf einen um

1) Die Ausdehnungslehre. Erster Theil, enthaltend die lineale Aus-
dehnungslehre. Die angeführten Sätze finden sich S. 61, 164 und 222.

r entfernten Punkt b durch Gravitation bei beliebigen Gewichten beider Punkte übt, proportional seyn:

$$\frac{a.b}{r^3} \quad \ldots \ldots \ldots \ldots \ldots \ldots 6$$

ein Ausdruck, welcher, vermöge der so eben angegebenen Bedeutung, zugleich die Richtung der Anziehung in sich schliefst. Eben so habe ich dort gezeigt, dafs der Flächenraum eines Parallelogramms als Product zweier aneinanderstofsenden Seiten a und b aufzufassen sey, wenn man an diesen Seiten zugleich ihre Richtung und Länge festhält, und dafs auch hier $a.b$ gleich $-b.a$ sey, und ich habe dort gezeigt, dafs, wenn an a und b zugleich die Linien, in der sie liegen, festgehalten werden sollen, dann das Product den mit jenem Flächenraum zusammengeschauten Durchschnittspunkt beider Linien darstellt. Nun ist der Zähler des Ausdruckes (5) offenbar der Ausdruck für den Flächenraum eines Parallelogramms, welches a und b mit Beibehaltung ihrer Richtungen zu Seiten hat. Somit geht, wenn man unter a und b die Stromelemente mit Feststellung der Linien, in welchen sie liegen, versteht, der Ausdruck (5) über in:

$$6 \quad \ldots \ldots \ldots \ldots \ldots \ldots \frac{a.b}{r^3},$$

welcher identisch ist mit dem für die Anziehung durch Gravitation aufgestellten, und dessen Gröfse die Gröfse der Schwenkung ausdrückt, welche sich beide Elemente mitzutheilen streben, während der durch das Product $a.b$ zugleich dargestellte Punkt das Schwenkungscentrum angiebt.

12) Diese Analogie haben wir nur nachgewiesen, wenn die Stromelemente sich verlängert schneiden. Hiervon ist nicht wesentlich abweichend der Fall, dafs die Stromelemente parallel sind, indem diefs so betrachtet werden kann, als ob ihre Verlängerungen sich in unendlicher Entfernung schnitten. Hingegen wird die Betrachtung schwieriger, wenn die Stromelemente nicht dersel-

ben Ebene angehören. Für diesen Fall will ich nur an-
führen, dafs sich die Bewegung zerlegen läfst in zwei
Bewegungen der Linien, denen jene Elemente angehören,
indem hier das gemeinschaftliche Loth beider Linien (ihre
kürzeste Entfernung) die Stelle des Durchschnittspunktes
vertritt. Die eine dieser Bewegungen besteht in einer
Schwenkung um diefs gemeinschaftliche Loth, welche wie-
der eine Verminderung oder Vergröfserung des Winkels
beider Ströme bewirkt; die andere dieser Bewegungen
besteht in einer Verminderung oder Vergröfserung jenes
Lothes, welche dadurch bewirkt wird, dafs jene Linien
auf diesem Lothe fortrücken. In beiden Fällen ist die
Bewegung eine gegenseitige, die Linien bleiben senkrecht
gegen das gemeinschaftliche Loth, und die Stromele-
mente ändern ihre Lage innerhalb dieser Linie nicht.
Man sieht leicht, wie hier wiederum die vollkommenste
Analogie in der Art der Bewegung mit der durch Gra-
vitation bewirkten stattfindet. Auch würde ich zeigen
können, dafs auch diese Bewegung sich durch den Aus-
druck (6) darstellen läfst. Allein ich kann diesen Nach-
weis hier nicht führen, ohne die Gesetze einer Analyse
zu entwickeln, welche zwar für die Physik von grofser
Bedeutung ist, und oft die scheinbar verwickeltsten Ver-
hältnisse in den einfachsten Formeln darstellt, welche
aber doch sich nicht so in der Kürze darlegen läfst [1]).

13.) Es bleibt mir nun noch übrig die Art anzuge-
ben, wie durch Versuche eine Entscheidung zwischen
beiden Theorien zu Wege gebracht werden könnte. Doch
ehe ich dazu übergehe, will ich eines Versuches erwäh-
nen, den man als beweisend gegen die neue Theorie
ansehen könnte, dessen beweisende Kraft aber freilich
bei genauerer Betrachtung gänzlich verschwindet. Näm-
lich nach der neuen Theorie üben gleichgerichtete Strom-

---

1) Ich verweise in dieser Beziehung auf mein oben angeführtes Werk,
in welchem ich die Anwendungen auf die Physik besonders hervor-
gehoben habe.

theile, welche in derselben geraden Linie liegen, (nach Formel 4) keine Wirkung auf einander aus, nach Ampère stofsen sie sich ab. Nun hat man diefs letztere durch Versuche beweisen wollen, indem man einen geschlossenen Strom, in der Gestalt eines Rechteckes, partiell in der Art beweglich gemacht hat, dafs durch die Bewegung eine Verlängerung des einen Seitenpaares entsteht, woraus man dann, ohne das andere Seitenpaar zu berücksichtigen, auf eine sich gegenseitig abstofsende Kraft derjenigen Stromtheile geschlossen hat, welche hier, in denselben Linien liegend, sich von einander entfernen. Um die Unrichtigkeit dieses Schlusses zu zeigen, brauche ich hier nur auf die obige Entwicklung hinzuweisen, nach welcher beide Theorien, auf geschlossene Ströme angewandt, mögen nun Theile derselben beweglich gemacht seyn, oder nicht, stets gleiches Resultat liefern. Ueberdiefs ist für diesen Fall noch zu bemerken, dafs bei dem Uebergange eines Stromes aus einem Leiter in einen andern eigenthümliche Kräfte wirksam sind, welche, wenn beide in gerader Linie liegen, in dieser Linie wirken, deren Natur und Wirkungsart wir aber noch nicht kennen.

14) Ueberhaupt ist klar, dafs eine Entscheidung zwischen beiden Theorien, da die Wirkung, welche geschlossene Ströme üben, nach beiden dieselbe ist, nur möglich ist, wenn man die Wirkung betrachtet, welche ein begränzter Strom übt. Nun ist aber die Stärke der Strömung bei demselben Leitungswiderstande der Differenz der an seinen Gränzen aufgehäuften Elektricitäten proportional [1]). Soll aber der Strom ein begränzter seyn, so dürfen die nach seinen Gränzen A und B übergeströmten Elektricitäten nicht weiter fortschreiten, weil

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1) Diefs gilt sowohl für jeden Leitungsdraht eines galvanischen Stromes, wie für den durch Reibungselektricität hervorgebrachten, nur dafs dort sich die elektrische Differenz stets auf derselben Höhe erhält. Aus diesem Gesetze läfst sich übrigens das Ohm'sche Gesetz *a priori* ableiten.

sonst eben A und B nicht die Gränzen des Stromes
wären. Folglich wird die Strömung nur so lange fort-
dauern, bis jene Differenz ausgeglichen ist, und das Quan-
tum der hindurchgegangenen Elektricität wird der elek-
trischen Differenz jener Gränzen gleich seyn. Daraus
folgt, dafs man das Maximum des Effects erhält, wenn
jene Differenz ein Maximum ist. Der begränzte Strom
würde daher so hervorzurufen seyn, dafs man zuerst
etwa zwei Kugeln mit entgegengesetzter Elektricität mög-
lichst stark lüde, und sie dann nach der Ladung (nicht
während derselben) in leitende Verbindung brächte. Dann
hätte man die Wirkung dieses begränzten Stromes auf
irgend einen elektrischen Strom oder besser auf einen
Magneten zu beobachten, und die Anordnung dabei so
zu treffen, dafs die Wirkungen nach beiden Theorien
möglichst verschieden erfolgten.

. 15) Da durch einen eingeschalteten Multiplicator
oder durch Anwendung einer Batterie, statt jener einfa-
chen Entladung, der begränzte Strom einem geschlosse-
nen angenähert, die Differenz der Wirkungen nach bei-
den Theorien also vermindert werden würde, so sind
diese Mittel zur Verstärkung der Wirkungen hier nicht
anwendbar, und man sieht daher die Schwierigkeiten,
welchen Versuche dieser Art unterliegen würden. Da
indessen diese Schwierigkeiten nicht an sich unüberwind-
liche sind, so wird es dennoch von Interesse seyn, die-
jenige Anordnung zu kennen, bei welcher ein Maximum
in der Differenz der Wirkungen nach beiden Theorien
erfolgte. Diefs Maximum findet nun, nach meinen Un-
tersuchungen, dann Statt, wenn die Magnetnadel senk-
recht gegen den geradlinigten Strom so aufgestellt wird,
dafs ihre Mitte in der Verlängerung jenes Stromes liegt,
und sich senkrecht gegen die durch den Strom und die
Nadel gelegte Ebene frei bewegen kann. Zur Erläute-
rung diene Fig. 3 Taf. I, in welcher AB den begränz-
ten Strom vorstellt, so dafs die positive Elektricität von

A nach B strömt, und wo zwei Magneten, deren Nord-
enden mit N bezeichnet sind, durch einen Bogen $S C N$
aus einer festen Substanz verbunden sind, welcher in C
an einem Faden aufgehängt ist.

16) Setzen wir, um für diesen Fall die Wirkungen,
welche der begränzte Strom nach beiden Theorien zu-
nächst auf unendlich kleine Magneten üben würde, zu
finden, statt des Magneten NS einen dagegen senkrech-
ten quadratischen Strom, welcher mit AB in gleicher
Ebene liegt, und von dessen vier Seiten zwei mit AB
parallel, die andern also dagegen senkrecht sind, und
zwar so, dafs das Nordende des Magneten von diesem
Strome aus betrachtet nach links hin liegt, so ist nach
beiden Theorien die Bewegung nach der gegen AB senk-
rechten Richtung nur von den mit AB parallelen Strom-
theilen abhängig. Ist nun ids ein Stromelement von AB
und b das mit AB gleichgerichtete b' das mit ihm ent-
gegengesetzt gerichtete Stromelement des quadratischen
Stromes, so ist, wenn r die Entfernung ihrer Mitten von
der Mitte des anziehenden Elementes ids ist, die Wir-
kung auf b nach der dagegen senkrechten Richtung ab-
stofsend gleich $\dfrac{idsb^2}{2r^3}$ [1]), und eben so die auf b', nur
dafs diese anziehend wirkt; beide Wirkungen, da sie die
Bewegung des Quadrates von b' nach b darstellen, sum-
miren sich, und geben $\dfrac{idsb^2}{r^3}$ als die Kraft, mit welcher,
nach Ampère, der quadratische Strom in der Richtung
von b' nach b getrieben wird. Nach meiner Formel ist

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1) Nämlich nach 1* ist sie in der Richtung r gleich $\dfrac{idsb}{r^2}(2-3cos^2\alpha)$,

also in der gegen b senkrechten gleich $\dfrac{idsb}{r^2}(2-3cos^2\alpha)sin\alpha$, also

da α unendlich klein $sin\alpha$ gleich $\dfrac{\frac{1}{2}b}{r}$ ist, gleich $-\dfrac{idsb^2}{2r^3}$, also ab-
stofsend.

die Wirkung auf b nach der dagegen senkrechten Richtung gleich $\frac{idsb^2}{2r^3}$ anziehend [1]), auf b' eben so grofs, aber abstofsend, also wirken beide zusammen zur Bewegung des Quadrates in der Richtung von b nach b' mit der Kraft $\frac{idsb^2}{r^3}$. Somit sind die Wirkungen nach beiden Theorien entgegengesetzt; und diese Beziehung wird auch bestehen bleiben, wenn man statt des unendlich kleinen Stromelementes ids und eines unendlich kleinen Magneten einen endlichen Strom AB und einen endlichen Magneten setzt, nur dafs in dem letzteren Falle die Wirkungen nicht mehr von gleicher Gröfse sind. Die Wirkungen lassen sich auf folgende Weise ausdrücken: »Wenn man sich bei der angenommenen Anordnung (Fig. 3 Taf. I) in die Richtung der Magnetnadel versetzt (den Kopf nach dem Nordende, die Füfse nach dem Südende gerichtet) und das Auge nach derjenigen Richtung wendet, nach welcher der positive Strom AB fliefst, so wird die Nadel, nach der Ampère'schen Theorie, nach der *rechten* Hand hin, nach der neuen Theorie, nach der *linken* Hand hin bewegt.«

· 17) Schliefslich will ich noch auf zwei sehr unwahrscheinliche Wirkungen hindeuten, welche ein begränzter Strom, nach Ampère, auf einen Magneten üben müfste; nämlich erstens würde danach ein Magnet durch einen begränzten Strom zugleich eine drehende Bewegung um seine magnetische Axe annehmen, welche in dem vorher (No. 16) betrachteten Falle ihr Maximum erreicht; und zweitens würde eine Magnetnadel, welche um ihren Mittelpunkt frei beweglich ist, in der Nähe eines begränzten Stromes, sofern nur dieser auf sie wirkt, im Allgemeinen keine Lage eines sicheren Gleichgewichts

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1) Nämlich sie ist gleich $\frac{idsb}{r^2} \sin \alpha$, also, da $\sin \alpha$ gleich $\frac{\frac{1}{2}b}{r}$ ist, gleich dem oben angeführten Ausdrucke und zwar anziehend. ·

18

annehmen, sondern bei der Entfernung aus der Gleich-
gewichtslage würde sie theils wieder zurückgehen, theils
aber auch in die entgegengesetzte Lage umschlagen, je
nachdem sie nach dieser oder jener Seite hin aus jener
Lage entfernt war.

18) Wenn ich nun gleich hoffen darf, durch die
vorhergehende Entwicklung die neue Theorie als in je-
der Hinsicht wahrscheinlich dargethan zu haben, so steht
doch zu wünschen, dafs durch die Erfahrung eine über
alle Zweifel erhobene Entscheidung zwischen dieser und
der Ampère'schen Annahme zu Stande gebracht werde.
Möchte es bald einem geübteren Physiker gelingen, alle
die Hindernisse hinwegzuräumen, welche jenem entschei-
denden Versuche, den ich vorher anführte, im Wege
zu stehen scheinen.
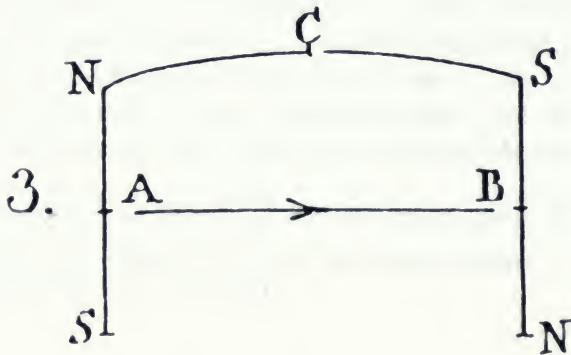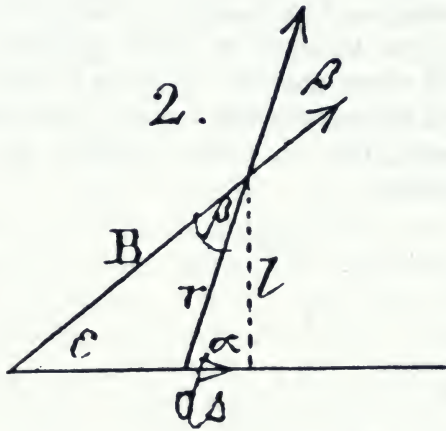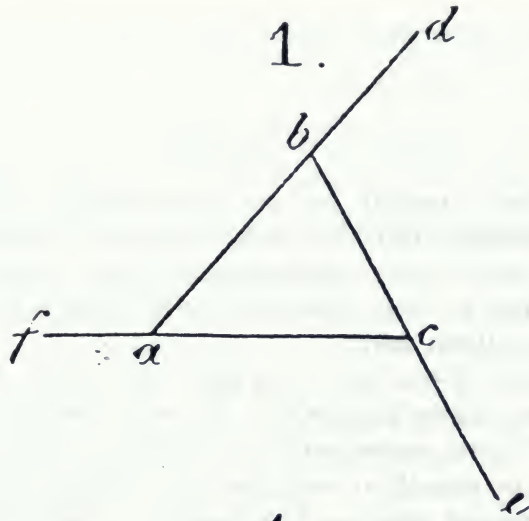
MARINOV'S COMMENTS TO THE PREVIOUS PAPER BY H. GRASSMANN

At the time of Graßmann vector algebra and vector calculus were in the cradle. Thus it is very difficult for us to follow Grassmann's reasonings and we can only wonder, seeing that without the appropriate mathematical tools and with a very limited experimental basis, Grassmann arrives at the discovery of his formula which is the <u>fundamental formula</u> for the magnetic interactions (the "cardinal formula" of electromagnetism).

Grassmann's formula in vector form is more simple than Ampere's formula and it opens immediately the door for introducing the magnetic potential **A** and thus for introducing the magnetic intensity **B**. However if vectors will be not used, then Ampere's formula looks more simple. And if we take Ampere's formula in its vector form (omitting the constant factor depending on the system of measuring units)

$$\mathbf{f} = \{3(\mathbf{r.a})(\mathbf{r.b}) - 2(\mathbf{a.b})r^2\}\mathbf{r}/r^5, \tag{A}$$

where **f** is the force with which the current element **a** acts on the current element **b**, we come at once to the scalar formula (1) of Grassmann's paper, if putting $\mathbf{r.a} = ra\cos\alpha$, $\mathbf{r.b} = rb\cos\beta$, $\mathbf{a.b} = ab\cos\varepsilon$, and $\mathbf{f}/f = - \mathbf{r}/r$.

However it is not at all as easy to come from Grassmann's formula in vector form

$$\mathbf{f} = \mathbf{b}\times(\mathbf{a}\times\mathbf{r})/r^3 \tag{B}$$

to the scalar formula (4) in Grassmann's paper.

I shall do it, in order to spare the time of the reader.

Let us take the current element **a** at the origin of the reference frame lying in the xy-plane, i.e., $\mathbf{a} = a_x\hat{\mathbf{x}} + a_y\hat{\mathbf{y}}$, assuming $a_x = a\cos\alpha > 0$, $a_y = a\cos(\pi/2 - \alpha) = a\sin\alpha > 0$. Let us take the vector **r** connecting **a** with **b** and pointing from **a** to **b** along the x-axis, i.e., $\mathbf{r} = r\hat{\mathbf{x}}$, assuming $r > 0$. The orientation of the vector **b** must be arbitrary, i.e., $\mathbf{b} = b_x\hat{\mathbf{x}} + b_y\hat{\mathbf{y}} + b_z\hat{\mathbf{z}}$, but for simplicity's sake let us assume $b_x > 0$, $b_y > 0$, $b_z > 0$.

Now formula (B) can be written

$$\mathbf{f} = \mathbf{b}\times(\mathbf{a}\times\mathbf{r})/r^3 = (b_x\hat{\mathbf{x}}+b_y\hat{\mathbf{y}}+b_z\hat{\mathbf{z}})\times\{(a_x\hat{\mathbf{x}}+a_y\hat{\mathbf{y}})\times r\hat{\mathbf{x}}\}/r^3 = - (b_\ell\hat{\boldsymbol{\ell}} + b_z\hat{\mathbf{z}})\times a_y\hat{\mathbf{y}}/r^2 =$$
$$- b_\ell a\sin\alpha(\hat{\boldsymbol{\ell}}\times\hat{\mathbf{z}})/r^2, \tag{C}$$

where $\mathbf{b}_\ell = b_\ell\hat{\boldsymbol{\ell}} = (b_x^2 + b_y^2)^{1/2}\hat{\boldsymbol{\ell}}$ is the component of **b** in the xy-plane and $\hat{\boldsymbol{\ell}}$ is the unit vector along the direction of this component.

We see that the unit vector $-\hat{\boldsymbol{\ell}}\times\hat{\mathbf{z}}$ lies in the xy-plane and as $\mathbf{b}_\ell$ concludes with $\hat{\mathbf{x}}$ an angle $0 < \beta' < \pi/2$, the vector $- \hat{\boldsymbol{\ell}}\times\hat{\mathbf{z}}$ concludes with $\hat{\mathbf{x}}$ an angle $\pi/2 < \beta'' < \pi$, as $\beta'' = \pi/2 + \beta'$. Thus the force f points from the end of the vector **r** to the end of the vector **a** (und zwar erfolgt die Bewegung senkrecht gegen $\mathbf{b}_\ell$ in der durch a und r gelegten Ebene nach derjenigen Seite hin, nach welcher der Schenkel a des Winkels α von dem anderen Schenkel aus (i.e., from the "Schenkel" **r**) betrachtet liegt).

I tried to find also Ampere's original paper where he introduces his famous formula (Ampère, "Mémoire sur la théorie mathématique des phénomènes electrodynamique", MÉMOIRES

DE L'ACADEMIE DE PARIS, vol. VI, 1823), however I could   come neither to the origi-
nal publication nor to some later publication of this paper. The deduction of this for-
mula which Maxwell gives in his "TREATISE" (§§ 502 - 507) is <u>atrocious</u>. And I am sure
that if one should ask some of the defenders of Ampere's formula, how <u>physically</u> has
one to deduce this formula, NONE of them will be able to do this.

I consider as the best <u>mathematical</u> deduction of Ampere's formula the deduction
given by Wesley (PROGRESS IN SPACE-TIME PHYSICS 1987, p. 199) when proceeding from
the Weber potential energy of two electrical charges moving with a certain velocity one
with respect to the other. This deduction is given also in the paper of Prof. D. Spen-
cer published in this volume.

The <u>physical</u> deduction of Grassmann's formula is based   on a complex of ex-
perimental facts which have been crystallized in the assumptions (A), (B) and (D) given
in my letter to Prof. Wesley of the 20 June 1990 (published in this volume).

The <u>mathematical</u> deduction of Grassmann's formula when proceeding from the Neumann
potential energy of two electric charges moving with teir respective absolute veloci-
ties is given in TWT-II, p. 82.

I leave the physical deduction with which Grassmann comes to his formula to my rea-
ders' teeth.

At the end I should like to turn my readers' attention to the two arguments in §3
of Grassmann's paper   against Ampere's formula.

The first argument was already discussed on p. 91 of TWT-II.

Here I shall cite the second argument:

> Denn bei allen anderen Kräften sind es ursprünglich punktartige Elemente, d.h. Ele-
> mente ohne bestimmte Richtungen, welche aufeinander wirken, und bei diesen läßt
> sich die Notwendigkeit der gegenseitigen Wirkung längs ihrer Verbindungslinie
> sogar *a priori* ableiten; was berechtigt uns aber, diese Analogie auf ein ganz
> fremdartiges Gebiet, auf welchem die Elemente mit bestimmten Richtungen begabt
> sind, zu übertragen?

On the other hand, however, if assuming that the forces with which two current ele-
ments act one on another are not equal and oppositely directed along the line connecting
them, one comes to a conflict with Newton's third law. And we know well that Ampere de-
duced his formula proceedings from four <u>experimental facts</u> observed by him (which until
the present day remained valid - see them described in Maxwell's "TREATISE" §§ 505-508)
and one <u>theoretical assumption</u>: Newton's third law. But this theoretical assumption,
although being canonized by the genius of all times, Newton, turned out to be <u>wrong</u> for
the case of interaction between current elements (as I showed in EPPUR SI MUOVE and
CLASSICAL PHYSICS, Newton's third law is valid for the <u>full</u> forces $m\mathbf{u} + qd\mathbf{A}/cdt$ but
not for the forces $m\mathbf{u}$, where $\mathbf{u}$ is the acceleration of the mass m whose electric charge
is q and $\mathbf{A}$ is the magnetic potential generated by the surrounding system at the point
of its location).

To establish whether Newton's third law is right at the interaction between current elements, one had to do experiments with unclosed loops. Such experiments have been done first by Graham and Lahoz (NATURE, **285**, 154, 1980) and then by me (see my Bul-Cub Machine without Stator in TWT-III and my Rotating Ampere Bridge with Displacement Current in TWT-IV). The results of these experiments were against Newton.

Grassmann noted on several places of his article that only by doing experiments with unclosed circuits can one establish whether his or Ampere's formula is the right one.

I shall cite Grassmann's words on this topic from his Introduction:

Ich habe schon angedeutet, daß diese neue Annahme, auf alle bisher beobachteten Erscheinungen angewandt, dieselbe Resultate liefert, wie die Ampere'sche; hingegen gibt es ein Gebiet der Erscheinungen, auf welchem nach beiden Annahmen oft gerade die entgegengesetzten Erfolge eintreten müßten, und welches dacher von Seiten der Erfahrung her die einzige Entscheidung über die Richtigkeit der einen oder der anderen Annahme liefern würde. Es ist dies, wie ich am Schluße dieser Abhandlung zeigen werde, das Gebiet der Strömungen, welche durch freie, an den Enden einer Leitung aufgehäufte (etgegengesetzte) Elektrizitäten hervorgebracht werden, also das Gebiet der durch Maschinenelektrizität hervorgerufenen Strömungen. Die Versuche, welche man bisher auf diesem Gebiete angestellt hat, um elektrodynamische Wirkungen, wie z. B. die Ablenkung einer Magnetnadel, nachzuweisen, sind sehr weit davon entfernt, die Differenz beider Hypothesen irgend wie hervortreten zu lassen. Auch stellen sich solchen Versuchen, welche dies leisten können, bisher noch bedeutende Schwierigkeiten entgegen. Dennoch scheint es mir wichtig, eine Hypothese als wahrscheinlich nachzuweisen, welche die Erfolge vorhersagen würde, die bei feineren Instrumenten und genaueren Beobachtungen eintreten müßten. Eine solche Annahme würde ein leitendes Prinzip werden, wonach von geübter Hand vielleicht bald entscheidende Versuche angestellt werden könnten. Es sei mir daher erlaubt, hier diese neue Annahme abzuleiten, und geübteren Physikern zur Prüfung vorzulegen.

And then from Grassmann's §1:

Hingegen hat man keinen Versuch angestellt, um die Wirkung eines Stromteils zu prüfen, weder die, welche er auf einen geschlossenen Strom, noch die, welche er auf einen anderen Stromteil übt.

Grassmann's words on this topic from his § 14 are cited in my paper "On the electric intensities induced in railguns" which is published in this volume.

And let me cite also Maxwell on this topic ("TREATISE", §509):

It may·be observed with reference to these experiments (the four experiments of Ampere - S.M.) that every electric current forms a closed circuit. The currents used by Ampere, being produced by the voltaic battery, were of course in closed circuits. It might be supposed that in the case of the current of discharge of a conductor by a spark we might have a current forming an open finite line, but

according to the views of this book even this case is that of a closed circuit (circuits with sparks are closed circuits but alternating currents ending at condensers without sparks are UNCLOSED circuits - S.M.). No experiments on the mutual action of unclosed currents have been made (my emphasize - S.M.). Hence no statement about the mutual action of two elements of circuits can be said to rest on purely experimental grounds. It is true we may render a portion of a circuit movable, so as to ascertain the action of the other currents upon it, but these currents, together with that in the movable portion, necessarily form closed circuits, so that the ultimate result of the experiment is the action of one or more closed currents upon the whole or a part of a closed current.

Thus the conclusion is: To be able to establish which is the formula governing the interaction between current elements one needs not four experiments and one theoretical assumption, but five experiments and no theoretical assumption. The fifth experiment is an experiment with unclosed circuits.

GRAVITY, ELECTRICITY, AND MAGNETISM

according to the lectures of
Bernhard Riemann

compiled by
Karl Hattendorf

ENERGY POTENTIAL

TOWARD A NEW ELECTROMAGNETIC FIELD THEORY

by Carol White

Preface

This book has resulted from the lectures that Riemann gave on gravity, electricity, and magnetism during the summer semester of 1861 in Göttingen. With the exception of some very brief notes, there is no manuscript extant by Riemann himself on these lectures. Thus, I alone am responsible for this presentation.

The friendly reception that my compilation of Riemann's lectures on partial differential equations has found among all those experts on the subject leaves me hope that this present book will not be unwelcome to friends of Riemann and those studying mathematics.

Just as in partial differential equations, here too we have to thank Lejeune Dirichlet. In addition to his great service to the further development of science it must not be forgotten that it was he who was the first to lecture about partial differential equations and the potential at German universities. These lectures did not end with his death. They now form a regular part of the program at almost all German universities, and Riemann too took over these lectures after Dirichlet. Concerning agreement on the subject matter, then, it is natural that much here agrees with Dirichlet in layout and execution. But Riemann did not limit himself to simply taking possession of his great predecessor's legacy. The connoisseur will discover that he has submitted an abundance of what is characteristically his.

<div style="text-align:right">

K. Hattendorff.
Aachen, June 24, 1875

</div>

EIGHTH DIVISION

The Fundamental Law Of Electric Interaction

Section 94
The Potential Of The Interaction Of Two Currents

The theorem in section 93 can be transferred immediately to two nonlinear closed currents. One only has to assume that the specific current intensities will undergo only infinitely small changes in time element dt at every place in the first as well as in the second conductor and to advance the hypothesis that the total work that originates in the interaction of both galvanic currents in time element dt is the complete differential of a function which possesses the characteristic properties of a potential (in the broader sense).

In order to comprehend this, one only needs to consider that one can conceive of one as well as the other nonlinear current as each being a system of linear currents.

The train of thought in section 93 will be repeated here. In equations (5), (6), and (7) in section 89, we found these expressions for function P

$$P = -\int dS \, (u_1' \, i_1 + u_2' \, i_2 + u_3' \, i_3)$$

$$(1) \qquad = -\int dS' (u_1 \, i_1' + u_2 \, i_2' + u_3 \, i_3')$$

$$= \int \int \frac{dS \cdot dS'}{r} \, (i_1 \, i_1' + i_2 \, i_2' + i_3 \, i_3').$$

We will now also want to consider this function P for the case where the specific current intensity can be independent of time. What matters then are the changes that function P undergoes in time element dt under the various permissible assumptions. $\delta_i P$ will denote the change that occurs when the specific current intensities in both conductors are regarded as independent of t, $\delta_{ii} P$ will denote the change that originates when one

considers the specific current intensities in the second conductor only as independent of time t, and $\delta_{ri'}$P will denote the change that results when the specific current intensitites are assumed to be independent of t only in the first conductor. Finally, dP will be P's complete differential that occurs in time element dt when the mutual position of the elements of the first and second conductor and the specific current intensities every place in both conductors undergo infinitely small changes in every time element.

First of all we have

(2) $\quad dP = -\delta_r P + \delta_{ri} P + \delta_{ri'} P.$

If one presumes that both currents are constant, then, according to section 89, the electrodynamic elementary work performed in the time interval from t to t+dt is

(3) $\quad \delta_r P.$

This expression, then, will still remain correct for the electrodynamic elementary work when the specific current intensities undergo infinitely small changes in time element dt every place in one, as well as in the other conductor. In this case, δ_rP is not a complete differential and consequently there is no potential present for the electrodynamic work alone. But, *electromotive work* that originates in the interaction of both galvanic currents will still be performed in both conductors.

We will advance the hypothesis that a potential exists for the total work that is performed by virtue of the interaction of both galvanic currents. In order to find this total work, we have to add that kind of contribution that is the sum of a complete differential to equation (3). This contribution is

(4) $\quad -\delta_{ri} P - \delta_{ri'} P$

and the sum is then the complete differential of -P.

Consequently,

$$
\begin{aligned}
D_1 &= \int dS \, (u_1' \, i_1 + u_2' \, i_2 + u_3' \, i_3) \\
(5) \quad &= \int dS'(u_1 \, i_1' + u_2 \, i_2' + u_3 \, i_3') \\
&= -\int \int \frac{dS \cdot dS'}{r} (i_1 \, i_1' + i_2 \, i_2' + i_3 \, i_3')
\end{aligned}
$$

is the potential of the interaction of both galvanic currents.

The total work breaks down into three terms, namely,

first: the electromotive work in the first conductor:

$$
dt \int dS \left(i_1 \, \frac{du_1'}{dt} + i_2 \, \frac{du_2'}{dt} + i_3 \, \frac{du_3'}{dt} \right);
$$

second: the electromotive work in the second conductor:

$$dt \int dS^{\cdot} \left(i_1^{\cdot\cdot} \frac{du_1}{dt} + i_2^{\cdot\cdot} \frac{du_2}{dt} + i_3^{\cdot\cdot} \frac{du_3}{dt} \right);$$

third: the electrodynamic work of both currents on one another:

$$dt \int \int dS \cdot dS' \frac{d \left(\frac{1}{r} \right)}{dt} (i_1 \, i_1^{\cdot\cdot} + i_2 \, i_2^{\cdot\cdot} + i_3 \, i_3^{\cdot\cdot}).$$

After we have gotten acquainted with the potential of the interaction of both galvanic currents, we will attempt to explain this interaction from the interaction of the individual electric particles.

For this purpose, it is necessary to discuss generally how the theorems in sections 36 to 43 are to be altered when the potential is not only dependent on the coordinates, but also on the velocities of the moving material points.

Section 95

The Expanded Theorem Of LaGrange:

$$\delta \int_0^t (T - D + S) \, dt = 0$$

We will consider a system of moving material particles. T is the kinetic energy of this system. The expression for the work performed (the potential) at time t may be broken down into two parts, $S+D$, so that S is only dependent on the particle's coordinates, with D, moreover, still dependent on the velocities. We will denote x, y, z as the coordinates for any one of the material points, and will write $(dx/dt)=x'$, $(dy/dt)=y'$, $(dz/dt)=z'$ as an abbreviation and, correspondingly, the second derivatives. The components of the force acting on point (x, y, z) are X, Y, Z. The work performed in time element dt, after the expiration of time t, is

(1) $\sum (Xx' + Yy' + Zz') \, dt.$

The summation is to be extended over all the points. This work is equal to the increase that the potential undergoes in time element dt:

(2) $\sum (Xx' + Yy' + Zz') \, dt = \left(\frac{dS}{dt} + \frac{dD}{dt} \right) dt.$

But now we have

(3) $\quad \dfrac{dS}{dt} = \sum \left(x' \dfrac{\partial S}{\partial x} + y' \dfrac{\partial S}{\partial y} + z' \dfrac{\partial S}{\partial z} \right),$

(4)
$$\dfrac{dD}{dt} = \sum \left(x' \dfrac{\partial D}{\partial x} + y' \dfrac{\partial D}{\partial y} + z' \dfrac{\partial D}{\partial z} \right)$$
$$+ \sum \left(x'' \dfrac{\partial D}{\partial x'} + y'' \dfrac{\partial D}{\partial y'} + z'' \dfrac{\partial D}{\partial z'} \right).$$

What results from equation (2) is that no term can occur in $(dS/dt)+(dD/dt)$ that does not contain one of the velocity components as a factor. Derivative dS/dt satisfies this condition. In order that the same is the case with dD/dt, no term can be present in D in which the velocities would only occur in the first power. For then, the second component of dD/dt would be loaded with terms which would be free of x', y', z'. So, one sees that in D the magnitudes x', y', z' must be at least contained in the *second* power.

As the simplest example, we will take a homogeneous function of the second degree of x', y', z' for D:

(5) $\quad D = \sum_{ij} \left\{ \begin{array}{l} A_{ij}\, x'_i x'_j + \quad B_{ij}\, y'_i y'_j + \quad C_{ij}\, z'_i z'_j \\ +2 D_{ij}\, z'_i x'_j + 2 E_{ij}\, x'_i y'_j + 2 F_{ij}\, y'_i z'_j \end{array} \right\}.$

Coefficients $A_{ij} \ldots F_{ij}$ are functions of the coordinates of all the points. Derivative dD/dt will then consist of a homogeneous function of the third degree of x', y', z' and a homogeneous function of the first degree of the same variable, and the coefficients that occur are functions of coordinates x, y, z. However, the homogeneous linear function of x', y', z' that occurs in dD/dt, just like function dS/dt, already has form (1) by itself, and cannot be put into this form in any other way. But, on the other hand, the function of the third degree occurring in dD/dt can be put into form (1) through a manifold of ways. So, the forces in motion are not totally determined by the expression for work.

The theory of the conservation of kinetic energy is expressed in the formula $T-S-D=\text{const}$. We will now inquire how the motion must proceed so that this theory is valid.

We have a clue in section 43 as to how to answer this question. There, it is proven that:

When P is only dependent on the coordinates $q_1, q_2, \ldots$ and this function's expression explicitly does not contain time t and, furthermore, when T is a homogeneous function of the second degree of q_1', q_2', $\ldots$, then

$$\delta \int_0^t (T + P)\, dt = 0$$

is the necessary and sufficient condition so that T-P=const. S, here, is only a function of coordinates x, y, z ..., a function whose expression does not explicitly contain time t, while it is T-D that is a homogeneous function of the second degree for x', y', z' .... Consequently, we can immediately apply the theorem in section 43, which will now read:

When motion is to proceed so that the theory of the conservation of kinetic energy

(6) $T - S - D = \text{const.}$

is valid, then the following necessary and sufficient condition is to be satisfied:

(7) $\delta \int\limits_{0}^{t} (T - D + S)\, dt = 0.$

This condition yields differential equations of form (6) in section 42. There, one only has to write T-D for T, and S for P to get our case above.

Section 96

The Potential Of Two Electric Particles. Weber's Form

The point is now to apply the theorem in section 95 to the case where the moving material points are electric particles and where the forces that are in motion due to their influence are the forces of mutual attraction and repulsion.

In this problem, D is the potential of the interaction of the electric particles, to the extent that it is co-dependent on the velocities. D will consist of three parts, namely, the potential D_1 of both currents on each other, the potential D_2 of the first current on itself, and the potential D_3 of the second current on itself. According to (5) in section 94,

(1) $D_1 = - \int \int \frac{dS \cdot dS'}{r} (i_1\, i_1' + i_2\, i_2' + i_3\, i_3').$

When an electrical conductor is moving and the electrical particles inside it are simultaneously in motion, then one can break down the motion of every such particle into two parts, namely, the motion which the conductor imparts to it, and its motion *relative* to the conductor. Then, dx/dt, dy/dt, dz/dt are the components of the absolute velocity of the electrical particle ϵ that is concentrated in point (x, y, z) and $v_1' v_2' v_3'$ are the components of the absolute velocity of elements of the conductor.

Then,

$$w_1 = \frac{dx}{dt} - v_1, \quad w_2 = \frac{dy}{dt} - v_2, \quad w_3 = \frac{dz}{dt} - v_3$$

are the components of the velocity of the electric particle relative to the conductor.

We will denote x, y, z as the coordinates of a point of dS, and x_1, y_1, z_1 as the coordinates of a point of dS'. Then, $r^2 = (x-x_1)^2 + (y-y_1)^2 + (z-z_1)^2$ and, consequently, by means of differentiation,

$$\frac{\partial^2 (r^2)}{\partial x\, \partial x_1} = \frac{\partial^2 (r^2)}{\partial y\, \partial y_1} = \frac{\partial^2 (r^2)}{\partial z\, \partial z_1} = -2, \quad \frac{\partial^2 (r^2)}{\partial x\, \partial y_1} = \frac{\partial^2 (r^2)}{\partial x_1\, \partial y} = 0,$$

$$\frac{\partial^2 (r^2)}{\partial y\, \partial z_1} = \frac{\partial^2 (r^2)}{\partial y_1\, \partial z} = 0, \quad \frac{\partial^2 (r^2)}{\partial z\, \partial x_1} = \frac{\partial^2 (r^2)}{\partial z_1\, \partial x} = 0.$$

If we now introduce a function F by means of the equation

(2) $\quad F = i_1'' \dfrac{\partial (r^2)}{\partial x_1} + i_2'' \dfrac{\partial (r^2)}{\partial y_1} + i_3'' \dfrac{\partial (r^2)}{\partial z_1},$

then we have

$$\frac{\partial F}{\partial x} = -2\, i_1'', \quad \frac{\partial F}{\partial y} = -2\, i_2'', \quad \frac{\partial F}{\partial z} = -2\, i_3''.$$

As a result of this, expression (1) for D_1 can be put into the following form:

(3) $\quad D_1 = \dfrac{1}{2} \displaystyle\int\!\!\int \dfrac{dS\, dS'}{r}\Big(i_1 \dfrac{\partial F}{\partial x} + i_2 \dfrac{\partial F}{\partial y} + i_3 \dfrac{\partial F}{\partial z}\Big).$

We will begin with integration over the first conductor; thus, with integral

$$\int \frac{dS}{r}\Big(i_1 \frac{\partial F}{\partial x} + i_2 \frac{\partial F}{\partial y} + i_3 \frac{\partial F}{\partial z}\Big)$$

through integration by parts [equations (1) and (2) in section 20], what we obtain for this is

(4)
$$-\int dS \cdot F \cdot \left\{ \frac{\partial \left(\frac{i_1}{r}\right)}{\partial x} + \frac{\partial \left(\frac{i_2}{r}\right)}{\partial y} + \frac{\partial \left(\frac{i_3}{r}\right)}{\partial z} \right\}$$
$$-\int d\sigma \cdot \frac{F}{r} \cdot \left\{ i_1 \frac{\partial x}{\partial n} + i_2 \frac{\partial y}{\partial n} + i_3 \frac{\partial z}{\partial n} \right\},$$

and the first of these integrals is to be extended over the space of the first conductor, while the second is to be extended over its surface. However,

we will assume that there are currents in which the density of the free electricity does not change in any place [section 57, equation (1)] and from which the conductor's surface is insulated [section 57, equation (2)]. We thus have

$$\frac{\partial i_1}{\partial x} + \frac{\partial i_2}{\partial y} + \frac{\partial i_3}{\partial z} = 0, \quad i_1 \frac{\partial x}{\partial n} + i_2 \frac{\partial y}{\partial n} + i_3 \frac{\partial z}{\partial n} = 0.$$

Hereafter, the volume integral in (4) simplifies itself and the surface integral drops out completely. As a consequence, expression (3) changes into

$$(5) \quad D_1 = -\frac{1}{2} \int\int dS\, dS'.\, F. \left\{ i_1 \frac{\partial\left(\frac{1}{r}\right)}{\partial x} + i_2 \frac{\partial\left(\frac{1}{r}\right)}{\partial y} + i_3 \frac{\partial\left(\frac{1}{r}\right)}{\partial z} \right\}.$$

In this formula, one really needs only to work out the differentiation for $1/r$ and to use function F from equation (2) once again in order to get this new expression,

$$(6) \quad D_1 = \int\int \frac{dS\, dS'}{r} \left(i_1 \frac{\partial r}{\partial x} + i_2 \frac{\partial r}{\partial y} + i_3 \frac{\partial r}{\partial z} \right)\left(i_1' \frac{\partial r}{\partial x_1} + i_2' \frac{\partial r}{\partial y_1} + i_3' \frac{\partial r}{\partial z_1} \right).$$

For a further transformation, it will be profitable to take into consideration the connection between the specific current intensities and the velocity components of the single electric particle. For, according to equation (5) in section 54, using the denotation employed here,

$$i_1\, dS = \sum \epsilon\, w_1 = \sum \epsilon\, \frac{dx}{dt} - \sum \epsilon\, v_1,$$

$$i_2\, dS = \sum \epsilon\, w_2 = \sum \epsilon\, \frac{dy}{dt} - \sum \epsilon\, v_2,$$

$$i_3\, dS = \sum \epsilon\, w_3 = \sum \epsilon\, \frac{dz}{dt} - \sum \epsilon\, v_3.$$

The summation extends over all the electric particles contained in spatial element dS. And, for one and the same conductor element, the velocity components v_1, v_2, v_3 can be taken in front of the summation signs. Since free electricity is not present any place in the interior of the conductor, what we have is

$$(7) \quad \sum \epsilon = 0.$$

Consequently, the last equations simplify themselves and we obtain

$$(8) \quad i_1\, dS = \sum \epsilon\, \frac{dx}{dt}, \quad i_2\, dS = \sum \epsilon\, \frac{dy}{dt}, \quad i_3\, dS = \sum \epsilon\, \frac{dz}{dt}.$$

Three corresponding equations result for the spatial element dS' of the second conductor. With the aid of these equations, expression (6) changes

into

$$(9) \quad D_1 = \sum\sum \frac{\varepsilon \varepsilon'}{r}\left(\frac{\partial r}{\partial x}\frac{dx}{dt} + \frac{\partial r}{\partial y}\frac{dy}{dt} + \frac{\partial r}{\partial z}\frac{dz}{dt}\right)$$

$$\times \left(\frac{\partial r}{\partial x_1}\frac{dx_1}{dt} + \frac{\partial r}{\partial y_1}\frac{dy_1}{dt} + \frac{\partial r}{\partial z_1}\frac{dz_1}{dt}\right).$$

The first summation is to be extended over all the electrical particles of the first current and the other summation is to be extended over all the particles of the second current.

Equation (9) can be written even more simply. Namely, if one denotes the change beginning in time dt by r and the change originating in the motion of particle ε by δr and the corresponding change in r originating in the motion of particle ε' by $\delta'r$, then what finally results is

$$(10) \quad D_1 = \sum\sum \frac{\varepsilon \varepsilon'}{r} \cdot \frac{\partial r}{dt} \cdot \frac{\partial' r}{dt}.$$

This expression gives potential D_1 as dependent on the *absolute* motion of the electric particles. And now, such terms as cancel themselves out in summation can also be added to equation (10); through their introduction it is brought about that only the *relative* velocity occurs.

The sum of these terms is

$$(11) \quad \frac{1}{2}\sum\sum \frac{\varepsilon \varepsilon'}{r}\left\{\left(\frac{\partial r}{dt}\right)^2 + \left(\frac{\partial' r}{dt}\right)^2\right\}.$$

It is easy to see that this double sum has the value of zero. For if we begin in

$$\sum\sum \frac{\varepsilon \varepsilon'}{r}\left(\frac{\partial r}{dt}\right)^2$$

with summation over the second conductor, then factor ε can be removed from the inner summation sign. For any single element of the second conductor $1/r(\delta r/dt)^2$ will be constant and $\Sigma\varepsilon' = 0$. Consequently, every element of the second conductor furnishes a contribution of zero to the sum and, therefore, the whole sum is equal to zero. We can show, in a corresponding manner, that the second component in (11) also has a value of zero.

If we now add contribution (11) to the right-hand side of (10) and write $(\delta r/dt) + (\delta'r/dt) = dr/dt$, then we get

$$(12) \quad D_1 = \frac{1}{2}\sum\sum \frac{\varepsilon \varepsilon'}{r}\left(\frac{dr}{dt}\right)^2.$$

This expression results when one puts:

(13) $D = \frac{1}{2} \frac{\varepsilon \varepsilon'}{r} \left(\frac{dr}{dt}\right)^2$

for the interaction of both single moving particles ϵ and ϵ'.

The electrostatic potential of both particles is

(14) $S = -\frac{\varepsilon \varepsilon'}{r}.$

But, it must be noted here that the quantities of electricity in equations (13) and (14) are measured according to different measures, namely, according to the magnetic one in D and according to the electrostatic one in S. If both expressions are to be combined, they must first be turned into the same kind of measurement. For example, we can introduce an electrostatic measurement into D. This occurs when we write $\epsilon\sqrt{2}/c$ and $\epsilon'\sqrt{2}/c$ instead of ϵ and ϵ' in equations (12) and (13). Magnitude c is a constant which is to be defined by experiment. After this, we will finally obtain the potential of two electrical particles:

(I) $S + D = -\frac{\varepsilon \varepsilon'}{r} \left\{ 1 - \frac{1}{c^2} \left(\frac{dr}{dt}\right)^2 \right\}.$

This expression gives us Weber's Basic Law of interaction between two electrical particles. We will deduce this law in section 97.

Section 97

Weber's Basic Law

We have assumed that the theory of the conservation of kinetic energy is valid in the interaction between electric particles. Consequently, the motion proceeds in such a way that *Lagrange's* expanded theorem (section 95) is satisfied, namely,

(1) $\delta \int_0^t (T - D + S) \, dt = 0.$

We will now take two electric particles which are concentrated in points (x, y, z) and (x_1, y_1, z_1). Their quantities of electricity will be ϵ and ϵ', and their masses m and m_1. In this case,

$$T = \frac{1}{2} \cdot m \left(x'^2 + y'^2 + z'^2\right) + \frac{1}{2} m_1 \left(x_1'^2 + y_1'^2 + z_1'^2\right),$$

$$D = \frac{1}{c^2} \frac{\varepsilon \varepsilon'}{r} \left(\frac{dr}{dt}\right)^2,$$

$$S = - \frac{\varepsilon \, \varepsilon'}{r}.$$

Therefore, we obtain

$$\delta \int_0^t T \, dt = m \int_0^t (x' \, \delta x' + y' \, \delta y' + z' \, \delta z') \, dt$$

$$+ m_1 \int_0^t (x_1' \, \delta x_1' + y_1' \, \delta y_1' + z_1' \, \delta z_1') \, dt.$$

This is the same transformation that was presented in section 39. Therefore, what results is

(2)
$$- \delta \int_0^t T \, dt = m \int_0^t \left(\frac{d^2 x}{dt^2} \, \delta x + \frac{d^2 y}{dt^2} \, \delta y + \frac{d^2 z}{dt^2} \, \delta z \right) dt$$

$$+ m_1 \int_0^t \left(\frac{d^2 x_1}{dt^2} \, \delta x_1 + \frac{d^2 y_1}{dt^2} \, \delta y_1 + \frac{d^2 z_1}{dt^2} \, \delta z_1 \right) dt.$$

Furthermore, we have

$$\delta \int_0^t S \, dt = \varepsilon \, \varepsilon' \int_0^t \frac{\delta r}{r^2} \, dt,$$

$$\delta \int_0^t (- D) \, dt = \frac{\varepsilon \, \varepsilon'}{c^2} \int_0^t \frac{\delta r}{r^2} \left(\frac{dr}{dt} \right)^2 dt - 2 \frac{\varepsilon \, \varepsilon'}{c^2} \int_0^t \frac{1}{r} \frac{dr}{dt} \frac{d \, \delta r}{dt} \, dt.$$

The last integral can still be transformed. Integration by parts results in

$$\int \frac{1}{r} \frac{dr}{dt} \frac{d \, \delta r}{dt} \, dt = \frac{1}{r} \frac{dr}{dt} \, \delta r - \int \frac{d \left(\frac{1}{r} \frac{dr}{dt} \right)}{dt} \, \delta r \, dt.$$

Due to setting up limits, the free part drops away, because $\delta r = 0$ at the beginning and at the end of the motion. Consequently, we obtain

$$\int_0^t \frac{1}{r} \frac{dr}{dt} \frac{d \, \delta r}{dt} \, dt = - \int_0^t \frac{1}{r} \frac{d^2 r}{dt^2} \, \delta r \, dt + \int_0^t \frac{1}{r^2} \left(\frac{dr}{dt} \right)^2 \delta r \, dt.$$

and, therefore,

$$\delta \int_0^t (-D + S)\, dt = \int_0^t dt\; \delta r \cdot \frac{\varepsilon\, \varepsilon'}{r^2}$$

(3)

$$\times \left\{ 1 - \frac{1}{c^2}\left(\frac{dr}{dt}\right)^2 + \frac{2r}{c^2}\frac{d^2 r}{dt^2} \right\}.$$

If we now insert components from (2) and (3) into (1), then what occurs is that *two electric particles ε and ε' exert a repulsion on each other at distance r, whose direction coincides with their connecting lines and whose magnitude is*

$$\frac{\varepsilon\, \varepsilon'}{r^2}\left\{ 1 - \frac{1}{c^2}\left(\frac{dr}{dt}\right)^2 + \frac{2r}{c^2}\frac{d^2 r}{dt^2} \right\}.$$

This is *Weber's Basic Law.*[†]

Section 98
The Potential Of Two Electric Particles. Riemann's Form

We will return to expression (5) in section 94. According to this expression

$$D_1 = -\int\int \frac{dS\, dS'}{r}\,(i_1\, i_1'' + i_2\, i_2'' + i_3\, i_3'')$$

holds for magnetic measurement, while, on the other hand,

(1) $$D_1 = -\frac{2}{c^2}\int\int \frac{dS\, dS'}{r}\,(i_1\, i_1'' + i_2\, i_2'' + i_3\, i_3'')$$

holds for electrostatic measurement. If we immediately introduce the velocities here with the aid of equations (8) in section 96 and with the aid of the three corresponding equations for the second conductor, then we obtain

(2) $$D_1 = \frac{1}{c^2}\sum\sum \frac{\varepsilon\, \varepsilon'}{r}\left\{ -2\left(\frac{dx}{dt}\frac{dx_1}{dt} + \frac{dy}{dt}\frac{dy_1}{dt} + \frac{dz}{dt}\frac{dz_1}{dt}\right)\right\}.$$

[†] *Weber.* Elektrodynamische Maassbestimmungen. Theil 1. Seite 99. (Abhandlungen der K. Sächsischen Gesellschaft der Wissenschaften zu Leipzig. 1846.)

We will want to continue to transform in such a way that only the *relative* position and the *relative* motions come into consideration. This is

$$(3) \quad \frac{1}{c^2} \sum \sum \frac{\varepsilon \, \varepsilon'}{r} \left\{ \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2 \right\} = 0.$$

Then, we can begin with the summation over the second conductor. The inner summation sign

$$\varepsilon \left\{ \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2 \right\}$$

will take precedence. For any arbitrary element of the second conductor, $1/r$ is constant. So, $1/r$ can also be taken as a factor in the summation over this element. But $\Sigma \varepsilon' = 0$ for every single element of the second conductor. Consequently, all single elements of the second conductor will furnish a contribution of zero and, therefore, the whole sum is equal to zero. In a corresponding way, we will show that

$$(4) \quad \frac{1}{c^2} \sum \sum \frac{\varepsilon \, \varepsilon'}{r} \left\{ \left(\frac{dx_1}{dt}\right)^2 + \left(\frac{dy_1}{dt}\right)^2 + \left(\frac{dz_1}{dt}\right)^2 \right\} = 0.$$

What then results from (2), (3), and (4) is

$$(5) \quad D_1 = \frac{1}{c^2} \sum \sum \frac{\varepsilon \, \varepsilon'}{r} \left\{ \left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)^2 \right\}.$$

We will, therefore, assume for two single particles,

$$(II) \quad D = \frac{1}{c^2} \frac{\varepsilon \, \varepsilon'}{r} \left\{ \left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)^2 \right\}.$$

Section 99

Riemann's Basic Law

We will also want to calculate the interaction between two electric particles with the aid of this second expression for D. As in section 97, we will start from the formula

$$(1) \quad \delta \int_0^t (T - D + S) \, dt = 0,$$

which expresses *Lagrange's* expanded theorem. So, now, we can follow once more the same path as in section 97. But, it is also permissible to immediately apply formula (6) in section 42, which reads

(2) $$\frac{d\left(\frac{\partial(T-D)}{\partial q'}\right)}{dt} = \frac{\partial(T-D+S)}{\partial q}.$$

Coordinates x, y, z, x_1, y_1, z_1 are to be inserted successively for q. We will carry out the calculation for q=x. It is

$$T = \frac{1}{2} m (x'^2 + y'^2 + z'^2) + \frac{1}{2} m_1 (x_1'^2 + y_1'^2 + z_1'^2),$$

thus,

$$\frac{\partial T}{\partial x'} = m x', \quad \frac{\partial T}{\partial x} = 0.$$

And, therefore, we now have

(3) $$X = m \frac{d^2 x}{dt^2} = \frac{d\left(\frac{\partial D}{\partial x'}\right)}{dt} - \frac{\partial D}{\partial x} + \frac{\partial S}{\partial x}.$$

But, what also results from formula (II) in section 98 is

$$\frac{\partial D}{\partial x'} = 2 \frac{\varepsilon \varepsilon'}{c^2} \frac{1}{r} \left(\frac{dx}{dt} - \frac{dx_1}{dt}\right),$$

$$\frac{\partial D}{\partial x} = - \frac{\varepsilon \varepsilon'}{c^2} \frac{1}{r^2} \frac{\partial r}{\partial x} \left\{\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)^2\right\}.$$

And, finally,

$$\frac{\partial S}{\partial x} = \frac{\varepsilon \varepsilon'}{r^2} \frac{\partial r}{\partial x}.$$

When one inserts this into equation (3), one obtains

(4) $$X = \frac{\varepsilon \varepsilon'}{r^2} \frac{\partial r}{\partial x} + \frac{\varepsilon \varepsilon'}{c^2} \frac{d\left\{\frac{2}{r}\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)\right\}}{dt}$$

$$+ \frac{\varepsilon \varepsilon'}{c^2} \frac{1}{r^2} \frac{\partial r}{\partial x} \left\{\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)^2\right\}.$$

just as

(5) $$Y = \frac{\varepsilon \varepsilon'}{r^2} \frac{\partial r}{\partial y} + \frac{\varepsilon \varepsilon'}{c^2} \frac{d\left\{\frac{2}{r}\left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)\right\}}{dt}$$

$$+ \frac{\varepsilon \varepsilon'}{c^2} \frac{1}{r^2} \frac{\partial r}{\partial y} \left\{\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)^2\right\},$$

$$(6) \quad Z = \frac{\varepsilon \, \varepsilon'}{r^2} \frac{\partial r}{\partial z} + \frac{\varepsilon \, \varepsilon'}{c^2} \frac{d\left\{ \frac{2}{r} \left(\frac{dz}{dt} - \frac{dz_1}{dt} \right) \right\}}{dt}$$

$$+ \frac{\varepsilon \, \varepsilon'}{c^2} \frac{1}{r^2} \frac{\partial r}{\partial z} \left\{ \left(\frac{dx}{dt} - \frac{dx_1}{dt} \right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt} \right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt} \right)^2 \right\}.$$

Tests have not yet been conducted successfully for moving free electricity.

Section 100

The Effect Of All The Particles ϵ On A Particle ϵ'. Riemann's Law

In order to investigate the effect of all the electric particles ϵ' on one particle ϵ, we have to put

$$(1) \quad S = \varepsilon \sum \left(-\frac{\varepsilon'}{r} \right) = \varepsilon \, V,$$

where V denotes the electrostatic potential function of particle ϵ' at point (x, y, z). Regarding D, we have to distinguish between the two hypotheses (sections 96 and 99). According to *Weber's* formula

$$(2a) \quad D = \varepsilon \sum \frac{\varepsilon'}{c^2} \frac{1}{r} \left(\frac{dr}{dt} \right)^2;$$

while, on the other hand, according to *Riemann's* formula,

$$(2b) \quad D = \varepsilon \sum \frac{\varepsilon'}{c^2} \frac{1}{r} \left\{ \left(\frac{dx}{dt} - \frac{dx_1}{dt} \right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt} \right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt} \right)^2 \right\}.$$

We want to deal first with the latter formula. If the square in (2a) is calculated, then D breaks down into three components, namely,

$$D = \varepsilon \sum \frac{\varepsilon'}{c^2} \frac{1}{r} \left\{ \left(\frac{dx}{dt} \right)^2 + \left(\frac{dy}{dt} \right)^2 + \left(\frac{dz}{dt} \right)^2 \right\}$$

$$+ \varepsilon \sum \frac{\varepsilon'}{c^2} \frac{1}{r} \left\{ \left(\frac{dx_1}{dt} \right)^2 + \left(\frac{dy_1}{dt} \right)^2 + \left(\frac{dz_1}{dt} \right)^2 \right\}$$

$$- 2 \varepsilon \sum \frac{\varepsilon'}{c^2} \frac{1}{r} \left\{ \frac{dx}{dt} \frac{dx_1}{dt} + \frac{dy}{dt} \frac{dy_1}{dt} + \frac{dz}{dt} \frac{dz_1}{dt} \right\}.$$

If we denote the velocity of particle ϵ by v, and the velocity of particle ϵ' by v', then this can be written in a shorter form:

$$D = \frac{\varepsilon}{c^2} v^2 \sum \frac{\varepsilon'}{r} + \frac{\varepsilon}{c^2} \sum \frac{\varepsilon'}{r} v'^2$$

$$- 2 \frac{\varepsilon}{c^2} \sum \frac{\varepsilon'}{r} \left\{ \frac{dx}{dt} \frac{dx_1}{dt} + \frac{dy}{dt} \frac{dy_1}{dt} + \frac{dz}{dt} \frac{dz_1}{dt} \right\}$$

$$= - \frac{\varepsilon}{c^2} v^2 V + \frac{\varepsilon}{c^2} \sum \frac{\varepsilon'}{r} v'^2 - 2 \frac{\varepsilon}{c^2} \frac{dx}{dt} \sum \frac{\varepsilon'}{r} \frac{dx_1}{dt}$$

$$- 2 \frac{\varepsilon}{c^2} \frac{dy}{dt} \sum \frac{\varepsilon'}{r} \frac{dy_1}{dt} - 2 \frac{\varepsilon}{c^2} \frac{dz}{dt} \sum \frac{\varepsilon'}{r} \frac{dz_1}{dt},$$

and we will want to set up the abbreviation

$$\sum \frac{\varepsilon'}{r} v'^2 = W, \quad \sum \frac{\varepsilon'}{r} \frac{dx_1}{dt} = u_1,$$

$$\sum \frac{\varepsilon'}{r} \frac{dy_1}{dt} = u_2, \quad \sum \frac{\varepsilon'}{r} \frac{dz_1}{dt} = u_3.$$

Then, we will have

(3) $\quad D = - \frac{\varepsilon}{c^2} v^2 V + \frac{\varepsilon}{c^2} W - 2 \frac{\varepsilon}{c^2} \left(u_1 \frac{dx}{dt} + u_2 \frac{dy}{dt} + u_3 \frac{dz}{dt} \right).$

Functions V, W, u_1, u_2, u_3 satisfy *Laplace's* equation and, consequently, D does too, to the extent that it is dependent on x,y,z:

(4) $\quad \dfrac{\partial^2 D}{\partial x^2} + \dfrac{\partial^2 D}{\partial y^2} + \dfrac{\partial^2 D}{\partial z^2} = 0.$

We still want to establish the change of V, which occurs in time element dt, so that the particles ε' are in motion and x,y,z are taken as constant. What results is

$$\frac{\partial V}{\partial t} = - \sum \varepsilon' \frac{\partial \left(\frac{1}{r} \right)}{\partial x_1} \frac{dx_1}{dt} - \sum \varepsilon' \frac{\partial \left(\frac{1}{r} \right)}{\partial y_1} \frac{dy_1}{dt} - \sum \varepsilon' \frac{\partial \left(\frac{1}{r} \right)}{\partial z_1} \frac{dz_1}{dt}.$$

Now, however, we have

$$\frac{\partial \left(\frac{1}{r} \right)}{\partial x_1} = - \frac{\partial \left(\frac{1}{r} \right)}{\partial x},$$

consequently,

$$- \sum \varepsilon' \frac{\partial \left(\frac{1}{r} \right)}{\partial x_1} \frac{dx_1}{dt} = \sum \varepsilon' \frac{\partial \left(\frac{1}{r} \right)}{\partial x} \frac{dx_1}{dt} = \frac{\partial u_1}{\partial x},$$

just as

$$-\sum \varepsilon' \frac{\partial\left(\frac{1}{r}\right)}{\partial y_1} \frac{dy_1}{dt} = \sum \varepsilon' \frac{\partial\left(\frac{1}{r}\right)}{\partial y} \frac{dy_1}{dt} = \frac{\partial u_2}{\partial y},$$

$$-\sum \varepsilon' \frac{\partial\left(\frac{1}{r}\right)}{\partial z_1} \frac{dz_1}{dt} = \sum \varepsilon' \frac{\partial\left(\frac{1}{r}\right)}{\partial z} \frac{dz_1}{dt} = \frac{\partial u_3}{\partial z}.$$

Therefore the expression for $\partial V/\partial t$ changes into the following:

(5) $\quad \dfrac{\partial V}{\partial t} = \dfrac{\partial u_1}{\partial x} + \dfrac{\partial u_2}{\partial y} + \dfrac{\partial u_3}{\partial z}.$

On the basis of this differential equation, one can make an assumption about the meaning of functions V, u_1, u_2, u_3. One can assume that the electric effect is mediated through an aether. By virtue of equation (5), V can then be considered the density and u_1, u_2, u_3 the current intensities of this aether.

Section 101

Continuation: Weber's Law

We will also want to establish the potential for the effect of all the particles ϵ' on one particle ϵ according to *Weber's* theory.

First of all, we again have

(1) $\quad S = \epsilon V.$

This function satisfies *Laplace's* equation. As an abbreviation, the sum of the three derivatives can be used for any function F:

$$\frac{\partial^2 F}{\partial x^2} + \frac{\partial^2 F}{\partial y^2} + \frac{\partial^2 F}{\partial z^2} = \Delta_2 F.$$

Through this designation, we obtain

(2) $\quad \Delta_2 S = 0.$

Function D can now be taken out of equation (2a) in section 100. But, since $r^2 = (x-x_1)^2 + (y-y_1)^2 + (z-z_1)^2$, consequently,

$$\frac{dr}{dt} = \frac{(x-x_1)}{r}\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right) + \frac{(y-y_1)}{r}\left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)$$

$$+ \frac{(z-z_1)}{r}\left(\frac{dz}{dt} - \frac{dz_1}{dt}\right).$$

We will insert this into the expression for D, getting

$$(3) \quad D=\frac{\varepsilon}{c^2}\sum\frac{\varepsilon'}{r^3}\left\{(x-x_1)\left(\frac{dx}{dt}-\frac{dx_1}{dt}\right)+(y-y_1)\left(\frac{dy}{dt}-\frac{dy_1}{dt}\right)\right.$$
$$\left.+(z-\dot{z}_1)\left(\frac{dz}{dt}-\frac{dz_1}{dt}\right)\right\}^2.$$

To the extent that this function is dependent on x,y,z, it does not satisfy *Laplace's* equation, but the complicated differential equation

$$(4) \quad \Delta_2\Delta_2 D = 0.$$

In order to prove this, we have

$$\frac{1}{r^3}\left\{(x-x_1)\left(\frac{dx}{dt}-\frac{dx_1}{dt}\right)+(y-y_1)\left(\frac{dy}{dt}-\frac{dy_1}{dt}\right)\right.$$
$$\left.+(z-z_1)\left(\frac{dz}{dt}-\frac{dz_1}{dt}\right)\right\}=G,$$

$$(x-x_1)\left(\frac{dx}{dt}-\frac{dx_1}{dt}\right)+(y-y_2)\left(\frac{dy}{dt}-\frac{dy_1}{dt}\right)$$
$$+(z-z_1)\left(\frac{dz}{dt}-\frac{dz_1}{dt}\right)=H.$$

The single summands in D, then, regardless of constant factors, are of the form G·H. But,

$$\Delta_2(GH)=G\Delta_2 H+H\Delta_2 G$$
$$+2\left(\frac{\partial G}{\partial x}\frac{\partial H}{\partial x}+\frac{\partial G}{\partial y}\frac{\partial H}{\partial y}+\frac{\partial G}{\partial z}\frac{\partial H}{\partial z}\right),$$

and it can easily be proven through differentiation that $\Delta_2 G=0$, $\Delta_2 H=0$. Consequently, we will obtain the simpler equation

$$\Delta_2(GH)=2\left(\frac{\partial G}{\partial x}\frac{\partial H}{\partial x}+\frac{\partial G}{\partial y}\frac{\partial H}{\partial y}+\frac{\partial G}{\partial z}\frac{\partial H}{\partial z}\right).$$

Factors

$$\frac{\partial H}{\partial x},\ \frac{\partial H}{\partial y},\ \frac{\partial H}{\partial z}$$

will be independent of x,y,z. So,

$$\Delta_2\Delta_2(GH)=2\left(\frac{\partial H}{\partial x}\frac{\partial\Delta_2 G}{\partial x}+\frac{\partial H}{\partial y}\frac{\partial\Delta_2 G}{\partial y}+\frac{\partial H}{\partial z}\frac{\partial\Delta_2 G}{\partial z}\right)$$

and this is equal to zero because $\Delta_2 G=0$. This also proves equation (4).

Thus, *Weber's* hypothesis, in the case of the problem at hand, leads to a more complicated differential equation.

Section 102
The Motion Of Particle ϵ.
Riemann's Law

We now want to deduce the equations of motion for particle ϵ. First, according to *Riemann's* hypothesis

(1) $\quad S = \epsilon\, V.$

(2) $\quad D = -\dfrac{\epsilon}{c^2}\, v^2\, V + \dfrac{\epsilon}{c^2}\, W - 2\,\dfrac{\epsilon}{c^2}\left(u_1\,\dfrac{dx}{dt} + u_2\,\dfrac{dy}{dt} + u_3\,\dfrac{dz}{dt}\right).$

Lagrange's expanded theorem will be valid for the motion and what results from it is like what resulted in section 90, equation (2):

$$\frac{d\left(\dfrac{\partial\,(T-D)}{\partial q'}\right)}{dt} = \frac{\partial\,(T-D+S)}{\partial q}.$$

Coordinates x, y, z are to be successively inserted here for q. We will obtain results for $q = x$ in the same manner as in section 99, equation (3):

(3) $\quad m\,\dfrac{d^2x}{dt^2} = \dfrac{d\left(\dfrac{\partial D}{\partial x'}\right)}{dt} - \dfrac{\partial D}{\partial x} + \dfrac{\partial S}{\partial x}.$

The partial derivatives $\partial D/\partial x$ and $\partial S/\partial x$ which are taken with respect to x are independent of the acceleration. But, of course, acceleration occurs in

$$\frac{d\left(\dfrac{\partial D}{\partial x'}\right)}{dt}.$$

Namely,

$$\frac{\partial D}{\partial x'} = -2\,\frac{\epsilon}{c^2}\, V\frac{dx}{dt} - 2\,\frac{\epsilon}{c^2}\, u_1.$$

Consequently,

$$\frac{d\left(\dfrac{\partial D}{\partial x'}\right)}{dt} = -2\,\frac{\epsilon}{c^2}\, V\,\frac{d^2x}{dt^2} - 2\,\frac{\epsilon}{c^2}\,\frac{dV}{dt}\,\frac{dx}{dt} - 2\,\frac{\epsilon}{c^2}\,\frac{du_1}{dt},$$

or, even more briefly,

$$\frac{d\left(\frac{\partial D}{\partial x'}\right)}{dt} = -2\,\frac{\varepsilon}{c^2}\,V\,\frac{d^2x}{dt^2} + \frac{\delta\left(\frac{\partial D}{\partial x'}\right)}{dt},$$

when one indicates by δ a differentiation by t, in which dx/dt is considered as constant. If one introduces this into equation (3), then the result is

$$(4) \quad \left(m + \frac{2\,\varepsilon}{c^2}\,V\right)\frac{d^2x}{dt^2} = \frac{\delta\left(\frac{\partial D}{\partial x'}\right)}{dt} - \frac{\partial D}{\partial x} + \frac{\partial S}{\partial x}.$$

We will obtain both of the other equations in the same way:

$$(5) \quad \left(m + \frac{2\,\varepsilon}{c^2}\,V\right)\frac{d^2y}{dt^2} = \frac{\delta\left(\frac{\partial D}{\partial y'}\right)}{dt} - \frac{\partial D}{\partial y} + \frac{\partial S}{\partial y},$$

$$(6) \quad \left(m + \frac{2\,\varepsilon}{c^2}\,V\right)\frac{d^2z}{dt^2} = \frac{\delta\left(\frac{\partial D}{\partial z'}\right)}{dt} - \frac{\partial D}{\partial z} + \frac{\partial S}{\partial z}.$$

Section 103

Continuation: Weber's Law

The equations of motion for *electric* particle ϵ will be finally derived from *Weber's* formula too:

$$(1) \quad S = \varepsilon\,V,$$

$$(2) \quad D = \frac{\varepsilon}{c^2}\sum\frac{\varepsilon'}{r^3}\left\{(x-x_1)\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right) + (y-y_1)\left(\frac{dy}{dt} - \frac{dy_1}{dt}\right) \right.$$
$$\left. + (z-z_1)\left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)\right\}^2.$$

This results in

$$\frac{\partial D}{\partial x'} = a\,\frac{dx}{dt} + b\,\frac{dy}{dt} + c\,\frac{dz}{dt} + k,$$

in which a, b, c, k are functions of x,y,z that satisfy the partial differential equation $\Delta_2\Delta_2 F = 0$. By means of differentiation by t we obtain

$$\frac{d\left(\frac{\partial D}{\partial x'}\right)}{dt} = a\,\frac{d^2x}{dt^2} + b\,\frac{d^2y}{dt^2} + c\,\frac{d^2z}{dt^2} + g,$$

and function g here is dependent only on coordinates x,y,z and on velocities dx/dt, dy/dt, dz/dt. According to this, the equations for motion read

$$(m-a)\frac{d^2x}{dt^2} - b\frac{d^2y}{dt^2} - c\frac{d^2z}{dt^2} = g - \frac{\partial D}{\partial x} + \frac{\partial S}{\partial x},$$

$$(3)\quad -a_1\frac{d^2x}{dt^2} + (m-b_1)\frac{d^2y}{dt^2} - c_1\frac{d^2z}{dt^2} = g_1 - \frac{\partial D}{\partial y} + \frac{\partial S}{\partial y},$$

$$-a_2\frac{d^2x}{dt^2} - b_2\frac{d^2y}{dt^2} + (m-c_2)\frac{d^2z}{dt^2} = g_2 - \frac{\partial D}{\partial z} + \frac{\partial S}{\partial z}.$$

So, elimination must take place here first.

Section 104
The Connection With Ampère's Law

Expression (12) in section 96 has been interpreted by us in such a way that the portion of the total potential of two closed currents acting on each other that is dependent on velocities is composed by means of summation of only single potentials. The single potential is generally based on two electric particles ϵ and ϵ'. So, if the question is about the potential D_1 of two currents acting on one another, then one has to combine every particle ϵ of one current with every particle ϵ' of the other current, form the single potential for every such combination, and then sum up all the single potentials. This is how the expression for D_1 in equation (13) in section 96 correctly resulted from equation (12) in the same section and the same holds for the expression for D_1 in equation (5) in section 98, which came from expression (II) in the same section.

If one then uses either

$$(1)\quad S + D = -\frac{\epsilon\,\epsilon'}{r}\left\{1 - \frac{1}{c^2}\left(\frac{dr}{dt}\right)^2\right\}$$

from *Weber's* fundamental law, or

$$S + D = -\frac{\epsilon\,\epsilon'}{r} + \frac{\epsilon\,\epsilon'}{c^2\,r}\left\{\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)^2\right.$$

$$(2)$$

$$\left. + \left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)^2\right\}$$

from *Riemann's* fundamental law to calculate the total interaction of all electric particles that are generally contained in two closed conductors at

rest and in current, then, for every combination of two different particles ϵ and ϵ', one has to set up expressions (1) and (2), respectively, and sum them.

We must distinguish between three different combinations here, namely, two particles at rest, one particle at rest and one moving, and, finally, two moving particles.

We will want to consider the special case of two closed constant currents in order to investigate whether *Weber's* basic law, or *Riemann's* basic law, repectively, is in agreement with *Ampère's* law. The question in *Ampère's* law concerns the electrodynamic interaction between two current elements of which one will belong to the first current, while the other will belong to the second current. So, what comes into consideration here is only the interaction between the *moving* electric particles of both constant currents.

It can first of all be proved that the contribution to the total potential of the moving electric particles that orignates in S is equal to zero. For, we can bring all the other ϵ' particles into combination beginning with a single ϵ particle. Then, ϵ will leave the sum sign and the summation of $\Sigma(\epsilon'/r)$ will extend over all of the particles ϵ' different from ϵ. If first we undertake summation over a current element so that $1/r$ can also be placed before the sum sign, then $\Sigma\epsilon' = 0$ in every current element for constant current. Thus, all contributions to the sum being formed are zero. This holds for the combination of every single particle ϵ with the particles ϵ' that are different from it. Consequently,

(3) $-\sum \dfrac{\epsilon\,\epsilon'}{r} = 0.$

So, only the sum of all the values for D is left for the combinations of each of two moving particles. These combinations will break down into three groups:

First: each single particle of the first current with a single particle of the second current;

Second: two particles each of the first current;

Third: two particles each of the second current.

These groups will successively furnish the potentials which are denoted in section 96 as D_1 D_2 D_3.

D_2 and D_3 are constant for constant currents. If we proceed from (1), then

(4) $D_2 = \dfrac{1}{c^2} \sum \dfrac{\epsilon\,\epsilon'}{r} \left(\dfrac{dr}{dt}\right)^2,$

when the summation is extended over all the combinations of the particles of the first current.

Because the conductor is assumed to have an invariant form, we can then base a *fixed, connected* coordinate system x,y,z on the same conductor. Then, given a *constant* current, function

$$\frac{\varepsilon\,\varepsilon'}{r}\left(\frac{dr}{dt}\right)^2$$

is only dependent on x,y,z on the one hand and on x_1, y_1, z_1 on the other hand. If one next takes a single ϵ and then sums for all ϵ', then the sum is uniquely and solely a function of x,y,z, i.e., of the coordinates of every particle ϵ. But, if one forms these sums for every value-combination x,y,z that belongs to points in the interior of the conductor and then combines all of these sums together through addition, the result is constant.

The same holds for sum

$$(5)\quad D_2 = \frac{1}{c^2}\sum\frac{\varepsilon\,\varepsilon'}{r}\left\{\left(\frac{dx}{dt}-\frac{dx_1}{dt}\right)^2+\left(\frac{dy}{dt}-\frac{dy_1}{dt}\right)^2+\left(\frac{dz}{dt}-\frac{dz_1}{dt}\right)^2\right\},$$

when it is extended over all combinations of particles of the first current.

We can prove that D_3 is constant with constant currents in the same way.

Thus, with constant currents, the total work performed by the moving electric particles is equal to the change in D_1 alone. So, according to this, it turns out that *Weber* and *Riemann's* basic laws are in agreement with *Ampère's* because *Ampère's* law is on constant currents. In his observations, *Ampère* watched for the equilibrium position of moving current conductors which have constant current flowing through them. It was from these observations that he abstracted his law. Because we have now deduced *Ampère's* law from D_1 and we have been able to produce the expression for D_1 from *Weber's* and also from *Riemann's* basic laws, then their complete agreement has been proven in fact.†

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† *Kirchhoff* published two treatises on the movement of electricity in wire-shaped and arbitrary conductors, both in *Poggendorff's* Annalen, Bd. 100 (S.193) and Bd. 102 (S.529). Here, the electromotive force is viewed as originating in the free electricity which is present and in the induction which occurs as a consequence of the changes in the current intensity in all segments of the conductor. As a result of this, *Kirchhoff* gets currents in which it is only as an exception that the density of the free electricity in the interior of the conductor is equal to zero. These investigations by *Kirchhoff* form the starting point for the developments reported by *Weingarten* and *Lorberg*. (*Weingarten*. Ueber die Bewegung der Elektricität in Leitern. *Borchardt's* Journal. Bd. 63 — *Lorberg*. Zur Theorie der Bewegung der Elektricitat in nicht linearen Leitern. *Borchardt's* Journal Bd. 71. S. 53.)

In 1858, *Riemann* presented a treatise to the Royal Society of Sciences at Göttingen, but later withdrew it. This same treatise is printed in *Poggendorf's* Annalen, Bd. 131 (S. 237) under the title, "Ein Betrag zur Elektrodynamik." This states the hypothesis that the force present in an electric particle at time t first begins its effect at a finite distance on another such paritcle at a later time $t + \Delta t$. We can also find this fundamental idea in a contemporary (1867) treatise published by *L. Lorenz:* Ueber die Identität der Schwingungen des Lichts mit den elektrischen Strömen, (*Poggendorff's* Annalen Bd. 131. S. 243). *C. Neumann* dealt with the same fundamental ideas further (Die Principien der Elektrodynamik. Tübingen 1868. Gratulationsschrift. — Allgemeine Betrachtungen über das *Weber*'sche Gesetz. Mathematische Annalen Bd. 8. 1875.)

In recent years, *Weber's* basic law has become the subject of a controversy incited by *Helmholtz.* One should see these treatises about it:

Helmholtz: Ueber die Bewegungsgleichungen für ruhende leitende Korper. (*Borchardt's* Journal Bd.72) — Ueber die Theorie der Elektrodynamik. (*Borchardt's* Journal Bd. 75 and Bd. 78.)

Weber: Elektrodynamische Maassbestimmungen, insbesondere über das Princip der Erhaltung der Energie. (Abhandlungen der mathematische-physischen Klasse der K. Sächsischen Gesellschaft der Wissenschaften Bd. 10)

C. Neumann: Ueber die den Kräften elektrodynamischen Ursprungs zuzuschreibenden Elementargesetze. (Ibid., Bd. 10)

Also: *C. Neuman's* papers in volumes 5 and 6 of the *Mathematischen Annalen* and his monograph: Die elektrischen Kräfte. Theil 1. Leipzig 1873.

Both of *H. Weber's* papers are of particular interest for the mathematician: Ueber die *Bessel*'schen Functionen und ihre Anwendung auf die Theorie der elektrischen. Ströme. (*Borchardt's* Journal Bd. 75) — Ueber die stationären Strömungen der Elektricität in Cylindern. (*Borchardt's* Journal Bd. 76)

· These texts deserve mention:

Beer: Einleitung in die Elektrostatik, die Lehre vom Magnetismus und die Elektrodynamik. Braunschweig 1865.

Wiedemann: Die Lehre vom Galvanismus und Elektromagnetismus. Second edition. Bd. I, II, 1 and 2. Braunschweig 1872. 1873. 1874.

Maxwell: A treatise on electricity and magnetism. Vol. I.II. Oxford 1873.

One will find a detailed overview of the literature in *Wiedemann's* book.

The Equivalence of Ampère's Electrodynamic Law and that of Biot and Savart

by R. C. LYNESS

Ampère's Law[1] for the force one current element, i in ds, exerts on another, i' in ds', is given by

$$\mathbf{A}_{ds,\ ds'} = \hat{r}\, \frac{ii'dsds'}{r^2}\, (2 \cos e - 3 \cos \phi \cos \phi'),$$

where $\hat{r}$ is the unit vector in the direction from P' at ds' to P at ds, e the angle between $\mathbf{ds}$ and $\mathbf{ds'}$ and ϕ, ϕ' the angles $\mathbf{ds}$ and $\mathbf{ds'}$ make with $\overline{P'P} = \mathbf{r}$. It is evident that $\mathbf{A}_{ds,\ ds'} = -\mathbf{A}_{ds',\ ds,}$ and Newton's Third Law is obeyed.

The Law of Biot and Savart is sometimes given thus:

the field at P' caused by i in ds at P is $i\,(\mathbf{ds} \times \mathbf{r})/r^3$ and the force on i' in ds' at P' is $\mathbf{B}_{ds,\ ds'} = ii'\ \mathbf{ds'} \times (\mathbf{ds} \times \mathbf{r})/r^3$ where $\mathbf{r} = \overline{PP'}$.

Now vector multiplication is not associative and $\mathbf{ds} \times (\mathbf{ds'} \times \mathbf{r})$ is not in general equal to $-\mathbf{ds'} \times (\mathbf{ds} \times \mathbf{r})$. So $\mathbf{B}_{ds,\ ds'}$ does not equal $-\mathbf{B}_{ds',\ ds,}$ and Newton's Third Law is not obeyed. It is, however, impossible to measure experimentally the force exerted by one current-element on another current-element and so nothing is lost by putting Biot–Savart in the form[2]:

the field at P' caused by a circuit C carrying current i is

$$i \int_C \frac{\mathbf{ds} \times \mathbf{r}}{r^3}$$

and the force exerted on i' in ds' at P' is $\mathbf{B}_{C,\ ds'} = ii'\ \mathbf{ds'} \times \int_C \frac{\mathbf{ds} \times \mathbf{r}}{r^3}.$

This can be verified experimentally be measuring the force the circuit C exerts on an element ds' by approximating to the element by a short wire whose ends are free to move in mercury cups.

Again, if we use the result for $\mathbf{B}_{ds,\ ds'}$ we can show that $\mathbf{B}_{ds',\ C} \neq -\mathbf{B}_{C,\ ds'}$, i.e. Newton's Third Law does not hold between a complete circuit and an *element*[3].

However, it is impossible to measure experimentally the force exerted by an element on a circuit and Biot–Savart can escape the charge of disobeying Newton's Third Law by denying that a current-element *can* produce a field and stating the Law in the form that gives the field caused by a *circuit*. The field can be thought of *as if it were* the vector sum of the ' fields ' caused by the separate current-elements which compose the circuit, each current-element producing at P' a ' field ' $i\,(\mathbf{ds} \times \mathbf{r})/r^3$ and a ' force ' on i' in ds' at P' of

$$ii'\ \mathbf{ds'} \times (\mathbf{ds} \times \mathbf{r})/r^3.$$

We can think in this way because the distributive law $\mathbf{a} \times \Sigma\, \mathbf{u}_s = \Sigma\, (\mathbf{a} \times \mathbf{u}_s)$ holds for vector multiplication over addition.

We now show that in the only case where experiment might discriminate between Ampère and Biot–Savart, no discrimination is possible, for

$$\mathbf{A}_{C,\ ds'} = \mathbf{B}_{C,\ ds'}$$

This result [4] implies, of course, that $A_{C, C'} = B_{C, C'}$ and, as from first principles $A_{C', C} = -A_{C, C'}$ it follows that $B_{C', C} = -B_{C, C'}$.
In other words Biot–Savart gives action and reaction between *two circuits* in accordance with Newton's Third Law.
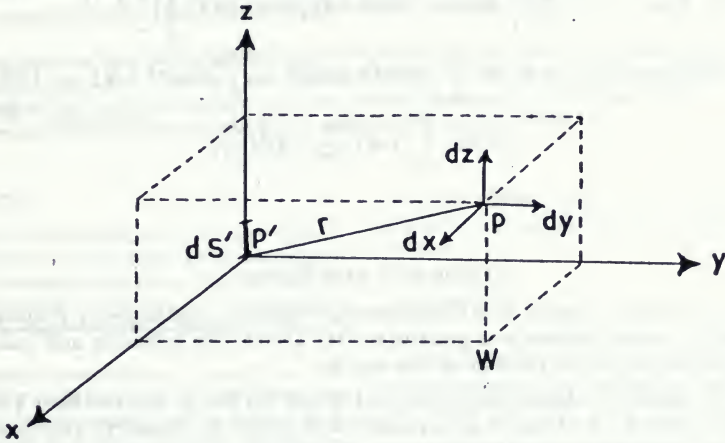
1. For $A_{ds, ds'}$:
$$A_{ds, ds'} = \hat{r}. \ ii' \ ds \ ds' \ (2 \cos e - 3 \overset{\cos\phi}{\cos} \phi')/r^2.$$

Take the origin at P', P'z in the direction ds' and P at (x, y, z) Fig. 1. Then

$$\cos e = \frac{dz}{ds}, \ \cos \phi = \frac{dr}{ds} \text{ and } \cos \phi' = \frac{z}{r}.$$

The resolved part of $A_{ds, ds'}$ in the direction $\hat{z}$ is

$$\hat{z} \frac{ii' \ ds' \ z}{r^3} (2 \ dz - 3 \frac{z}{r} dr) = \hat{z} \ ii' \ ds' \ d \left(\frac{z^2}{r^3}\right)$$

This vanishes when integrated round a complete circuit C[5].

The resolved part in the direction $\hat{x}$ is $\hat{x} \frac{ii' \ ds' \ x}{r^3} (2 \ dz - 3 \frac{z}{r} dr)$.

2. For $B_{ds, ds'}$:
$$B_{ds, ds'} = ii' \ ds' \times (ds \times r)/r^3.$$

Because the distributive law holds we can replace $\vec{ds}$ by $\vec{dx} + \vec{dy} + \vec{dz}$. The force on i' ds' at P' exerted by i dz at P is

$$\frac{i'i \ ds' \ dz}{r^2} \frac{\overline{P'W}}{r} = \frac{ii' \ ds' \ dz}{r^3} (\hat{x}x + \hat{y} y).$$

The force on i' ds' at P' exerted by i dx at P is $- \frac{ii' \ ds' \ dx}{r^3} (\hat{x}z)$, and

Equivalence of Ampère's Electrodynamic Law and that of Biot and Savart 455

Hence $\mathbf{B}_{ds,\ ds'} = \dfrac{ii'\ ds'}{r^3}\left[\hat{\mathbf{x}}(xdz - zdx) + \hat{\mathbf{y}}(ydz - zdy)\right]\left(-1\right)$

$\mathbf{A}_{C,\ ds'} - \mathbf{B}_{C,\ ds'} = ii'\ ds' \displaystyle\int_C \hat{\mathbf{x}}\,(xdz + zdx - \dfrac{3xz}{r}dr)/r^3 + \hat{\mathbf{y}}\,(\ .....)/r^3$

$\qquad\qquad\qquad ydz + zdy\ -\ \dfrac{3yz}{r}\,dr$

$\qquad\qquad = ii'\ ds' \displaystyle\int_C \hat{\mathbf{x}}\,d\left(\dfrac{xz}{r^3}\right) + \hat{\mathbf{y}}\,d\left(\dfrac{yz}{r^3}\right)$

$\qquad\qquad = 0.$

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1. R. A. R. TRICKER, *Ampère as a Contemporary Physicist.* Contemporary Physics, Vol. 3, Number 6. I am indebted and grateful to Dr. Tricker for initiating and maintaining my interest in the subject matter of this article.

2. See C. A. COULSON—*Electricity* (Oliver and Boyd) 5th Ed. p. 104 and later § 62 p. 121 where it is stated ' the force on a tiny element *ds* cannot be measured experimentally '. While this is strictly true if the element is tiny enough, it is more interesting here that the force *exerted by* a tiny element *ds* cannot be measured experimentally.

3. $\mathbf{B}_{ds',\ C}$ is often shown equal to $ii'\ ds' \displaystyle\int_C \hat{\mathbf{r}}\,\dfrac{\cos e\ ds}{r^2}$ by replacing

   $\mathbf{B}_{ds',\ ds}$ by an equivalent sum of two forces one along $\hat{\mathbf{r}}$ and the other parallel to $\mathbf{ds'}$. The integral around C of the latter vanishes.

4. A special case with C a straight line and *ds'* parallel to it is proved in the course of an article by W. G. V. ROSSER—*Contemporary Physics Vol. 3*, Number 1, on The Biot–Savart Law.

5. This is hardly surprising for Ampère obtained his force in the form
   $$(ii'\ ds\ ds'/r^2)\ (\cos e + K\cos\phi\cos\phi')$$
   using units of current that were $1/\sqrt{2}$ times ours. His experiments had shown that a current-element in the form of a circular arc whose ends were in mercury cups and which was constrained to rotate in its plane about its centre, did not in fact move however he varied the field. This caused him to make $k = 3/2$. See *Mem. de l'Acad* 6, 175 published in 1825.

Detection of a force between a charged metal foil and a current-carrying conductor

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An experiment is described which demonstrates a force between static charges on a metal foil and a large steady electric current in a wire. The observed force vectors cannot be readily explained in terms of diamagnetism, paramagnetism, eddy currents, thermal convection currents, or the Poynting electric field associated with a dc current outside the path of the current.

INTRODUCTION

Early attempts to explain the magnetic force due to current-carrying wires in terms of electrostatic forces culminated in Maxwell's electromagnetic field theory. However, the two fundamental forces remained separate, yet it was established that they had a common speed of propagation through space.

Technologies initiated by Maxwell's theory have occupied scientists for more than a century. Interest in the underlying connection between magnetic and electrostatic forces waned. Could there be another relation between the two forces which is not implied by Maxwell's theory? Speculation along these lines led to the experiment described in this paper. It revealed the existence of a mechanical interaction between static electric charges on a metal foil and a steady electric current in a metallic conductor. The experiment

proved unsuitable for measuring the magnitude of the interaction force, but gave a threshold value which it must have exceeded.

I. EXPERIMENT

Figure 1 is a simplified diagram of the apparatus used by the author at MIT in which (1) was a 14-cm-long thin torque bar of balsa wood, suspended at its midpoint on a 72-cm-long AWG 40 copper wire (2) inside a glass-topped, cubic wooden enclosure (3) 2 ft on a side. The inside surfaces of the wooden box were covered with aluminum foil. The suspension wire was connected to an adjustable, regulated ± 3-kV high-voltage supply (4) and a 2×2-cm silver foil (5) mounted with its face vertically at one end of the balsa torque bar. The second terminal was connected to a U-shaped current conductor (6) and a laboratory ground.

The glass top (7) consisted of two plates with a narrow gap between them through which the suspension wire passed. On this top stood a 41-cm-high cardboard tube (8). The suspension wire attached to a piece of cardboard placed over the upper opening of the tube. The wire fell through the tube, the slot between the glass plates and down to the center of the cubic enclosure which prevented air drafts from blowing against the silver vane. The 0.95-cm-diameter U-shaped copper conductor passed through the holes in the enclosure wall. The U conductor had 50-cm-long legs spaced 10 cm apart. It was held in a horizontal plane. The silver foil was arranged to face the cross bar of the U-shaped conductor with a clearance of approximately 3.5 cm. Heavy leads from a ± 1000-A, 8-V, adjustable, regulated dc current supply were connected to the two ends of the U conductor.

If f is a force normal to the plane of the silver foil and l is the effective length of the balsa torque bar, then the twist torque experienced by the suspension wire is

$$T = f(l/2). \tag{1}$$

The wire parameters which determine the angular deflection θ were:

wire length = $L = 0.72$ m,

wire radius = $r = 3.94 \times 10^{-5}$ m,

moment of inertia of wire section = $I = \pi r^4 / 2 = 3.79 \times 10^{-18}$ m^4,

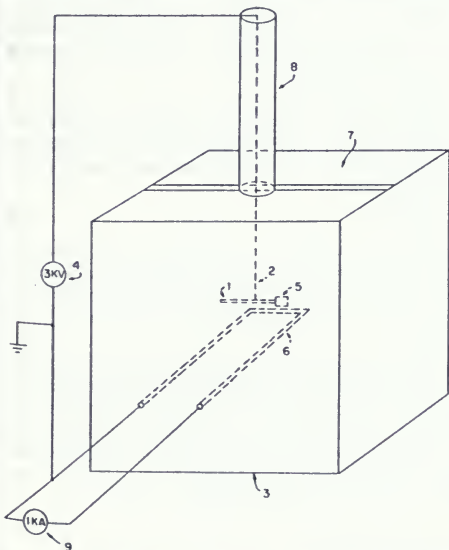torsion modulus of copper = $R = 4.3 \times 10^{10}$ N/m^2.

FIG. 1. Experimental setup: (1) torque bar, (2) suspension wire, (3) glass-topped wooden box, (4) ± 3-kV voltage supply, (5) silver foil, (6) U-shaped current conductor, (7) glass top plates, (8) cardboard tube, (9) current supply.
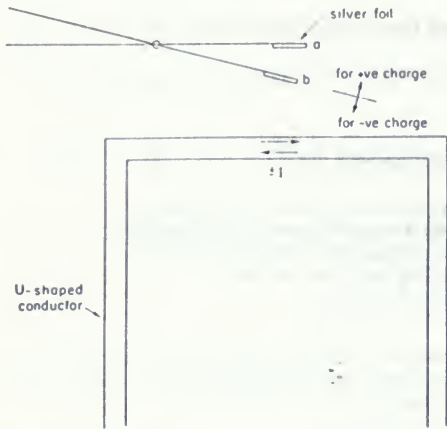
FIG. 2. Observed vane deflections.

The angular deflection of the suspension wire is related to the torque by

$$T = (RI/L)\theta. \tag{2}$$

Hence, the force on the vane per unit angle of deflection is

$$f/\theta = (2RI/lL)\text{N/rad.} \tag{3}$$

Substituting numerical values into Eq. (3) gives

$$f/\theta = 3.23 \times 10^{-6} \text{ N/rad} = 5.64 \times 10^{-8} \text{ N/deg.}$$

Deflections of less than ± 3 degrees of the torque bar could not be reliably resolved in background fluctuations due to floor-borne vibrations. Therefore, forces smaller than

$$f_{min} = 1.7 \times 10^{-7} \text{ N}$$

could not be detected.

Figure 2 illustrates the important experimental result. Initially the torque arm was arranged with the silver vane in position (a). Then a potential difference of the order of 1000 V was applied between the vane and the U-shaped conductor. This produced a rotation of the balsa torque arm which was consistent with the silver foil being attracted to the conductor, as an electrode of a capacitor should be. This attraction was independent of the polarity of the vane.

With the vane in this equilibrium position (b), the current through the U-shaped conductor was raised to 900 A. When the vane was charged positively, the effect of the current was to repel the vane from the equilibrium position. This repulsion was observed for both directions of the current. However, when the charge on the silver vane was negative, the vane was attracted to the current, regardless of the direction of the current. After reducing the current to zero and discharging the vane, the latter returned to the original position (a) of Fig. 2.

The magnitude of the deflection of the torque arm due to the flow of current was variable and showed signs of insta-

bility. The explanation of this behavior is as follows. Any deviation of the vane from the equilibrium position (b) of Fig. 2 will change the electrostatic force between the capacitor electrodes. In both cases, the attraction and the repulsion of the vane produced by the current, the change in the electrostatic force adds to the effect of the current and, therefore, leads to instability.

Nevertheless, it can be said with certainty that the force due to the current was greater than 1.7×10^{-7} N.

II. DISCUSSION OF POSSIBLE DISTURBING FORCES

Some silver alloys are diamagnetic while others are paramagnetic. In either case the silver vane should have experienced a small force while finding itself in the magnetic field of the U-shaped conductor. Paramagnets move toward regions of stronger magnetic field while diamagnets seek regions of weaker field strength. The reversal of the trapped charge on the vane should not influence the magnetic force. Consequently, the observed reversal of force with charge polarity rules out interference from magnetic forces. This also applies to the possible ferromagnetic contamination of the vane.

Eddy currents induced in the vane by the rate of change of current in the U conductor should invariably result in repulsion between the inducing and the induced current. Therefore, induced eddy currents could not explain the attraction of the vane to the U conductor when the vane is charged negatively.

The 900 A of current heats the copper rod and quite rapidly raises its temperature. This may give rise to air convection currents in the enclosure which could move the vane, but the air convection currents would not change direction when the polarity of the vane is reversed, and so this effect may also be ruled out as the cause of the observed attraction or repulsion of the vane.

Much thought has been given to the electric field that is supposed to exist outside a dc conductor and which, by virtue of the Poynting vector, is assumed to transport energy to the conductor for conversion into Joule heat. Inside the conductor this field is

$$\mathbf{E} = \rho \mathbf{J}, \tag{4}$$

where ρ is the electrical resistivity of the conductor and J the current density vector. For 900 A flowing through the U-shaped conductor, the field inside the copper comes to 0.214 V/m. This is the greatest value E can assume outside the conductor.

Furthermore, it has been estimated that the upper limit of the capacitance between foil and U-shaped conductor is 0.5 pF. For a potential difference of 1000 V across this capacitance, the charge on the foil is 0.5×10^{-9} C. Hence, the maximum force exerted by this mechanism on the foil would be 10^{-10} N. This lies several orders of magnitude below the detection threshold of the apparatus.

III. CONCLUSION

Since no other disturbing forces have been suggested, it has to be assumed that the observed attraction/repulsion

effect is an electrodynamic interaction of static charges on a metal foil with current elements in the conductor. In the experiment described here the interaction force was greater than 1.7×10^{-7} N.

It appears the perfect electrostatic screening of metal ions in a copper conductor by the conduction electrons is somehow upset by the flow of an electric current. The screening deficiency makes the conductor appear to possess a net positive charge.

ACKNOWLEDGMENTS

AMPERE'S CARDINAL LAW IN EXPLAINING VIOLENT WATER ARC EXPLOSIONS VERSUS MAGNETIC POTENTIAL

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The alpha-torque hypothesis of Peter Graneau was presented in the June 1989 issue of E&WW. This hypothesis was purported to explain a series of effects unexplained by classical electrodynamics, i.e., violent water arc explosions. Graneau's alpha-torque hypothesis is an extension of the old concept of Neumann potential, based on Ampere's cardinal law for electrodynamic forces between currents. The cardinal law for two currents I_1, I_2, can be written using vector notation:

$$F_{12} = -kr_{12}(I_1 I_2/r_{12}^3)[2ds_1 \cdot ds_2 - (3/r_{12}^2)(ds_1 \cdot r_{12})(ds_2 \cdot r_{12})] \quad 1,$$

where r_1, r_2 being the vector radii to I_1, I_2; $r_{12} = r_1 - r_2$; F_{12} being the force on I_1 by I_2; and k being a coefficient depending on the units([2]).

Graneau assigns to this law a "potential" function which in MKSA units is given as:

$$\Delta P_{n,n} = -(\mu_0/4\pi)I_n I_n(dmdn/r_{n,n})[(0.5\cos\varepsilon - 1.5\cos(2+\varepsilon)] \quad 2.$$

Graneau assumes that the principle of virtual work is valid for this "potential". However, by differentiating with respect to all the variables involved, he finds not only the original cardinal force, but also two extra torque forces, namely the epsilon and alpha torques. The prime objection to the Graneau method arises from the minimum mathematical requirement for applying the principle of virtual work. This minimum requirement is conservation of energy for the potential function. This is not shown by Graneau, nor can it be shown, because "potential 2", as an inverse distance law multiplied by an angle function, is not energy conserving. However, this deficiency is a major one, associated also with every "magnetic potential" definition. Indeed, as an example, we may refer to the fact that a "magnetic potential" can not determine consistently the energy of the following two identical cases. The energy of a pair of two similar permanent magnets, as compared to the energy of a pair of two similar coils, supplied with constant currents and generating magnetic fields identical to the magnets. The energy of the interacting coils is assumed to be supplied by the voltage sustaining the currents. However, for the case of the equivalent permanent magnets the interacting energy is assumed to be taken out from the magnetic potential of the fields themselves. It is obvious that for the two cases of pair of fields, assumed identical, the same potentials are assumed different for the energy transactions. If a consistent magnetic "potential" existed, it should have provided an identical description of the energy transactions for the magnets and coils. This inherent weakness,

also known to the classical electromagnetism, does not allow the definition of a proper energy-conserving magnetic potential and apparently led Neumann to abandon his own concept of potential. When the Graneau/Neumann potential is differentiated (applying the principle of virtual work), additional forces appear which are not present in the original cardinal law; e.g. the alpha and epsilon torques. This proliferation of forces can be attributed to the fact that the potential is energy non-conserving and therefore, not applicable to the principle of virtual work.

The key to explain in a simple way, the violent explosions and other interesting phenomena in spark discharges, is to consider the cardinal law for individual moving charges. Graneau avoided the direct use of the cardinal law, saying that this law is not applicable to individual charges. Graneau indirectly cites Lorentz[3] for his argument. However, Lorentz has given no explicit counter evidence. Nor it was possible for such evidence to exist[4] at his time. The only experimental evidence known at that time were the cathode rays, i.e., moving electrons under the influence of a magnet or an electromagnet. This displayed the effect of an effectively closed circuit on a moving charge. However, it is well known[5] that both laws that of Ampere and that of Lorentz provide identical results for such a closed circuit and individual charges. Today's evidence of self interactions in high energy beams may allow us to distinguish between the two laws. However, the relevant effect of beam self-focusing[6] favors the cardinal law of Ampere.

The cardinal law of electrodynamics for individual charges may be found from the cardinal law, Formula (1), in the same way that Lorentz[7] developed his force law from the Coulomb, Biot-Savart or Grassmann law, that is, by substituting qv for ids, where q is the quantity of charge and v is its velocity, I is the corresponding current intensity, ds is the corresponding current element length, i.e.,

$$ids = (qds)/dt = qv \qquad 3.$$

Adding the Coulomb force:

$$F_{12} = r_{12}q_1q_2/r_{12}^3 \qquad 4,$$

which always coexists between charges, into Formula (1), then, this formula should be rewritten, using vector notation and electrostatic units, $(k=1/c^2)$ as:

$$F_{12}=-r_{12}(q_1q_2/r_{12}^3)[2v_1 \cdot v_2/c^2 - (3/c^2r_{12}^2)(v_1 \cdot r_{12})(v_2 \cdot r_{12}) - 1] \qquad 5.$$

This is the author's formula, first announced and published[7] in 1988, which in every respect may replace the Lorentz law for individual charges, which is given as:

$$F_{12} = q_1(E_1 + v_1 \times B_2/c) = q_1[r_{12}q_2/r_{12}^3 + v_1 \times ((q_2v_2 \times r_{12})/r_{12}^3c)/c] =$$

$$(q_1q_2/r_{12}^3)[r_{12} + (v_1 \cdot r_{12})v_2/c^2 - (v_1 \cdot v_2)r_{12}/c^2] \qquad 6.$$

Lorentz's Formula (6) implies only repulsive forces between similar charges. However, Formula (5), implies attractive as well as repulsive forces which may look as strong compressive or explosive forces in strong currents in high density electrolytes or plasmas, containing fast moving charges. In a high density current, several charges move side by side, parallel to one another. Let us consider two of those charges. Suppose, they are two electrons, moving in such a way, that $v_1=v_2=v$, $q_1=q_2=e$, with a) r_{12} perpendicular to their velocity v, and b) r_{12} parallel to their velocity v. In case (a), v and r_{12} perpendicular, Formula (5) and Formula (6) reduce respectively to:

$$F_{12} = r_{12}(e^2/r_{12}^3)(1-2v^2/c^2) \qquad 7.$$

$$F_{12} = r_{12}(e^2/r_{12}^3)(1-v^2/c^2) \qquad 8.$$

In case (b), v and r_{12} parallel, Formula (5) and (6) reduce respectively to:

$$F_{12} = r_{12}(e^2/r_{12}^3)(1+v^2/c^2) \qquad 9.$$

$$F_{12} = r_{12}(e^2/r_{12}^3) \qquad 10.$$

The cardinal law based Equations (7) and (9), differ from the Lorentz Equations (8) and (10). However, for forces caused by closed circuits, the closed circuits integrals of Equations (7) and (9), produce identical results to those of Equations (8) and (10). This should not surprise us. What may surprise us, however, are the forces between individual charges. In a high current density plasma, i.e., an arc, the Lorentz forces, Equation (8) and (10), are repulsive. However, the cardinal forces (7) and (9) are more complex. The force based on Equation (7), is, first, repulsive for low velocity similar charges, ($v < 0.707c = c/\sqrt{2}$), and second, attractive for high velocity similar charges ($v > 0.707c$). The force based on Equation (9), is always repulsive. Both the cardinal law forces based on Equations (7) and (9), have unique features. According to Equation (7), repulsion may turn to attraction, when similar charges sufficiently accelerate, and vice versa, attraction may turn to repulsion when the charges sufficiently decelerate. These features readily explain violent lateral explosions in arcs and other relevant phenomena. For example, suppose, a high density beam of electrons travelling at a sufficiently high speed. Then, according to Equation (7), the electrons are compressed. After a while they reach a metal anode and vigorously decelerate, due to their collisions with the metal. The resulting strong repulsive force, produced by the dominating coulomb term of Equation (7), then, appears as a strong lateral explosion of the beam of electrons. Thus the cardinal law of Ampere seems to explain the self-focusing([ ]) of sufficiently fast electron beams; the explosions and high pressures in water arc guns, and other phenomena, described by Graneau([ ]). There is only one question left. Is there enough acceleration in a water arc for the charge velocities to surpass the 0.707C limit, and produce the necessary initial attraction-compression. The answer is yes. To achieve a

velocity of 0.707 C, electrons require an accelerating potential of the order of 125000 volts, which itself is not very high. In addition, the repulsive character of the complementary Equation (9), assists the accelerating potential, by propelling forward the front runner charges. Therefore, Equation (9) boosts front charges to much higher velocities than those determined by the external cathode-anode voltage, allowing them to surpass the 0.707c limit with much less voltage than 125 Kv.

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MARINOV'S COMMENTS ON THE PRECEDING PAPER BY P. T. PAPPAS

In my paper "On the electric intensities induced in railguns" which is published in this volume, I explain which is the reason for the explosive forces observed in wires when strong currents flow along them.

On the other hand, it is lost time, I think, to discuss the results which can be obtained on the basis of Ampere's formula, as this formula is SIMPLY WRONG. My Bul-Cub machine without stator (TWT-III, p. 48) and my Rotating Ampere Bridge with Displacement Current (TWT-IV, p. 126) have shown that Ampere's formula is in conflict with the physical reality.

But Prof. Pappas raised in his paper a very interesting question which I should like to comment on. Prof. Pappas writes on p. 1 of his paper:

> ... as an example, we may refer to the fact that a "magnetic potential" can not determine consistently the energy of the following two identical cases. The energy of a pair of two similar permanent magnets, as compared to the energy of a pair of two

similar coils, supplied with constant currents and generating magnetic fields identical to (these) of the magnets. The energy of the interacting coils is assumed to be supplied by the voltage sustaining the currents. However, for the case of the equivalent permanent magnets the interacting energy is assumed to be taken out from the magnetic potential of the fields themselves. It is obvious that for the two cases of pair of fields, assumed identical, the same potentials are assumed different for the energy transactions.

First I have to note that the magnetic energy of the interacting coils (let us consider for simplicity only a single coil) is taken from the battery supplying the current only during the time of the increase of the current from zero to its nominal value, I, because of the induced back tension

$$U_{back} = L(dI/dt),$$ (A)

where L is the inductance of the coil. The electrical energy consumed from the battery and "transformed" into magnetic energy is

$$W = \int_0^{t_o} U_{back} I dt = \int_0^I L I dI = (1/2) L I^2,$$ (B)

where t_o is the moment when the current reaches its nominal value I.

By switching off the current, this magnetic energy will be returned to the battery, again because of the induced back tension which, in this case, will have the same polarity as the driving tension supplied by the battery. During the time when the current is constant, the energy consumed from the battery goes to cover only the ohmic heat losses. If the coil is superconducting, the battery can be excluded from the circuit and once it has sent the current I circulate in the coil, this current will circulate eternally.

The magnetic energy of the permanent magnet (again I consider only one magnet) is the sum of the magnetic energies of all its elementary magnetic DOMAINS which are aligned along a preferred direction. Before the magnetization of the iron this magnetic energy DOES EXIST, however, as the elementary magnetic domains are not aligned along a preferred direction, there is no experimental possibility to establish MACROSCOPICALLY its existence.

We can ascribe magnetic potentials both to the coil and to the permanent magnet and operate with them on exactly equal ways for making all necessary calculations.

The big problem which Pappas has raised, without having rightly indicated it, is however the following:

To magnetize a coil, one must spend the energy (B). Has one to spend the same energy to magnetize a piece of iron making it permanent magnet with the same size which generates exactly the same magnetic potential in space? And the answer, in surprise to all supporters of the energy conservation law, is: NO! - The magnetic energy EXISTS in the piece of non-magnetized iron, as the elementary magnetic domains are all the time magnetized. By putting the iron in an external magnetic field and by magnetizing it, we do not FURNISH some additional energy to the iron and consequently we do not take some energy from the source generating the external magnetic field. And if a permanent magnet demagnetizes spontaneously (in a shorter or longer period of time), it does not deliver energy, as is the case with a coil by switching off the current. Here I assume that the hysteresis curve of the iron has surface zero and thus there are no hysteresis losses; it is a big error to think that the hysteresis losses are transformed magnetic energy, as somebody wrongly may think that the ohmic losses of a coil are its transformed magnetic energy.

To make my assertions more clear, let us consider the following thought experiment, which, of course, can be more or less exactly realized:

Let us have a cylindrical current coil and at a certain distance from it a coaxial cylindrical piece of non-magnetized ABSOLUTELY SOFT iron, i.e., with residual induction (the iduction B_r for H = 0) and coercive force (the reverse external magnetic intensity H_c needed to reduce B to zero) equal to zero.

The coil will attract the iron with a certain feeble force. When the iron will approach the coil the attracting force will become bigger and bigger and the velocity of the iron

piece will increase, reaching its maximum when it will cross the middle of the coil and when its magnetization will be maximum. Due to its inertia, the iron will exit from the other orifice of the coil, diminishing its velocity and losing its magnetization, until it will reach the same distance at the other side of the coil as was the initial one, where it will stop moving and the whole process will be repeated in the opposite direction. Thus, if there will be no losses, the iron piece will begin to oscillate infinitely long; if there will be losses, the oscillations will be damped.

Let us consider the case where the motion of the iron piece will be stopped after half a period of its oscillation. It is clear that during the first fourth-period there was a back tension induced in the coil and the kinetic energy acquired by the iron was equal to the electric energy lost by the battery. During the second fourth-period, again due to the induced back tension, the kinetic energy lost by the iron will be equal to the elecric energy returned to the battery. But the magnetic energy of the iron when it was in the coil has been produced from NOTHING.

The experiment can be repeated with a permanent magnet. Again the same eternal free or damped oscillations will be observed in which there will be a continuous transformation of electric energy into kinetic energy and vice versa.

Two German students, Hermann Lübers and Martin Allerman, demonstrated on the Dr. Nieper's conference in Hannover in March 1987 the following experiment:

A permanent magnet can rotate about an axle in front of another stationary permanent magnet. When the positive pole of the rotating magnet is attracted by the negative pole of the stationary magnet, one changes with a quick magnetizing pulse the polarization of the stationary magnet to the opposite. Consequently the positive pole of the rotating magnet will be now repulsed and its negative pole will be attracted. Then again the polarity of the stationary magnet is changed to the opposite and thus one obtains an electromagnetic motor. The German students hoped that the magnetic energy lost for the remagnetizations of the stationary magnet can be made less than the kinetic rotational energy acquired by the rotating magnet. I THINK THAT THEIR EXPECTATIONS ARE RIGHT (although in their set-up this was not achieved), as one has not to spend energy for the magnetization (re-magnetization) of the iron (if there are no hysteresis losses). And as showed above, the electric energy spent for "magnetizing a coil" returns to the battery after "demagnetizing" it.

Here the following objection can be raised: The hysteresis losses can be made low, only if the corcive force, H_c, will be very small. However, at a low coercive force, the rotating permanent magnet will remagnetize the stationary magnet by its own field and the latter will be unable to repulse the former. Thus the coercive force must be considerable. In such a case the hysteresis losses are inevitable. Thus the construction of a perpetuum mobile on this principle becomes very problematic.

The fact that one can magnetize iron without spending energy is the reason for the violation of the energy conservation law in my machine MAMIN COLIU (TWT-III, p. 89):

The magnetic flux generated by the stationary and rotating magnets in MAMIN COLIU's iron core is bigger when the magnets are overlapping one another. The alternating magnetic flux generates an alternating current in the coil. However, because of the cylindrical symmetry, the "back magnetc field" generated by the coil's current cannot brake the rotation of the rotating magnets. For the magnetization of the iron core NO energy is needed, emphasizing that the magnetization of the core is always with one polarity but, during the rotation of the movable magnets, it is different at the different cross-sections of the core and the NET magnetic flux is oscillating about some middle value, inducing an alternating tension in the coil.

Thus I consider Pappas' INDIRECT approach to the problem whether one needs energy for the magnetization of a piece of iron as very IMPORTANT, noting that Pappas' principal attention is directed to the question whether the magnetic potential **A** describes the energetic aspects of the magnetic interactions in the same way as the electric potential Φ describes the energetic aspects of the elctrostatic interactions. The answer is: In both cases one must take the potential energies $U = q\Phi$, $W = (q/c)\mathbf{v}.\mathbf{A}$, not the potnetials Φ, **A**. For the electric interactions the results in both cases are the same, as U is a scalar

product of q and Φ, but for the magnetic interactions this cannot be done.

I showed that the fundamental equation of motion in electromagnetism, called by me the Newton-Lorentz equation, must be written in the form (see TWT-II, pp. 85 and 86)

$$(d/dt)(p_0 + qA/c) = - q\,\text{grad}(\Phi - v.A/c) = - \text{grad}(U - W), \tag{C}$$

where p_0 is the proper momentum of a particle with charge q crossing with a velocity v a reference point where the electric and magnetic potentials of the surrounding system are Φ and A.

Let us consider two positive electric charges. Their electric energy is $U = q_1q_2/r$. They must repel one another, as in such a case their electric energy decreases and the loss of electric energy transforms into kinetic energy of the charges. Similarly two masses attract one another, as their gravitational energy $U_g = - \gamma m_1m_2/r$ is negative; at their attraction their gravitational energy DECREASES and the loss is transformed into kinetic energy of the masses.

Let us now consider two magnets (permanent magnets or coils). If they will attract one another with their opposite poles, their magnetic energy will increase and their kinetic energy, TOO. If these magnets will repel one another with their homonymous poles, their magnetic energy again will increase and their kinetic energy, TOO. The reason for this is that W enters in equation (C) with a sign OPPOSITE to U. We thus see that the MOST SIMPLE interaction between two magnets VIOLATES the energy conservation law. I have not read a textbook where one has pointed out to this FACT.

Thus the magnetic energy is a potential energy and can be treated in many aspects as the electric potential energy, but in many aspects not. The fact that the term $(q/c)dA/dt$ figures on the left side of equation (C) makes magnetism VERY STRANGE, leading to violation of the simple Newton's third law and consequently to violation of the laws of conservation of momentum and angular momentum, as my Bul-Cub Machine without Stator and my Rotating Ampere Bridge with Displacement Current have demonstrated. The strange fact that iron can be magnetized without loss of energy leads to violation of the energy conservation law, as my machine MAMIN COLIU has demonstrated.

The reason why the machine TESTATIKA violates the energy conservation law is not clear for me, as I was not allowed to become acquainted with the real principle of its action.

ADDENDUM. It is only our choice to consider the electric energy of two positive charges as positive and the gravitational energy of two masses (which are "positive" gravitational charges) as negative, so that the energy conservation law can be preserved. Thus we can save the energy conservation law at the magnetic interactions, if we consider the magnetic energy of two charges moving with velocities v_1, v_2 not in the form

$$W = q_1q_2v_1.v_2/c^2r \tag{D}$$

as conventional physics (including ME) does, but in the form

$$W = - q_1q_2v_1.v_2/c^2r. \tag{E}$$

In this case the ninth axiom of my absolute space-time theory must be written not in the form (see CLASSICAL PHYSICS, vol. III, p. 70)

$$dU + dW = dU_w, \tag{F}$$

where U is the sum of the space energies (electrical and gravitational) of any isolated material system, W is the sum of its space-time energies (magnetic and magretic, the last being at the present time only hypothetically introduced), and U_w is the world energy of the system (its gravitational energy with the masses of the whole Universe), but in the form

$$dU - dW = dU_w. \tag{G}$$

This change will, probably, make many formulas more elegant (see §22 in vol. III of CLASSICAL PHYSICS). For example, now we shall have (see my comments to the following paper of Prof. Spencer):

$$U + W = (q_1q_2/c^2r(c^2 - v_1.v_2) = - (q_1q_2/c^2r)\overset{\leftrightarrow}{v}_1.\overset{\leftrightarrow}{v}_2. \tag{H}$$

Three Formulations of Electrodynamics

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The paper discusses the three forms of electrodynamic equations which are believed by some modern physicists: the Weber equation, the classical equation, and the new relative velocity formulation. It is shown that only one of these formulations can possibly predict the tangential forces which have been observed in many experiments and the Edwards effect.

Many different equations for the force between moving charges have been proposed[1,2,3]. But at low velocities only three of these are distinct and have modern adherents.

The oldest equation is that suggested by Wilhelm Weber[4] in 1848. This equation is seriously considered by Wesley[5], Marinov[6], Graneau[7], Papas[8] and Phipps[9]. The Weber equation can be derived[2] from a scalar potential which is a function of the relative velocity of source and receiver. Consider a charge Q which is in motion at velocity **v**, Fig. 1, in a non-rotating coordinate system in which the force on a test charge at point P is to be measured. The distance between source and receiver is r. A unit vector $\mathbf{a_r}$ points from source to receiver. The test charge at point P moves with velocity **u**. Expressed in the mks system the Weber scalar potential is

$$\varphi_w = \frac{Q}{4\pi\varepsilon_0 r}\left\{1 + \frac{1}{2c^2}\left[(\mathbf{v}-\mathbf{u})\cdot\mathbf{a_r}\right]^2\right\}$$
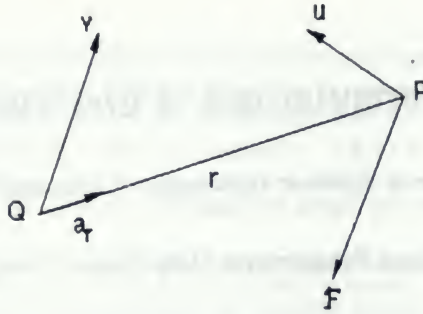
(1)

Fig. 1

The force per unit test charge at point P produced by the charge Q is

$$\mathbf{F_w} = -\text{grad } \varphi_w \tag{2}$$

or

$$\mathbf{F_w} = \mathbf{a}_r \frac{Q}{4\pi\varepsilon_0 r^2} \left\{ 1 + \frac{(\mathbf{v} - \mathbf{u})\cdot(\mathbf{v} - \mathbf{u})}{c^2} - \frac{3\left[(\mathbf{v} - \mathbf{u})\cdot \mathbf{a}_r\right]^2}{2c^2} \right\}$$
$$- \mathbf{a}_r \frac{Q}{4\pi\varepsilon_0 c^2 r} \left[\frac{d\mathbf{v}}{dt} - \frac{d\mathbf{u}}{dt} \right] \cdot \mathbf{a}_r \tag{3}$$

If the velocities $\mathbf{u}$ and $\mathbf{v}$ are constant, this equation reduces to that first proposed by Gauss in a letter in 1835. Note that this equation gives a force $\mathbf{F_w}$ which is always in the radial direction.

The other two equations considered today incorporate the Neumann[10] idea of a vector potential. The electric field strength $\mathbf{E}$ is defined in terms of both a scalar potential φ and a vector potential $\mathbf{A}$ by the equation

$$\mathbf{E} = -\text{grad } \varphi - \frac{\partial \mathbf{A}}{\partial t} \tag{4}$$

while the magnetic flux density $\mathbf{B}$ is expressed in terms of the vector potential $\mathbf{A}$ as

$$\mathbf{B} = \text{curl } \mathbf{A} . \tag{5}$$

The force per unit charge $\mathbf{F}$ is defined as suggested by Heaviside[11] as

$$\mathbf{F} = \mathbf{E} + \mathbf{\omega} \times \mathbf{B}. \tag{6}$$

In the classical theory which is in harmony with the work of Maxwell[12], Lorenz[13] and Lorentz[14], the scalar potential is assumed to be a function of position

$$\varphi = \frac{Q}{4\pi\varepsilon_0 r} \tag{7}$$

but the vector potential is a function of the velocity $\mathbf{v}$ of the source

$$\mathbf{A}_c(t) = \frac{\mu_0}{4\pi} \frac{Q \, \mathbf{v}(\tau)}{r}, \tag{8}$$

while the velocity of the receiver enters only in the term

$$\mathbf{w} = \mathbf{u}. \tag{9}$$

Thus, the electric and magnetic field vectors $\mathbf{E}$ and $\mathbf{B}$ are assumed to be functions of the absolute velocity of the source $\mathbf{v}$, while the force on a moving charge is assumed to be a linear function of the velocity of the test charge.

At low velocities, $v/c \ll 1$, $u/c \ll 1$, where retardation can be neglected, the classical formulation is

$$\mathbf{F}_c = \frac{Q}{4\pi\varepsilon_0 r^2} \left[\mathbf{a}_r + \frac{\mathbf{u}(t) \times (\mathbf{v}(t) \times \mathbf{a}_r)}{c^2} \right]$$
$$- \frac{Q}{4\pi\varepsilon_0 c^2 r} \left[\frac{d\mathbf{v}(t)}{dt} - \frac{\mathbf{u}(t)}{c} \times \left[\frac{d\mathbf{v}(t)}{dt} \times \mathbf{a}_r \right] \right]. \tag{10}$$

This equation is believed by the majority of modern physicists.

However. if we follow the Gaussian idea that relative velocities should be employed by utilizing both a scalar and a vector potential, the expressions[2,3] for $\mathbf{A}$ and $\mathbf{w}$ become

$$\mathbf{A}_R(t) = \frac{Q\left(\mathbf{v}(t) - \mathbf{u}(t)\right)}{8\pi\varepsilon_0 c^2 r} \tag{11}$$

$$\mathbf{w}_R = \mathbf{u}(t) - \mathbf{v}(t) \tag{12}$$

The relative velocity form of the electrodynamic force equation at low velocities and neglecting retardation becomes

$$\mathbf{F}_R = \frac{Q}{4\pi\varepsilon_0 r^2}\left[\mathbf{a}_r + \frac{\left(\mathbf{v}(t) - \mathbf{u}(t)\right) \times \left(\left(\mathbf{v}(t) - \mathbf{u}(t)\right) \times \mathbf{a}_r\right)}{2c^2}\right]$$
$$- \frac{Q}{8\pi\varepsilon_0 c^2 r}\left[\frac{d\mathbf{v}(t)}{dt} - \frac{d\mathbf{u}(t)}{dt} + \frac{\left(\mathbf{v}(t) - \mathbf{u}(t)\right)}{c} \times \left[\left[\frac{d\mathbf{v}(t)}{dt} - \frac{d\mathbf{u}(t)}{dt}\right] \times \mathbf{a}_r\right]\right]. \tag{13}$$

This equation is associated with the recent investigations[2,3] of Moon, Spencer, Mirchandaney, Uma, and Mann.

The Force Between Current Elements

From each of the three equations it is possible to determine an expression for the force between current elements. In order to do so we must consider the forces between the charges in each current element.
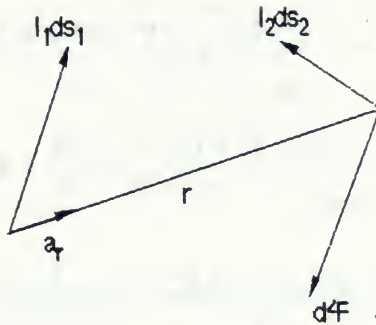
Fig. 2

For the positive charge which is stationary, $\mathbf{v}_+ = \mathbf{u}_+ = 0$, so

$$F_{w++} = F_{c++} = F_{R++} = \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2} \mathbf{a}_r \qquad (14)$$

However, if we consider the positive charge in ds_1 for which $\mathbf{v}=\mathbf{v}_+=0$ and the negative charge in ds_2 for which $\mathbf{u}=\mathbf{u}_-$, we obtain three different expressions

$$\mathbf{F}_{w+-} = -\frac{|Q_1 Q_2| \mathbf{a}_r}{4\pi\varepsilon_0 r^2}\left[1 + \frac{\mathbf{u}_- \cdot \mathbf{u}_-}{c^2} - \frac{3\left[\mathbf{u}_- \cdot \mathbf{a}_r\right]^2}{2c^2}\right]$$
$$- \mathbf{a}_r \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 c^2 r}\left[\frac{d\mathbf{u}_-}{dt} \cdot \mathbf{a}_r\right]$$

$$F_{c+-} = -\frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2}\left[\mathbf{a}_r\right] \qquad (15)$$

$$F_{R+-} = -\frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2}\left[\mathbf{a}_r + \frac{\mathbf{u}_- \times (\mathbf{u}_- \times \mathbf{a}_r)}{2c^2}\right]$$
$$+ \frac{|Q_1 Q_2|}{8\pi\varepsilon_0 c^2 r}\left[-\frac{d\mathbf{u}_-}{dt} + \frac{\mathbf{u}_-}{c}\times\left[\frac{d\mathbf{u}_-}{dt}\times \mathbf{a}_r\right]\right].$$

For negative charge in ds_1 and positive charge in ds_2, $\mathbf{v}=\mathbf{v}_-$ but $\mathbf{u}_+=0$ so

$$F_{w-+} = -\frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2}\left[\mathbf{a}_r + \frac{\mathbf{v}_- \cdot \mathbf{v}_-}{c^2} - \frac{3\left[\mathbf{v}_- \cdot \mathbf{a}_r\right]^2}{2c^2}\right]$$
$$+ \mathbf{a}_r \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 c^2 r}\left[\frac{d\mathbf{v}_-}{dt}\cdot \mathbf{a}_r\right]$$

$$F_{c-+} = -\frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2}\left[\mathbf{a}_r\right] + \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 c^2 r}\frac{d\mathbf{v}_-}{dt} \qquad (16)$$

$$F_{R-+} = -\frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2}\left[\mathbf{a}_r - \frac{\mathbf{v}_- \times (\mathbf{v}_- \times \mathbf{a}_r)}{2c^2}\right].$$

Finally, for the force between the negative charge in both current elements, $\mathbf{v}=\mathbf{v}_-$ and $\mathbf{u}=\mathbf{u}_-$ so

$$F_{w--} = \mathbf{a}_r \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2}\left[1 + \frac{(\mathbf{v}_- - \mathbf{u}_-)\cdot(\mathbf{v}_- - \mathbf{u}_-)}{c^2} - \frac{3\left[(\mathbf{v}_- - \mathbf{u}_-)\cdot \mathbf{a}_r\right]^2}{2c^2}\right]$$
$$- \mathbf{a}_r \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 c^2 r}\left[\frac{d\mathbf{v}_-}{dt} - \frac{d\mathbf{u}_-}{dt}\right]\cdot \mathbf{a}_r$$

$$F_{c--} = \frac{|Q_1 Q_2| \, \mathbf{a}_r}{4\pi\varepsilon_0 r^2} \left[\mathbf{a}_r + \frac{\mathbf{u}_- \times (\mathbf{v}_- \times \mathbf{a}_r)}{c^2} \right]$$

$$- \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 c^2 r} \left[\frac{d\mathbf{v}}{dt} - \frac{\mathbf{u}_-}{c} \times \left(\frac{d\mathbf{v}}{dt} \times \mathbf{a}_r \right) \right] \tag{17}$$

$$F_{R--} = \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2} \left[\mathbf{a}_r - \frac{(\mathbf{v}_- - \mathbf{u}_-) \times ((\mathbf{v}_- - \mathbf{u}_-) \times \mathbf{a}_r)}{2c^2} \right]$$

$$- \frac{|Q_1 Q_2|}{8\pi\varepsilon_0 c^2 r} \left[\frac{d\mathbf{v}_-}{dt} - \frac{d\mathbf{u}_-}{dt} + \frac{\mathbf{v}_- - \mathbf{u}_-}{c} \times \left[\left[\frac{d\mathbf{v}_-}{dt} - \frac{d\mathbf{u}_-}{dt} \right] \times \mathbf{a}_r \right] \right].$$

The total force between the current elements is

$$F = F_{++} + F_{+-} + F_{-+} + F_{--}. \tag{18}$$

Addition gives,

$$F_{\mathbf{w}} = \mathbf{a}_r \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2} \left[-2 (\mathbf{v}_- \cdot \mathbf{u}_-) + 3 (\mathbf{v}_- \cdot \mathbf{a}_r) (\mathbf{u}_- \cdot \mathbf{a}_r) \right]$$

$$F_c = \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 r^2} \left[\mathbf{u}_- \times (\mathbf{v}_- \times \mathbf{a}_r) \right]$$

$$+ \frac{|Q_1 Q_2|}{4\pi\varepsilon_0 c^2 r} \left[\mathbf{u}_- \times \left(\frac{d\mathbf{v}_-}{dt} \times \mathbf{a}_r \right) \right] \tag{19}$$

$$F_R = \frac{|Q_1 Q_2|}{8\pi\varepsilon_0 c^2 r^2} \left[\frac{(\mathbf{u}_- \times (\mathbf{v}_- \times \mathbf{a}_r)) + (\mathbf{v}_- \times (\mathbf{u}_- \times \mathbf{a}_r))}{2c^2} \right]$$

$$+ \frac{|Q_1 Q_2|}{8\pi\varepsilon_0 c^3 r} \left[\mathbf{u}_- \times \left[\frac{d\mathbf{v}_-}{dt} \times \mathbf{a}_r \right] + \mathbf{v}_- \times \left[\frac{d\mathbf{u}_-}{dt} \times \mathbf{a}_r \right] \right].$$

However, if there are N_1 and N_2 free elements per unit volume and cross sectional areas are A_1 and A_2

$$|Q_1| = N_1 A_1 |Q_e| |ds_1| \quad , \quad |Q_2| = N_2 A_2 |Q_e| |ds_2| . \tag{20}$$

The current density J in each conductor is

$$J_1 = -|\rho_{1-}| v_- \quad , \quad J_2 = -|\rho_{2-}| u_- \tag{21}$$

and

$$|\rho_{1+}| = |\rho_{1-}| = N_1 |Q_e| \quad , \quad |\rho_{2+}| = |\rho_{2-}| = N_2 |Q_e| \tag{22}$$

so

$$J_1 = -N_1 |Q_e| v_- \quad , \quad J_2 = -N_2 |Q_e| u_- . \tag{23}$$

Therefore, the current in each conductor is

$$I_1 = |J_1| A_1 = N_1 |Q_e| A_1 |v_-| \quad , \quad I_2 = |J_2| A_2 = N_2 |Q_e| A_2 |u_-| . \tag{24}$$

Multiplication by the appropriate element of distance gives

$$I_1 |ds_1| = N_1 A_1 |Q_e| |ds_1| |v_-| = |Q_e| |v_-|$$
$$I_2 |ds_2| = N_2 A_2 |Q_e| |ds_2| |u_-| = |Q_e| |u_-| . \tag{25}$$

Therefore

$$d^2 F_w = a_r \frac{I_1 I_2}{4\pi\varepsilon_0 c^2 r^2} \left[-2 (ds_1 \cdot ds_2) + 3 (ds_1 \cdot a_r)(ds_2 \cdot a_r) \right] \tag{26}$$

which is exactly the equation suggested by Ampere[15] in 1823. However,

$$d^2F_c = \frac{1}{4\pi\varepsilon_0 c^2}\left[\frac{I_1(t)\,I_2(t)}{r^2} + \frac{I_2(t)\,\dfrac{dI_1(t)}{dt}}{cr}\right]\left(ds_2 \times (ds_1 \times \mathbf{a}_r)\right) \tag{27}$$

and

$$d^2F_R = \frac{I_1(t)\,I_2(t)}{8\pi\varepsilon_0 c^2 r^2}\left(ds_1 \times (ds_2 \times \mathbf{a}_r) + ds_2 \times (ds_1 \times \mathbf{a}_r)\right)$$

$$+ \frac{1}{8\pi\varepsilon_0 c^3 r}\left[\begin{array}{l} I_1(t)\,\dfrac{dI_2(t)}{dt}\,ds_1 \times (ds_2 \times \mathbf{a}_r) \\[2mm] + I_2(t)\,\dfrac{dI_1(t)}{dt}\,ds_2 \times (ds_1 \times \mathbf{a}_r) \end{array}\right] \tag{28}$$

In order to measure the force on an element of current $I_2 ds_2$ it is necessary to integrate these expressions about a closed path with respect to ds. However, we have previously[16] shown that for the Ampere-Weber expression for the force between current elements, the result is identical for the Grassmann[17] force

$$d^2F_G = \frac{I_1(t)\,I_2(t)}{4\pi\varepsilon_0 c^2}\left(ds_2 \times (ds_1 \times \mathbf{a}_r)\right) \tag{29}$$

and for the Ampere force. Clearly the classical expression and the Grassmann expression always give forces perpendicular to the current element ds_2. Therefore, in any closed circuit, it is impossible for the Ampere equation to produce any tangential component.

Therefore, we must conclude that the only one of these three formulations which can possibly explain the many experiments[7] of Herring, Graneau and others which appear to prove the existence of tangential forces is the modern relative velocity formulation. A detailed analysis of each of the crucial experiments should be carried out to see if the relative velocity formulation can quantitatively predict the tangential forces found by

Herring, Graneau, Pappas, Phipps and others. This has been started in the analysis of Ampere's hairpin experiment by Saria and Spencer[18].

The Edwards Effect

In the Edwards effect, the source is a steady current so $d\mathbf{v}/dt=0$ and the receiver is stationary so $\mathbf{u}=0$, $d\mathbf{u}/dt$.

Fig. 3
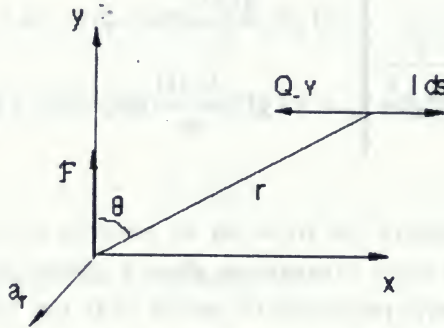
In this special case the Weber equation, Eq. (3), becomes,

$$\mathbf{F_w} = \mathbf{a_r} \frac{Q}{4\pi\varepsilon_0 r^2}\left[1 + \left(\frac{v}{c}\right)^2 - \frac{3\left(\mathbf{v}\cdot\mathbf{a_r}\right)^2}{2c^2}\right].$$

(30)

For the classical equation (10),

$$\mathbf{F_c} = \mathbf{a_r} \frac{Q}{4\pi\varepsilon_0 r^2}$$

(31)

and for the new relative velocity formulation, Eq. (13)

$$F_R = \frac{Q}{4\pi\varepsilon_0 r^2}\left[\mathbf{a}_r - \frac{\mathbf{v}\times\left(\mathbf{v}\times\mathbf{a}_r\right)}{2c^2}\right].$$

$$(32)$$

Adding terms from Eqs. (30), (31) and (32) for both positive and negative charges in the conductor, the coulomb terms cancel and

$$F_w = \mathbf{a}_r\frac{Q}{4\pi\varepsilon_0 r^2}\left(\frac{v}{c}\right)^2\left[1 - \frac{3}{2}\sin^2\theta\right]$$

$$(33)$$

$$F_c = 0$$

$$(34)$$

$$F_R = \frac{Q}{8\pi\varepsilon_0 r^2}\left(\frac{v}{c}\right)^2\cos\theta\,\mathbf{a}_y.$$

$$(35)$$

The classical theory predicts that the Edwards effect does not exist. Both the Weber and the relative velocity formulation give a force which is proportional to $(v/c)^2$. However, the directions and the angular dependence are different. Only the expression, Eq. (35), is in agreement with the Edwards experiments as has been previously pointed out[20].

Conclusions

Thus, we can conclude that there appears to be a large body of experiments which can be explained by the new relative velocity formulation of electrodynamics. The Weber-Ampere formulation appears to provide tangential forces when d^2F_w is considered. However, when the necessary integrations are carried out for a closed circuit dF_w is always perpendicular to the current element and agrees exactly with classical theory. The relative velocity expression contains a true tangential component which does not disappear when d^2F_R is integrated about a closed circuit. Detailed analysis of each experimental configuration is necessary to

determine whether these qualitative conclusions can be verified quantitatively in all cases.

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MARINOV'S COMMENTS ON THE PREVIOUS PAPER BY D. E. SPENCER (et al.)

I published in TWT-IV (p. 200) a paper of Prof. Spencer on the force between moving charges, noting in my comments that I consider her approach as scientifically unsound. I sustain further this opinion.

The force with which one moving charge acts on another moving charge (as a matter of fact the potential energy of two moving charges) is the basis on which the whole body of electromagnetism is to be built. Thus proceeding from the potential energy of two electrical charges q_1, q_2 moving with velocities v_1, v_2 in absolute space, one has to explain all electromagnetic phenomena. The proponents of Weberian electromagnetism and of the Ampere formula for the force of interaction between two moving charges often forget that they have to explain a tremendously big amount of electromagnetic effects which, for the time being, are explained only proceeding from the well-known Newton-Lorentz equation (i.e., from the Neumann form for the magnetic energy of two moving charges) and consequently from the Grassmann formula for the force of interaction acting between two moving charges, as the latter is a direct result of the Newton-Lorentz equation.

In addition to the effects known to today's physics, one has to explain also the effects demonstrated by the machines constructed recently by me which violate Newton's third law (the Bul-Cub machine without stator and the Rotating Ampere Bridge with Displacement Current, presented in TWT-III and TWT-IV). It is well known that Grassmann's formula violates Newton's third law but before me none (with the exception of Graham and Lahoz, Nature, 285, 154, 1980, who have not understood the importance of their experiment) has observed such violations. Strangely enough, nobody of the proponents of Ampere's formula has commented on my experiments. I understand all these people, as my experiments offer a direct experimental disproof of Ampere's formula and a firm experimental verification of Grassmann's formula. Concerning my friends from the "Amperian camp" (all of whom have read or at least purchased TWT) I can only repeat the ancient maxim: The neglection of experiments can never save a theory.

Prof. Spencer thinks that the formula which she obtains at her "new relative velocity" approach allows to explain the effect in the Edwards' experiment (Phys. Rev. D, 14, 922, 1976). First I must say that the report of Edwards et al. is written so badly that only a person who has not what to do on this Earth would spend time to try to decipher it. Sansbury has presented a much more better written report (Rev. Sc. Instr., 56, 415, 1985) on a similar experiment, so that I suggest to the people who wish to understand the essence of such kind of experiments to look at Sansbury's paper.

I reprint Sansbury's paper in this volume, so that the readers of TWT do not lose time for searching it in the libraries.

The explanation of this effect is childishly simple, and one has not to change the fundamental electromagnetic formula with the aim to "explain" it, as Prof. Spencer does. The fundamental formulas are saint. They must be touched only at extremely critical situations. Edwards' effect is a simple side effect and its explanation can be done as follows:

Electric current begins to flow along a metal wire when, in general, at the one of its ends, there is electrons' concentration higher than the concentration of the positive ions of the metal lattice, while, at the other end, there is electrons' concentration lower than the concentration of the positive ions. The electrons will be "sucked" by the positively charged end of the wire. This "sucking" will be possible only if the concentration of the electrons throughout the wire will become lower than the concentration of the positive ions, otherwise the electrons in the wire can not "feel" that there is scarcity on electrons at the one end of the wire. It is   nonsense   to think that the higher concentration of the positive ions at the one end of the wire (at the positive electrode of the battery) acts directly with its Coulomb electric potential on the excess of electrons at the other end of the wire (at the negative electrode of the battery). Indeed, the wire can be long kilometers and this direct Coulomb action can become nil. Thus the excessive positive charges from the positive electrode of the battery attract by Coulomb forces the electrons in the immediate neighbourhood of the wire, the emerging higher positive concentration of the ions attracts the electrons from its immediate neighbourhood and the process propagates with a velocity near to c towards the negative electrode of the battery. We thus see that during the transfer of current the whole wire must become slightly positively charged and, clearly, at a higher current, the positive charging of the wire must be higher.

This is exactly what Sansbury has observed: When current went along a wire, it attracted a negatively charged vane and repulsed a positively charged vane, independently of the direction of the current.

Here I must add that besides this effect which, following Prof. Spencer, I shall call the Edwards effect, there is also the Kennard effect which appears only if the current wire moves with respect to absolute space. In the last case an induced electric intensity appears according to the formula

$$E = (V.grad)A + V \times rotA,$$

where **V** is the absolute velocity of the wire and **A** is the magnetic potential generated by it at the laboratory's point where the positively (negatively) charged vane is placed. If Sansbury has done his experiment more attentively, he could see that there is an additional force acting on the positively (negatively) charged vane coming from the electromagnetic induction due to the absolute velocity of the Earth. This effect depends not on the square of the current but on the current, i.e., for opposite currents it has opposite signs and, for a prolongated rectangular loop, depends on the angle which this loop concludes with the laboratory's absolute velocity. I have observed this electric force in my inertial Kennard experiment (see TWT-IV, p. 110).

Now I wish to make some "technical" remarks on Prof. Spencer's paper.

Prof. Spencer obtains formula (3) for the force with which a charge Q, moving with a velocity **v**, acts on a test unit charge, moving with a velocity **u**, proceeding from formula (2), in which she puts the Weber electromagnetic potential (1). Let us accept for bre-

vity $\mathbf{w} = \mathbf{v} - \mathbf{u}$, where $\mathbf{w}$ is the velocity of the charge Q with respect to the test unit charge (compare with Prof. Spencer's formula (12)). At $\mathbf{w} = \mathbf{Const}$, the calculation gives

$$(4\pi\varepsilon_0/Q)\mathbf{F_W} = -\text{grad}\{1/r + (\mathbf{w.r})^2/2c^2r^3\} =$$
$$-\text{grad}(1/r) - \{(\mathbf{w.r})^2/2c^2\}\text{grad}(1/r^3) - \{(\mathbf{w.r})/c^2r^3\}\{(\mathbf{w.grad})\mathbf{r} + \mathbf{w}\times\text{rot}\mathbf{r}\} =$$
$$\mathbf{r}/r^3 + 3(\mathbf{w.r})\mathbf{r}/2c^2r^5 - (\mathbf{w.r})\mathbf{w}/c^2r^3.$$

Meanwhile, if we put in Prof. Spencer's formula (3) $d\mathbf{v}/dt - d\mathbf{u}/dt = d\mathbf{w}/dt = 0$, we obtain

$$(4\pi\varepsilon_0/Q)\mathbf{F_w} = \mathbf{r}/r^3 - 3(\mathbf{w.r})^2\mathbf{r}/2c^2r^5 + w^2\mathbf{r}/c^2r^3.$$

Thus formula (3) of Prof. Spencer can be not obtained from formulas (1) and (2).

Prof. Wesley (Progress in Space-Time Physics 1987, p. 195) obtains the force with which the charge Q moving with the velocity $\mathbf{w}$ acts on the unit test charge at rest, proceeding from a slightly different Weber's potential (note the negative sign before the second term)

$$\Phi_W = (Q/4\pi\varepsilon_0)\{1/r - (\mathbf{w.r})^2/2c^2r^3\},$$

and making the following calculation:

For the rate of change of this potential in time, we shall have, noting that $d\mathbf{r}/dt = -\mathbf{w}$, as vector $\mathbf{r}$ points from Q to the unit test charge, and thus $\mathbf{w} = d(-\mathbf{r})/dt$,

$$(4\pi\varepsilon_0/Q)d\Phi_w/dt = \{(\mathbf{r.w})/c^2r^3\}\{c^2 + w^2 - 3(\mathbf{r.w})^2/2r^2 - \mathbf{r.}(d\mathbf{w}/dt)\}.$$

Prof. Wesley obtains the last term wrongly with sign "+" and I think that this is only a misprint.

As

$$d\Phi_w/dt = \mathbf{F_w.w},$$

we obtain the Weber force

$$(4\pi\varepsilon_0/Q)\mathbf{F_w} = (\mathbf{r}/c^2r^3)\{c^2 + w^2 - 3(\mathbf{r.w})^2/2r^2 - \mathbf{r.}(d\mathbf{w}/dt)\}.$$

Comparing this formula with formula (3) of Prof. Spencer, we see that they coincide. Thus Prof. Spencer's formula (3) can be obtained proceeding from Wesley's form of the Weber potential and following Wesley's way of calculation.

Let me note that Riemann (A Contribution to Electromagnetism, in the book Energy Potential, towards a new electromagnetic theory, by Carol White, Campaigner Publ., New York 1977) writes the Weber potential in the form of Wesley (see p. 279 of the book), although without the factor "2" in the denominator.

I should like also to note that the way on which Prof. Spencer obtains Ampere's formula (the first formula (19)) from the formula for the Weber force (3) is already indicated by Prof. Wesley on p. 199 of his Space-Time Physics 1987.

At the end I shall give, as reference, the forms of the electric and magnetic energies of two electric charges according to Coulomb-Neumann, Coulomb-Weber and Coulomb-Riemann. For clarity, I shall not use Prof. Spencer's notations but more simple notations, deno-

ting the charges of the first and second particles by q_1, q_2 and their velocities in absolute space by $\mathbf{v}_1$, $\mathbf{v}_2$ which, if measured on a clock which rests in absolute space, are called their universal velocities:

1. COULOMB-NEUMANN ELECTRIC AND MAGNETIC ENERGIES (see, however, my comments to Pappas' article published in this volume)

$$U + W = (q_1 q_2/c^2 r)(c^2 + \mathbf{v}_1 . \mathbf{v}_2) = (q_1 q_2/c^2 r)(c_1 c_2 + \mathbf{v}_1 . \mathbf{v}_2),$$

where $\mathbf{v}_1$, $\mathbf{v}_2$ are called also universal <u>space</u> velocities of the particles, $c_1 = c$, $c_2 = c$ are called their universal <u>time</u> velocities and are equal to the universal light velocity (however, the proper time velocities of the particles $c_{01} = c(1 - v_1^2/c^2)^{-1/2}$, $c_{02} = c(1 - v_2^2/c^2)^{-1/2}$ are not equal one to another), and r is the distance between the particles.

As

$$\overset{\leftrightarrow}{v}_1 = (v_1, ic), \qquad \overset{\leftrightarrow}{v}_2 = (v_2, ic)$$

are the 4-velocities of the particles (the <u>universal</u> 4-velocities), we have

$$W - U = (q_1 q_2/c^2 r)(\mathbf{v}_1 . \mathbf{v}_2 - c^2) = (q_1 q_2/c^2 r)\overset{\leftrightarrow}{v}_1 . \overset{\leftrightarrow}{v}_2.$$

The here indicated notations are introduced by me and many of the notions are attributes of my absolute space-time theory (see my CLASSICAL PHYSICS).

Thus the Coulomb-Neumann electric and magnetic potential energies are the basis of the 4-dimensional approach to electromagnetism which presents the electromagnetic formulas in a very elegant and compact way unifying the space and time characteristics of the particles (see CLASSICAL PHYSICS, volumes 3 - 5).

When proceeding from the Coulomb-Neumann potentials, the force with which two electric charges (two current elements) act one on another is given by the Grassmann formula.

2. COULOMB-WEBER ELECTRIC AND MAGNETIC ENERGIES

$$U + W = (q_1 q_2/c^2 r)\left[c^2 - \{(\mathbf{v}_2 - \mathbf{v}_1) . r\}^2/2r^2 \right] = \frac{q_1 q_2}{c^2 r}\{c^2 - \frac{1}{2}(\frac{dr}{dt})^2\}.$$

When proceeding from the Coulomb-Weber potentials, the force with which two electric charges (two current elements) act one on another is given by Ampere formula.

3. COULOMB-RIEMANN ELECTRIC AND MAGNETIC ENERGIES

$$U + W = (q_1 q_2/c^2 r)\{c^2 - (\mathbf{v}_2 - \mathbf{v}_1)^2\}.$$

It is not clear to me whether the term $(\mathbf{v}_2 - \mathbf{v}_1)^2$ must be divided by the factor 2 (I rather think that it <u>must</u> be divided) and with which force two charges (two current elements) act one on another when proceeding from the Coulomb-Riemann potentials.

As far as I know, in his Course on Theoretical Physics, Prof. God of the Champs Elysees University works only with the Coulomb-Neumann potentials and with Grassmann's formula.

> *The following paper was presented at a conference*
> *held at the Imperial College in London, England*
> *in September 1988. The conference concerned the*
> *physical interpretation of the mathematical*
> *structure which we associate with the theory of*
> *relativity. This is the conference referenced on*
> *p. 295 of Stefan Marinov's book THE THORNY WAY OF TRUTH:*
> *Part IV, from which Stefan Marinov was denied admission.*
> *Several speakers at the conference presented papers*
> *which were rather challenging and so were not in*
> *conformity with what establishment relativists wish*
> *to hear. Notwithstanding Learned society sponsorship,*
> *the organizer was unable to arrange for the conference*
> *proceedings to be accepted by a publisher. The author is*
> *therefore indebted to Stefan Marinov for his willingness*
> *to include this paper in his latest version of:*
> *   THE THORNY WAY OF TRUTH.*

FOUR QUESTIONS CONCERNING PHYSICAL REALITY AND RELATIVITY

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The physical reality underlying the experimental support for
relativity poses certain unanswered questions of particular
significance to the role of the 'observer' versus the role of a
physical electromagnetic frame of reference. These questions,
which concern electrodynamic interactions, energy transfer, and
standing waves as 'forcing' influences, causal to invariance, are
discussed. It is concluded that a physical insight into the
meaning of the theory of relativity has no certain basis until
further experiments resolve several open issues. The most
important ·is whether field energy entrained by standing waves in
test apparatus causes the vacuum coextensive with that apparatus to
have an electromagnetic reference frame seated with and moving with
the apparatus, even though that same vacuum region presents a

different frame of reference to freely moving electromagnetic waves.

There is something disturbing about participating in a conference that concerns the theme "Physical Interpretations of Relativity Theory". Why should a theory need physical interpretation when it owes its recognized existence to its reputed acclaim for having 'explained' a whole series of physical phenomena? Or are we to presume that the theory of relativity is lacking in some respects? Are we to believe that relativity is merely an empirical mathematical formalism that somehow provides a unique but abstract correlation of what is observed but now needs to be transformed into real physics to give it substance?

This conference has the stated intention of reviewing the fruitfulness of 'orthodox' relativity, as developed from the Einstein-Minkowski formulation, and to suggest how history and philosophy of science clarify the relationship between the accepted relativistic formal structure and the various physical interpretations associated with it. The outcome of such debate could, therefore, be the conclusion that 'orthodox' relativity is indeed very fruitful in explaining what we all know it purports to explain and has a particularly sound physical basis of a specific nature. However, the very fact that a conference such as this can be convened some 83 years after Einstein first presented his Special Theory of Relativity suggests that no definitive conclusion of this kind is to be expected. There is clearly a great measure of uncertainty and, in some measure, dissatisfaction with the relativistic theory. Otherwise, we would be content to regard the subject as closed, much as we do with regard to the theory of thermodynamics.

One can approach this subject by asking questions which are conventionally regarded as within the province of relativity and then discussing the conventional response. The issues of the preferred frame or the clock paradox are prime candidates in such a

debate. I have chosen to pose and then anticipate answers to certain questions which are capable of being settled by experiment and have a crucial bearing upon the theory of relativity.

The first question is to ask whether what we know as the electromagnetic properties of charge in motion is a property confined to interactions involving charged leptons. Does all charge in motion behave in the same manner as charge carried by electrons or by positrons? This is a simple question. Put more succinctly, do two protons in motion interact electrodynamically as if they were two positrons? What does the theory of relativity say on this subject? Inasmuch as the theory has no characteristic that distinguishes between leptons and hadrons one expects the electrodynamic forces to satisfy the same formulae in both cases. But is this what is found in practice? Would a magnetic field set up exclusively by a proton beam assert the expected magnetic force on a freely moving proton?

Note here that leptons and hadrons are distinguished by the fact that a lepton cannot sense the strong force, but can sense the electromagnetic force. Leptons do interact electromagnetically with hadrons, but do hadrons interact electromagnetically with other hadrons as a function of their charge motion?

The next question is equally simple. We have certain knowledge that a charged particle such as an electron or a proton moving freely though space will increment in mass as its speed is increased. Such particles are isolated but they may have a definitive quantum-related structure commensurate with the wavelength of electromagnetic radiation as does the matter which they constitute to create atoms. Standing electromagnetic wave resonances associated with photon exchanges that demand an electromagnetic frame of reference can occur in matter on a collective basis and even in the self field of a point or isolated free charge. Suppose that the increment of mass of an element of matter is dependent upon motion of that element relative to the

effective local electromagnetic reference frame. An isolated particle such as an electron cannot be said to move steadily and progressively relative to its own standing wave field, but such a particle can move through the common standing wave system that is shared by a structured charge background.

In a sense, there is an intuitive feeling that creeps into this analysis and suggests that the vacuum background itself may have properties which can physically determine the electromagnetic frame of reference, possibly as a joint venture with matter having an atomic structure but not with a mere isolated electron or proton in passage through it. Note that if energy is conserved when a charge accelerates, then its kinetic energy will augment its mass, but if energy is exchanged by collective association with the vacuum field environment then that mass augmentation need not occur. The question then is whether aggregations of matter, such as neutral composite particles or even the body of the Earth, can really be said to increase in mass as speed is increased. This is a very pertinent question if our objective is to interpret the physical basis of relativity theory. Relativity does not tell us that the phenomenon should depend upon charge or the physical dimensions of the particle in question. Hence there should be no distinction at all, but what does experiment tell us on this question?

Before discussing these two questions in a relativistic context let us consider the third question. This enters the realm of General Relativity, whereas the previous questions concerned Special Relativity. Simply stated, given that a planet moves in an orbit around the sun determined by the distorting effect of the solar mass upon a 4-dimensional representation of Minkowski space, how does energy traverse the distance between sun and planet in the exchanges which occur between planetary kinetic energy and gravitational potential as the planet describes an elliptical orbit? Mathematically, it is easy to say that the planet describes a defined path we term a 'geodesic', but in physical terms we are concerned with the energy transfer and it is natural to wonder what

route this energy takes, what form it adopts and how fast it travels.

It is hoped that the reader will see the gist of this discussion. It is all too easy for us to follow the conventional discussion themes in the debate on the pros and cons of various ways of looking at relativity, but in the final analysis the theory of relativity will have to be judged on its relevance, whether physical or merely mathematical, to phenomena in general. Once we bring in provisos which limit the territory in which relativity can be said to have meaning, we have taxed the theory to its limits and its viability becomes very questionable. One needs, it seems, to hesitate before trying to give physical interpretation to a theory which is still open on crucial issues such as those raised above. All important are the experimental facts and we have yet to determine what these are.

One other question, which we will not discuss in depth but which is mentioned to show that there is so much yet to be resolved, is that posed by the speed of an electromagnetic wave moving freely through space. When an electromagnetic wave is reflected back on itself by a mirror it will have to travel through the energy field of a wave of similar intensity moving in the opposite direction. How do we know that it will move as it would if it were freely moving without being subject to the energy field of such a reflected wave? We assume that the wave velocity is unaffected by the energy field of other waves, but we know that light is slowed down on passage though the energy fields of water molecules, for example. I find reliance on this assumption difficult to comprehend, especially bearing in mind that it is the untested assumption on which the Michelson-Morley experiment is founded and the null result of that experiment is of crucial physical importance to the theory of relativity.

It was after Michelson and Morley reported their experiment that Wiener discovered that standing waves are set up by 180 degree

mirror reflection and that the electric field nodes are locked to the mirror surface. The translational motion of the Michelson-Morley apparatus through space could therefore be forcing the energy in the standing wave oscillations to share the motion of the apparatus. If the wave energy is 'forced' to share that motion, how can we be so sure that the wave velocity is not equally constrained to be referenced on that energy field and so on the apparatus that shares the motion of the earthly observer? If the phenomenon owes its existence to relativistic principles, then those principles must in some way arise from the 'forced' behaviour of the standing wave system. Does relativity involve us in the differences between standing waves and free waves and how does the 'observer' relate to standing waves nodes that lock themselves onto mirror surfaces?

This theme then leads us to ask what happens if a free wave component having no reflected counterpart is superimposed upon a standing wave system having the same frequency. Will the free wave travel at a speed referenced on some other frame, perhaps a preferred frame? This, of course, is easily tested because the free wave would then modulate the standing wave along its length as a linear function of motion of the apparatus relative to that preferred frame.

If we can contend that the standing wave energy, being forced to share the motion of the mirror and so of the apparatus, entrains, as it were, the effective electromagnetic reference frame so far as that standing wave system is concerned, are we to imagine that a different electromagnetic reference frame can operate in the same vacuum region? If so, then what role do we assign to the notional observer in Einstein's theory?

The essential point which emerges from the context of the above questions is that what we know as Lorentz invariance has come from observations on free particles with data derived from measurements in which field energy is entrained by the apparatus. A physical

invariance with translational non-acceleration motion may have a causal dependence upon that entrainment of standing waves with the motion of the apparatus. The Lorentz formulation, with its time transformation condition, becomes a mathematical consequence of assuming that the physical constraints on the free electron or the free wave are identically those applicable to bulk matter and standing waves.

It would seem that not enough is said on this subject in the literature and, having become concerned by this general problem, it becomes very difficult to accept Lorentz invariance as proven. A recent concluding comment by J. S. Bell, speaking to a distinguished scientific audience on the subject of action-at-a-distance in quantum mechanics, was that Lorentz invariance has 'become very problematic' and that 'an ether would be the cheapest solution' [1]. This really guides one to focus attention upon the issue of the preferred frame rather than paradoxical features of relativistic time dilation.

There are still so many crucial questions to be answered that it seems to the author to be somewhat premature to search for physical justification for the formal features of the theory of relativity until the answers are known.

In spite of this we can reach some conclusions on interpretation, and concerning the first question above, if we examine how relativity applies to the simple electrodynamic interaction between two parallel wires carrying electric current in the same direction and both moving in that same direction.

The observer sees positive electric charge carried one way by the atomic lattice of the wire substance and an equal negative electric charge having two components of motion, one corresponding to it being carried along with the atomic lattice and the other corresponding to the electric current involved. The force between the two wires arises from the mutual interactions of all these

charge motions. From experiment we know that the mutual
interaction force is proportional to the product of the two current
strengths. It seems to have no dependence whatsoever on the motion
of the wires themselves, though one cannot be sure, because any
second-order functions of that motion in relation to the speed of
light would hardly be apparent in a normal experiment.

The force measured, being only a function of current and not a
function of motion relative to the observer, can, on the face of
it, hardly be said to exhibit any dependence upon relativistic
interpretation. Yet we know from Einstein's 1905 paper [2] that
electrodynamics is at the very heart of the theory of relativity.
Furthermore, we are in a position to say that, because the observer
has a position of rest in the relevant inertial and electromagnetic
reference frame, according to relativity, we could specify that the
motion of the wire exactly cancels the component of motion
corresponding to the electron current. The observer is then
looking at current interaction that is exclusively attributable to
the charged ionic lattice of the wire substance and not to the
action of free conduction electrons. Relativity, therefore, makes
no distinction between the forms of the charge carrier. A
negatively charged ion of atomic size is seen as setting up the
same electromagnetic actions as an electron moving at the same
speed.

Suppose now that we were to argue that the ionic lattice of the
wire conductor locates the determining electromagnetic reference
frame for electromagnetic actions generated in that wire. It may
well do this owing to the internal exchanges of energy by thermal
radiation and their effective standing wave resonances, or this may
arise from the sheer dominance of the electromagnetic energy fields
within the atomic systems. This ignores relativity completely.
The only external electromagnetic action that could then arise in
the normal experiments is that due to the conduction electron
motion. The overall effect is just the same as that based on
relativity theory, but we need not be concerned in this case about

how currents produced by heavy ions interact electrodynamically.
It is only if they are freely moving in isolation from a lattice to
which they are bound that we then need to think of their role in
producing magnetic fields.

The thrust of this argument will be understood when we note that
there is reason to believe (see analysis below) that two heavy ions
moving freely will not interact in precisely the same manner
electrodynamically as will two electrons. If this is so, and if it
were to be confirmed by experiment, then the theory of relativity
faces a problem. This is the basis of the question whether
hadrons, being non-leptons, interact electrodynamically as do
leptons, the electron being a lepton.

To justify this position, consider the energy potential
expression:

$$W = (1/2rc^2)(V)^2 \qquad (1)$$

where r is the separation distance vector between two unit electric
charges, c is the speed of light in vacuo and V is the relative
velocity of the two charges. This is a classically-derived
expression dating from the nineteenth century. It relates to the
electrodynamic component of the interaction between the charges.

Supposing that this formula has a valid physical basis, how can
it possibly correspond to an action which sets up a force
proportional to the product of the two charge velocities as
referenced on the relevant inertial frame of reference?

The answer is also provided by classical theory. We must regard
the electric current of at least one of the charges as involving
the equal counterflow of both positive and negative charge. One of
the unit charges used in equation (1) must comprise two component
charges of value +1 and -1 moving in opposite directions relative
to their electromagnetic reference frame.

To formulate this, let this two unit charge elements be effective as a unit charge moving at velocity v in the electromagnetic reference frame. Then the +1 charge component will have a velocity +v/2 and the -1 charge component will have a velocity -v/2. Together, they will produce the same magnetic field as the single charge moving at velocity v. However, when brought into the equation (1) the effect is totally different.

We then have two potential components. The first, based on the +1 charge moving at +v/2, will have a relative velocity V given by (v/2-u), where u is the velocity of the interacting charge referenced on the same electromagnetic reference frame. The second, based on the -1 charge moving at -v/2, will have a relative velocity V given by (v/2+u). When these two V values are squared and put in the two potential expressions, which are then combined, we obtain the simple potential formula:

$$W = -(1/rc^2)(v.u) \tag{2}$$

This has the required form, being dependent upon the product of the two charge velocities and being derived from a 'relativistic' expression brought into the framework of a proper reference frame. This expression is the basis from which one can deduce the Lorentz force law [3].

However, as may be seen from its derivation, the relativistic expression has only served to justify the potential of equation (2) because we have admitted that a physical process is occurring causing one of the currents to be developed by countermoving charge pairs having a symmetrical countermotion in a specific frame of reference. Unquestionably, this suggests that an electron in motion is really characterized by electron-positron pair creation in the vacuum field ahead of the electron, followed by countermotion of the positron and the original electron, which come together in an annihilating encounter.

Hence we do face the possibility that the form of equation (2), which is basic to the Lorentz formulation, owes its origin to a

classical relativistic expression for electrodynamic potential, namely equation (1).

Moreover, we are alerted to the possibility that only particles capable of exhibiting the pair creation and annihilation properties can possibly be effective in the electrodynamic sense. Yet the formal theory of relativity fails to make any distinction on this score, owing to its empirical dependence upon electron interactions.

It is submitted, therefore, that the best way of approaching the physical interpretation of relativity theory is to continue to pose new questions that can be tested experimentally. Acceptance of the theory of relativity forces us to accept that protons, for example, must migrate by involving charge pair creation and annihilation. If that is feasible to the nuclear physicist, then relativity can hold up. Otherwise there is a problem.

Evenso the author confesses unease about interpreting the relevant electromagnetic reference frame as that in which particle pair creation and annihilation occur, bearing in mind that relativity requires this frame to be that of the chosen observer. Whether or not particles exist and where they happen to be seems a question of fact rather than illusion dependent upon the mobile observer. This is one of the open issues left for resolution elsewhere.

The relativistic increase in mass is believed to occur as a function of motion relative to the 'observer', but the experimental evidence only relates to isolated charged particles (not even atoms) in motion relative to an electromagnetic frame of reference that is usually that of the accelerating apparatus. The question that arises here is whether the Earth or planet Mercury experience any variation of mass owing to their variable speed in elliptical orbit around the sun. The change in mass need not affect the force balance as between gravitational action and centrifugal effects,

but it certainly will affect the angular momentum in orbit unless the velocity moment varies so as to assure constancy of angular momentum. This is a topic that is not adequately considered in the textbook treatment of relativity, the real point being whether we are concerned with a physical reference frame for the electromagnetic phenomena or an arbitrary 'observer' frame. See reference [4] for further comment on this problem.

In trying to clarify the uncertainty in this one is also confronted with the issue of how energy is transferred between sun and planet. If, for example, the planet describes a true circular orbit relative to the sun as centre, then we know that no energy is being exchanged as between the gravitational potential and the kinetic energy of the planet. However, if there is a small superimposed radial oscillation as there is for an elliptical orbit, then such energy transfer must occur. Does this energy then travel at the speed of light along a thin line drawn between the sun's centre and the centre of the planet? If it does there will be some retardation of the action and the radial oscillations will be slightly slower than the orbital cycle. This means that there will be a progressive advance of the perihelion, quite apart from any that is determined by the 4-space character of the basic geodesic path. How, then, is this allowed for in Einstein's theory?

This is a particularly relevant point, because Paul Gerber [5] in 1898 derived the 43 arc sec. per century perihelion advance of Mercury on exactly this assumption. His analysis was in error by a factor of two-thirds, but the 43 arc sec. value could still apply if the energy exchange at the speed of light were to be along curved energy flux lines. This is easily proved [6] and it is manifestly more logical to have energy flowing around flux contours rather than concentrating itself into a thin straight path, as Gerber assumed.

Without wishing to criticize the orthodox relativistic treatment, it nevertheless seems to be fair comment to ask why Gerber's basic proposition is ignored. Though he made an error in his analysis, that error did not nullify what he said from a qualitative viewpoint and a quantitative correction still left a perihelion advance that is ignored in presenting the General Theory of Relativity.

The fact that Gerber's work is ignored has to mean that the theory of relativity does not concern itself with the retarded transfer of energy in the gravitational field. This, of course, is a very interesting point when we are considering physical interpretations.

It is, incidentally, of interest to take note that Gerber's 1898 treatment of the perihelion motion of Mercury was the subject of a further paper [7] expressly published in the same journal as Einstein's famous General Relativity paper shortly after that appeared. Gerber's physical explanation for the anomalous perihelion advance is firmly of record in a contemporary association with the Einstein paper and attracted rapid feedback in the same journal, so there is no justifiable excuse for it being ignored by science philosophers and historians.

The recent Aspect experiments [8] that support action-at-a-distance effects in quantum mechanics have given impetus to the idea that force asserted by a field could be an action of non-retarded potential in both gravitational and electrodynamic interactions. Then, when two interacting particles are in relative motion, there is energy redeployment which causes some of the potential to act from a field position that is a function of the motion. The energy in transit will neither be the primary potential nor the kinetic energy of the particles, but it could assert action-at-a-distance effects on a par with potential energy but from its distributed position. In other words, the distortion of the space metric that we see as the 4-space of Einstein's theory

when modified by the presence of matter could represent the
secondary potential action of this dynamic energy. It has recently
been shown [9] that such argument leads to a derivation of equation
(1) when applied to electrodynamic potential and, when applied to
gravitational potential, to a modification of Newton's law of
gravitation to bring it into conformity with the law derived from
Einstein's Theory of General Relativity. The Gerber theory can
therefore be seen in a more formal context in this recent paper
[9].

From what has been said, it is evident that physical
interpretation of relativistic methods must take account of energy
transfer processes. This theme brings us to the point already
mentioned concerning how standing wave energy might affect the wave
velocity of the standing wave components in a way that differs from
the free wave. An experiment in which such a free wave is
superimposed upon a standing wave system having the same frequency
and being generated from the same laser has been reported [10, 11].
The object of the experiment was to scan a detector along the
standing wave to see if it was subject to any amplitude modulation
that could betray a velocity difference. In theory, and without
regard to relativistic considerations, an anisotropy in the
modulation pattern as the apparatus is rotated should then afford a
measure of the speed at which the apparatus moves relative to the
frame of reference governing the free wave velocity. In practice,
the initial reports on this U.S. Air Force sponsored experiment
suggest that such anisotropy has been observed. If this finding
holds up it will inevitably affect the ways in which we can
interpret relativity theory, especially as the evidence points to
the existence of the preferred frame. Invariance as we have come
to know it may become a feature of actions in systems governed by
standing wave resonance, such as apparatus involving oscillations
within cavities, whether these are natural field states in physical
systems or man-made devices.

The conclusion to be drawn from this discussion is that the theory of relativity has yet to settle the basic question of the role of the 'observer' in relation to the role of a physical electromagnetic frame of reference. That physical frame has presumably some connection with the events of lepton pair creation and annihilation, but the 'observer' seems enigmatic in that connection. More clarifying experiments are urged in the hope that answers can be found to the specific questions which have been raised. Also one should really come to terms with the Gerber's classical explanation of planetary perihelion advance before theorizing with total reliance on the Einstein derivation.

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MARINOV'S COMMENTS ON THE PREVIOUS PAPER BY H. ASPDEN

In his article, Dr. Aspden puts many questions but gives few answers. We know well that the good *belle littérature* is this one which puts questions and not this one which gives answers(remember the tragic degeneration of the literary method called "socialist realism" when ᵒⁿᵉbegan to give answers). But physics is not *belle littérature*.

On the other hand, however, the right answers in physics are to be given not by the physicists but by Nature itself. We have only to put to Nature <u>clever</u> questions and present our <u>predictions</u>, the right answers will then come <u>automatically</u>.

A <u>clever</u> question is this one which can be <u>experimentally</u> answered (the simpler the experiment, the better the question!) and which is <u>alternative</u>, i.e., for which two rival theories predict different issues, so that the experiment can reject one (or both) of the proposed theories. Thus, after all, the good physicist, as the good fiction-writer, is this one who can pose to Nature good questions.

Unfortunately, I could not find in Dr. Aspden's paper many clever questions and I did find either a single prediction.

I shall give my comments to Dr. Aspden's paper choosing some of the most important of his questions (some questions are commented on two or more places in the paper).

1. WHICH IS THE ROLE OF THE 'OBSERVER'?

Fr. Nietzsche proclaimed at the end of the XIXth century: "Gott ist tot." As we do not know whether God was living before, the assertion of Nietzsche is pretty bombastic. However, now, after the restoration of the absolute space-time concepts, and after having established that all physical phenomena depend on the absolute velocities of the particles, we can safely assert: "Der Beobachter ist tot (the observer is dead)." Thus <u>nothing</u> depends on the observer. All physical effects are <u>independent</u> of the observer.

The electromagnetic phenomena are to be described (if one wishes to make predictions which then will be confirmed by the experiment) by the absolute velocities of the charged particles. One can choose one's reference frame either in absolute space or in the moving laboratory, but the equations with which one has to operate in these two cases are different: in the first case the absolute Newton-Lorentz equation is to be used, and in the second the relative Newton-Lorentz equation. In both cases the predicted effects will be exactly the same.

As the nodes of a standing light wave in the laboratory are solid to the laboratory (and move with its absolute velocity V in absolute space), Dr. Aspden asks whether this standing wave does not define an "electromagnetic reference frame seated with and moving with the apparatus". Of course, one can accept that the nodes of a light standing wave define a reference frame solid to the laboratory, but this frame is substantially different from the frame defined by a light standing wave produced in a laboratory which is at rest in absolute space. Why? - Because in the latter the maximum illuminations at the nodes appear at the <u>same</u> moment, while in the former at <u>different</u> moments.

2. WHAT WAS WRONG WITH THE IMPERIAL-COLLEGE-CONFERENCE?

The comments which Dr. Aspden gives concerning the conference "Physical Ineterpretation of the Relativity Theory" are, of course, right. In no way can this conference be called "scientific". The fact of my violent expulsion by three men of the security staff, under the order of the conference's organizer, Dr. M. Duffy, is sufficient to disqualify the conference. When the proponents of a certain theory become afraid to hear unpleasant questions, this theory has begun to stink.

3. IS THERE A DIFFERENCE IN THE ELECTROMAGNETIC BEHAVIOUR OF ELECTRONS AND PROTONS?

Dr. Aspden asks whether there is a difference in the electromagnetic behaviour of leptons (electrons) and hadrons (protons). As he poses his question retorically but not to Nature (there is no proposal of some experiment), the question remains void. At the present time I do not know some experiment which has indicated ^{at} such a difference.

4. IS THERE MASS INCREASE FOR MACROSCOPIC BODIES?

This is again a question without proposal of some experiment. I have considered this problem in my CLASSICAL PHYSICS, vol. IV, p. 196, where I have proposed the following experiment:

> Let a rotor, with a moment of inertia J about its axis of rotation, be in equilibrium with an equivalent mass on a very sensible ballance. If we set the rotor in rotation with an angular velocity Ω, its proper energy will become
> $$E_o = E + E_k = Mc^2 + (1/2)J\Omega^2,$$
> where M is its mass, E its universal energy, and E_k its kinetic energy.
>
> Thus its mass which plays the role of "gravitational charge" will increase by
> $$\Delta M = J\Omega^2/2c^2.$$
> Taking $J = 1.8 \times 10^5$ g cm^2, $\Omega = 100,000$ rad/sec $\cong 16,000$ rev/sec, we obtain $\Delta M = 1$ µg. This is such a small mass increase that it can hardly be measured. However this "ROTOR ON A BALLANCE" EXPERIMENT shows that with the help of certain experimental sophistications, perhaps, soon one will be able to establish experimentally in the most direct way whether there is a mass increase with velocity for macroscopic bodies.

I firmly believe that there is mass increase with velocity increase for macroscopic bodies. As an experimental verification can be considered the seculiar perihelion's rotation of the planets, for which my theory gives a value equal to the half of that given by general relativity (but general relativity does not take into account the Sun's oblateness and the increase of its density towards the center which cause perihelion's rotation in the same direction and of the same amount as this one caused by the mass increase). As, however, too many factors (certain of which are not known exactly) influence the perihelion's rotation, the relevant observations cannot give convincing support (or rejection) to the different theories (see for detail CLASSICAL PHYSICS, IV, §63).

I have further to add that in the expression for the proper mass

$$m_0 = m(1 - v^2/c^2)^{-1/2},$$

where m is the universal mass, i.e., the mass of the particle when it is at rest in absolute space, one has to take ALWAYS the absolute velocity of the particle and NOT its laboratory velocity, as conventional physics does (see CLASSICAL PHYSICS, vol. III, §44).

5. CAN KINETIC ENERGY BE TRANSFERRED FROM ONE BODY TO ANOTHER?

According to my concepts kinetic and potential energies are things which we write on pieces of paper. It is senseless to pose a question whether kinetic energy can be transferred from one body to another and to try to locate potential energy. The transformation of potential energy into kinetic energy is a purely mathematical operation. Our last experiments violating the law of energy conservation (the ball-bearing motor - TWT-II, p. 101, the machine ADAM - TWT-II, p. 324, the machine MAMIN COLIU - TWT-III, p. 84, the machine TESTATIKA - TWT-V, p. 8) showed that energy can be created from nothing. Thus energy is NOT a thing, energy is only a notion.

6. IS AN AETHER THE CHEAPEST SOLUTION?

After so many experiments which I have done with light in my life, I came to the conclusion: light kinematics is one of the most simple topics in physics. I wonder that humanity has spent so much time to ruminate over the essence of light kinematics. And after the statement of J. S. Bell cited by Dr. Aspden "An aether would be the cheapest solution", I shall add: "An aether is not only the cheapest but the only possible solution".

7. WHICH IS THE POTENTIAL MAGNETIC ENERGY OF TWO CHARGES?

Dr. Aspden accepts as magnetic potential energy of two charges q_1, q_2, moving with velocities v_1, v_2, the following expression

$$W = q_1 q_2 (v_1 - v_2)^2 / 2c^2 r,$$

deduced first by Riemann and now sustained by   some   ·   supporters of Ampere's formula for the interaction of two current elements. Meanwhile the magnetic energy of the above two charges, where v_1, v_2 are their ABSOLUTE velocities, is

$$W = q_1 q_2 v_1 \cdot v_2 / c^2 r,$$

as it is accepted AXIOMATICALLY in my absolute space-time theory. Only proceeding from this expression for the magnetic energy of two charges, I could obtain the relative (and   absolute) Newton-Lorentz equation and then predict the results of the inertail Kennard experiment and of the inverse Rowland experiment. The results of these experiments carried out then by me confirmed MY predictions. Meanwhile the predictions of the relativity theory are DIFFERENT. Dr. Aspden knows very well the results of these two experiments reported, respectively, in TWT-IV, p. 110 and TWT-VII, p. 325. I think that before to throw questions in the air, Dr. Aspden has first to give his comments on the experiments carried out by me which demonstrate without any ambiguity that the relativity theory is DEAD.

My expression for the magnetic energy of two charges is absolute and thus contradicts the principle of relativity, while Riemann expression is RELATIVE and it is in concordance with the principle of relativity. Thus if absolute effects (Kennard's experiment) and non-reciprocal effects (direct and inverse Rowland experiment) have been observed, one must by force reject Riemann expression.

8. WHICH IS THE BEST WAY OF APPROACHING THE PHYSICAL INTERPRETATION OF RELATIVITY THEORY?

Dr. Aspden asserts: "It is submitted that the best way of approaching the physical interpretation of relativity theory is to continue to pose new questions that can be tested experimentally."

First, this is not the "best" way of "approaching the physical interpretation" of relativity theory, but this is the ONLY way of exploring physics (see item 1).

Second, when there are enough underlined unambiguous experimental invalidations of a given theory it is not necessary to search for other invalidations (or confirmations). The invalidating experiments are to be made largely known, and the theory in question will then DIE AUTOMATICALLY.

9. IS "INTERACTION" PROPAGATING?

This is an eternal question, as is the question: "Is there a God?" As until the present day nobody has demonstrated experimentally either the existence of God or the existence of "propagation of interaction", the answer can be only one: "There is neither God, nor propagation of interaction". And I should like to note once more: the so-called retarded (or advanced) potentials are NOT the potentials at the retarded or advanced moment, but the potentials at the moment of observation. The advanced and retarded potentials are numerically EXACTLY EQUAL ONE TO ANOTHER and are defined by the velocity of the charge and its distance to the reference point at the moment of observation, as

$$r = r'(1 - n'.v/c) = r''(1 + n''.v/c),$$

where r' is the distance at the advanced moment (wrongly called retarded)

$$t' = t - r'/c,$$

r" is the distance at the retarded moment (wrongly called advanced)

$$t'' = t + r''/c,$$

r is the distance at the observation moment t, n' is the unit vector (from the charge) along the advanced distance r', n" is the unit vector along the retarded distance r" and v is some MIDDLE velocity between the advanced and retarded moment, so that if moving with this middle velocity during the time t" - t' = (r" + r')/c the charge will cover the shortest distance between the advanced and retarded   positions. For more detail see CLASSICAL PHYSICS or at least TWT-IV, p. 30.

10. IS SILVERTOOTH'S EXPERIMENT CONCLUSIVE?

At the end of his article Dr. Aspden dedicates few lines to Silvertooth's experiment.

It is to be noted that there is no relativist all over the world who has commented on Silvertooth's experiment. As small children are afraid to enter dark rooms, so the relativists evade to discuss any experiment where one pronounces the word aether. I wonder whether somebody other than Dr. Adpden has mentioned Silvertooth's experiment at the Imperial-College-conference.

We have been with Dr. Silvertooth and Dr. Aspden at the conference GALILEO BACK IN ITALY   in Bologna and Perugia in 1988 (Dr. Aspden has visited only the Bologna conference). Both these conferences have been dedicated primarily to the discussion of Silvertooth's experiment. And until now it is not clear to anybody of us (including Silvertooth) what Silvertooth has really done. The three Silvertooth's descriptions of the experiment (NATURE, **322**, 590 (1986), SPEC. SC. TECHN., **10**, 3 (1987), ELECTRONICS & WIRELESS WORLD, May 1989, p. 437) are mutually contradicting and pretty enigmatically written.

I carried out a simplified variation of Silvertooth's experiment (SPEC. SC. TECHN., **12**, 187 (1989)) to see whether there is a difference in the nodal spacing in the "Silvertooth's standing wave" and in the "Michelson's standing wave" (in the first standing wave two DIFFERENT light beams interfere, while in the second one   the same light beam does interfere with itself (to and fro going light)). I established that there is NO difference in the nodal spacing (first, because of the non exact parallelism of the light rays in my experiment, I registered differences in the signals which I wrongly attributed, following Silvertooth, to the absolute velocity of the laboratory).

In his last publication Silvertooth asserts that he is measuring not different distances in the two standing waves (he consented that such differences do not exist) but he proceeds from the fact   (in MY simple and CLEAR interpretation) that the maximum illumantions at the different nodes of the light standing wave appear at different moments. This effect, according to me, is EXISTING, but I do not see a possibility for observing this effect with light waves because of their short periods and I proposed (TWT-VII, p. 331) to try to observe this effect with wave-guides. For me Silvertooth's method for revealing this effect (which is the effect SAME for the Silvertooth's and Michelson's standing waves) is not clear. If he has succeeded to register THIS effect, he is a genius.

Now Dr. Aspden writes that according to his interpretation, in Silvertooth's experiment "a free wave is superimposed upon a standing wave system having the same frequency and being generated from the same laser". I do not see in Silvertooth's experiment an interference between a "free wave" (i.e., light propagating in one direction only) and a "standing wave" (i.e., light propagating in two exactly opposite directions). I shall be very glad if Dr. Aspden will give his interpretation of Silvertooth's experiment (including his own PREDICTION) and I shall gladly publish his paper in TWT-IX.

I think that it is time for the world to give the answer: Has Silvertooth measured indeed the Earth's absolute velocity? - I firmly sustain the opinion that he has not, as according to me his experiment is inconclusive but I shall do (as I have already

done) any efforts so that the mystery around Silvertooth's experiment should be cleared, as all his publications (Dr. Aspden refers to the first two only), as well as his two speeches in Bologna and Perugia were pretty enigmatic.

Silvertooth has submitted reports on his experiment to the leading physical journals which have been rejected (I know about the submission to the editor of PHYSICS LETTERS A, Prof. J. P. Vigier). Prof. Vigier was invited to the Bologna-Perigia conferences. He has accepted the invitation and Dr. Monti, the organizer of the conferences, has sent him the ticket. Prof. Vigier did not come to Bologna under the pretext that the ticket has been delivered to him a day later.

I propose that a petition should be signed by Monti, Bartocci, Aspden, Wesley, Pappas, Graneau, Marinov, etc., submitting it to Prof. Vigier and suggesting that he publishes a WELL-WRITTEN report of Silvertooth in PHYSICS LETTERS A.
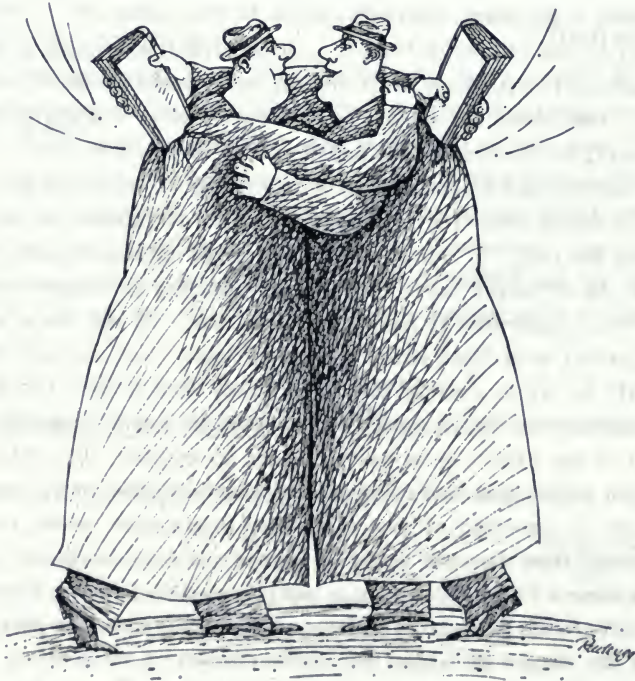
SPECIAL RELATIVITY AND OTHER IRRATIONAL BELIEFS

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Why is "special relativity", which is so obviously irrational, accepted? We live in a world bursting full with irrational beliefs, such as: baptism, astrology, holy communion, prayers to Allah, Bridey Murphy, psychoanalysis, communism, extrasensory perception, spiritualism, immaculate conception, hell, capitalism, seances, etc., etc., etc. Since "special relativity" is just another such irrational belief; we must first ask: Why are irrational beliefs in general so common and so firmly believed by large groups of individuals? A typical isolated individual all by himself is ordinarily quite rational. But put such an, otherwise rational, individual together with other such individuals, they soon generate irrational common group beliefs. Why? The answer, I feel lies in the evolutionarily selected instincts of man. Primitive man, as a social animal, evolved in small groups or villages capable of fighting off neighbors to maintain a territory large enough to support the survival of the group or village. A close coordinated action of the members of the group or village was necessary to effectively fight territorial battles as well as for other activity such as hunting large game. The immediate selfish goals of an individual had to be firmly suppressed to be replaced by group goals. An individual had to be able to sacrifice even his own personal existence, if necessary, for the group. The large big-brained clear thinking individualistic Neanderthal probably became extinct at the hands of the puny small-brained Cro-Magnon man of today, who was apparently able to live in larger more effective groups with each individual sacrificing more of his own individual rational beliefs for irrational group beliefs.

Evolutionary selection that allows group beliefs that permit more effective concerted group action would seem to be obvious. But there is still the question: Why are group beliefs so often, if not usually, irrational? The answer here is again clear, it seems to me: There must

be some group beliefs held by the group or village which can distinguish the particular group or village from its neighbors. Such labelling beliefs need not be rational; and the more irrational the more effective is the differentiation. In addition, there must be group beliefs that distinguish an individual who is willing and capable of acting in concert with the group from the individual who is not willing or not capable of acting in concert with the group. Such group beliefs must immediately reveal the individual thinker, who will not act in concert with the group. Such an individual thinker can endanger the group. He must be coerced into accepting group beliefs or be killed or banished. Thus, a group belief that is to guarantee the subservience of the individual to the group must be irrational. It must be a belief so outlandish that no isolated rationally thinking individual could possibly entertain such a belief. For example, Christ rose from the dead. No isolated normal rational individual would swallow such a silly idea. But hundreds of thousands of individuals denying such institutionalized irrationality have been tortured and burned at the stake. They were not deemed as being sufficiently subservient to the group will. The role of irrational beliefs held by large groups of individuals can be, thus, explained (although such beliefs cannot, of course, be recommended nor condoned by a rational individual thinker).

Such irrational group beliefs are frequently encapsulated in a few words, constituting a dogma that is obviously irrational and absurd. For example, God is all powerful. No one knows what the word "God" means; nor can anyone determine if "God" exerts any power or not. In "special relativity" one has: The velocity of light is constant with respect to the moving observer. Any 5 year old child recognizes the fact that the velocity of nothing at all can be independent of the velocity of the observer. Or another dogma from "special relativity": The absolute velocity of the laboratory cannot be measured. This is an outrageous denial of the known observations of Roemer, Bradley, Sagnac, Conklin, Michelson-Gale, and Marinov. It violates common sense; we live in only one universe and the laboratory must move with a unique or absolute velocity with respect to this one and only universe. This irrational dogma makes it easy to distinguish the heretic from the true believer, or the individual capable of thinking for himself from the subservient member of the physics establishment.

It is of interest to note how such irrational group beliefs are ordinarily presented and promulgated. Such beliefs are wonderful, beautiful, exalted, eternal, and sublime. They are devine truths. They shine with an irresistable inner light. One has only to believe in them and all of the goodies of this and the next world will become available. one has only to believe to experience a wonderful feeling of joy and inner peace. These ridiculously exaggerated positive claims are accompanied by equally ridicoulously exaggerated threats of hell and damnation (often merely implied or veiled) in case one were not to believe. If the beliefs were merely matters of fact, no such irrelevant social pressures would be needed at all. But the beliefs, being discussed here, are irrational and are not matters of fact. They serve a social function only, where social pressures are to be expected. Authors writing pro-"relativity" papers typically digress to stand a moment in reverential awe of the great power, the equisite simplicity, and the universal acceptance of their beliefs. They eagerly reveal the secret that "relativity" gives all of the answers (to forestall some unindoctrinated individual who might have an embarrassing question). It is always pointed out that "relativity" fits all experimental results (in case some heretic might wish to point out one of the many experiments where it fails). It is rapturously presented as amazingly beautiful (in case some individualist might happen to see it as the ugly deformed creature it is). The lavish praise heaped on "relativity", the publicity and propaganda, and all of the rest of the hullabaloo would, of course, be quite unnecessary and out of place, if "relativity" were a rational theory based simply on fact. The incessant campaign to make everyone believe in "relativity" is not benign. It is an irrational social movement to coerce conformity. Ordinarily no one denying "special relativity" gets a professorship, a prize, a contract, tenure, a paper published, or can remain in the billion (10^9) dollar particle accelerator business.

Rationally it may be readily recognized that "special relativity" is just another fad like dyanetics, Bridey Murphy, table tapping, or the Great I Am. One may expect the craze to die out. But before it withers away and is finally forgotten (like dyanetics, Bridey Murphy, table

tapping, and the Great I Am), it is best to ignore it rather than to confront it head on. I have never been able to convert a Catholic to atheism; I never expect to be able to convert a true believer in "special relativity" to science.

It might at first seem strange that physics, which is presumably dedicated to <u>rational</u> beliefs, could be afflicted with <u>irrational</u> beliefs. Unfortunately, <u>all</u> social institutions, implying group effort and, therefore, group beliefs, can be afflicted with irrational beliefs used to force their members into concerted group action. Physics has grown suddenly since 1900 into a huge world-wide social group, which brings with it the irrationality of group beliefs. I am afraid that only the few rare physicists, who remain independent of the physics establishment and can think individually, will be able to advance physics as a science in the future. One should not feel that physics is unique in being especially afflicted with irrational beliefs. Most fields of study, such as sociology, economics, political science, chemistry and medicine, are in far worse shape. Group effort implies, unfortunately, the evolution of possible irrational group beliefs.

One might ask if irrational group beliefs have any pattern. Over the centuries one might conclude that such group beliefs will rarely interfere with "business as usual". Thus, a religion would not be expected to arise which would prohibit the harvesting of wheat in a grain eating community. The belief in the immaculate conception does not interfere with the Wall-Street markets. Unfortunately, however, the irrational beliefs are sometimes found to disturb a community's fight for survival. The <u>holy cow</u> shall not be milked!! Such beliefs on occassion can lead to the extinction of a community. The benign aspect of irrational group beliefs, while perhaps the rule, cannot be relied upon in general. There appears to be no pattern for irrational group beliefs; as might almost be inferred from the meaning of the word "irrational". Never-the-less, one form of irrationality seems to occur very often. It involves switching the physical roles of an independent and a dependent variable. For example, the cock Chanteclaire caused the sun to rise every morning by crowing; because every morning he crowed and then, sure enough, the sun rose. A Methodist farmer, knowing the efficacy of prayer, prayed for rain. It rained the next day; so the farmer knew he had caused the rain to fall through his prayers to God.

If it is found in the laboratory that y is a function of x and equals x^2, then the typical naive mathematician would say that x is then a function of y and equals $\pm\sqrt{y}$. But in the laboratory y is also found to be a function of an entirely different physical variable z and equals z^2; so clearly not both x and z can always be functions of y equal to $\pm\sqrt{y}$. One must know the precise physical situation before the concepts of <u>function</u>, <u>independent variable</u>, and <u>dependent variable</u> have any clear meaning. The differentiation between <u>independent</u> and <u>dependent</u> variable is a vital characteristic of the physical circumstance. The differentiation is not a mathematical one.

The primitive variables of physics, the basic <u>independent</u> variables of physics are space and time. To act as independent variables space and time must be <u>unique</u>, i.e., they must be <u>absolute</u> (as stressed by Newton). One is not permitted to view space and time as <u>dependent</u> variables. Space and time cannot be considered to be functions of velocity, one's particular point of view, the frame of reference, or any other such arbitrarily chosen parameter. One's primitive <u>independent</u> variables must remain exactly that: primitively <u>independent</u>. Velocity is a <u>dependent</u> variable defined as a function of the two independent variables space and time, the distance divided by the time to travel the distance. It is completely schizophrenic to try to invert things and to turn things around backwards by saying that space and time are <u>dependent</u> variables which are functions of the <u>independent</u> variable velocity. If space and time were to be dependent variables rather than independent variables, then they would no longer be unique or absolute. As in the irrational "special relativity", after appropriate round trips, twins can each be older than the other; "simultaneity" can be "nonsimultaneous"; causes can be effects; etc.

Rational independent thinkers should forget "special relativity". There are <u>real</u> problems in physics that need attention.

DIALOGO SUI MASSIMI SISTEMI

di paolo brunetti e roberto monti

(Telefono).

P. Sì.

R. Sono io!

P. Io chi?

R. IO!!! Stai dormendo?

P. Sì.

R. Sono appena tornato da Parigi: notizie clamorose e sconvolgenti...
Ma se vuoi dormire te le dico dopo.

P. Momento! Che or'é?

R. Le sette.

P. Dove sei?

R. In Istituto.

P. Devo alzarmi alle sette e mezza...

R. Allora faccio colazione e poi vengo.

P. Va bene.

(studio)

P. Allora?

R. La Relatività é crollata.

P. Spiegati.

R. Dunque. Sta a sentire. Ricordi che ti avevo parlato di *Vigier*?

P. Chi é?

R. Quello di Danzica, che dopo i miei seminari si era dichiarato disponibile a pubblicare due lettere su Physics Letters. E' l'Editore. Bene. Io le ho scritte e poi sono andato da lui per discuterne con calma.

P. E come é andata?

R. Un capolavoro di diplomazia, del quale, scommetto, non mi avresti creduto capace.

P. Puoi giurarci. Tu diplomatico!... Un elefante al confronto é come una farfalla!

R. Invece era il caso, e lo son stato. Innanzitutto ho cominciato col lasciarlo parlare, come se io fossi andato a Parigi al solo scopo di ascoltarlo. E solo verso sera, prima di accomiatarmi, ho buttato lì casualmente, con molta discrezione, che magari il giorno dopo, nei ritagli di tempo, avremmo potuto anche dare un'occhiata alle mie lettere.

P. Notevole! Non ti riconosco.

R. Sto cambiando. Il giorno dopo ho ascoltato attentamente l'elenco dei motivi per i quali le mie ipotesi potevano essere una conferma delle *sue* teorie: una variante della Teoria della Relatività con tanto di Etere Covariante e di fotoni dotati di massa a riposo non nulla.

P. Cos'é sta storia della "massa a riposo"?

R. Tu hai mai visto il mare in bonaccia?

P. Certamente.

R. Bene. La massa a riposo dei fotoni é la massa delle onde del mare in bonaccia.

P. Ma quando il mare é in bonaccia non ci sono onde!

R. Sono d'accordo, ma tu vaglielo a dire. Io comunque ascoltavo finché lui a un certo punto ritenendo, suppongo, di aver sufficientemente accertata l'onestà e la purezza delle mie intenzioni, decide che sono pronto ormai a trarre il giusto profitto da due cattivi esempi. *Silvertooth*: mai sentito? No. *Marinov*? Neppure. Bene. Costoro pretendono, così d'un sol colpo, senza sfumature, di aver distrutto la Relatività! Reprobi, indubbiamente. E dove? Ecco qua il *Marinov*, per incominciare. *Silvertooth* non lo trova.

P. E tu che hai fatto?

R. Era tardi, ho preso su, dicendo che ci avrei guardato in serata. Ne avremmo parlato il giorno seguente.

P. E allora?

R. C'ho guardato e a momenti m'é venuto un colpo!

P. Perché?

R. Si trattava dei due tempi, quello di andata e quello di ritorno, di un raggio di luce tra due specchi.

P. Cerca di essere più chiaro.

R. Non adesso. Per adesso ti basti che questi due tempi, secondo *Einstein* eguali, devono essere invece diversi, e che, di conseguenza, di essi si può fare prima la somma e poi la differenza. Io ho cominciato a lavorare anni fa, nel '68, con tutt'e due, senza venirne a capo. Poi, nell' '82, di nuovo con la somma. E questa volta con tale successo da non pensare più alla differenza: l'ho appena indicata, nel primo numero di Seagreen, e poi l'ho trascurata.

P. E allora?

R. E allora *Silvertooth* e *Marinov* hanno lavorato con la differenza, e ce l'hanno fatta!

P. A far che?

R. A misurare la velocità assoluta della Terra, ovviamente.

P. Ovviamente un c...

R. Basta così! Comunque é chiaro, e poi ti spiegherò. Il giorno dopo ho spiegato la cosa al *Vigier*, chiaramente. Lui si é preoccupato di sapere se c'era qualche speranza per la massa dei suoi fotoni. Forse. Ma innanzitutto l'etica professionale prescriveva un immediato sopralluogo.

P. E cioé?

R. Andare a Graz, da *Marinov*, a controllare.

P. E lui?

R. Mi ha nominato seduta stante, con lettera autografa, Referee di Physics Letters A, incaricato Ufficialmente dell'Inchiesta. Io l'ho presa al volo. Lui, fortunatamente, di lì a poco, aveva un impegno, e ci siamo salutati.

P. Perché "fortunatamente"?

R. Perché altrimenti avremmo dovuto occuparci, subito dopo, della massa dei fotoni a riposo, anziché, come é stato, il giorno seguente.

P. Vuoi dire che...

R. Proprio così. Nessuna speranza: né per lui né per la Relatività. C'é rimasto male. Ma, a questo punto, molto abilmente devo riconoscere, io ho invocato lo Spirito della Scienza, affermando la necessità di anteporre a qualsiasi discorso (e dunque al mio come al suo) ancorché ne paresse assai fondato, ciò che l'esperienza e il senso ci dimostra. E comunque, a scanso di ripensamenti, avevo opportunamente "dimenticato" la lettera di Investitura in albergo, e mi sono affrettato a salutare: questioni imprescindibili di orario ferroviario... ed eccomi qua.

P. E adesso?

R. Adesso vado a Graz.

(telefono)

P. Pronto?... Sei tornato!?

R. Certo.

P. Che or'é?

R. Le sei.

P. Vieni subito?

R. No. Sono in Istituto. Devo fare delle fotocopie e dopo colazione. Sono da te alle otto.

P. Va bene.

(studio)

P. Dài, com'é andata?

R. Bene.

P. Racconta.

R. Ho incontrato *Marinov*.

P. Com'é?

R. Matto come un cavallo: in senso buono intendo. Un bulgaro. Pensa che ha cominciato prima a contestare Einstein e poi, non contento, il regime dove stava.

P. E allora?

R. Puoi immaginarlo. Se contestare *Einstein* poteva sembrare il segno di una perdonabile eccentricità, contestare il Socialismo Reale era sicuramente quello di un'imperdonabile follia... L'hanno impacchettato e sbattuto in un Manicomio di Stato, quelli di casa sua, e se n'é fatto un bel po'. Poi é riuscito a scappare, "Oltre Cortina", come suol dirsi, che sarebbe dalle nostre parti.

P. E qua?

R. Male ugualmente. Così ha pensato fosse una buona idea sbarcare il lunario con la Macchina del Moto Perpetuo.

P. E com'é andata?

R. Per andare é andata, nel senso che c'é un sacco di gente, a quanto pare, disposta a finanziare l'avventura del Moto Perpetuo. Un po' c'é campato. Ma, per la Scienza Ufficiale, é irrimediabilmente bruciato.

P. E l'esperimento?

R. L'esperimento va. Me l'ha spiegato. Ma non é questo il meglio.

P. Cosa dunque?

R. Ricordi? Avevo chiesto a *Vigier* una copia del lavoro di *Silvertooth*; ma lui non l'ha trovata. *Marinov* l'aveva, e me

l'ha data.

P. E allora?

R. E allora quando l'ho letto in albergo, ho fatto un altro salto.

P. Cosa c'era?

R. C'era che la faccenda era già chiusa. Fin dai tempi di Sagnac.

P. Spiegati.

R. Vedi, *Silvertooth* ha lavorato con la differenza dei due tempi. Ricordi?... e per far questo gli occorrevan due equazioni. Una l'ha presa da *Michelson* e l'altra, dice lui, non c'era. Così l'ha presa da *Sagnac*, e poi ha fatto la misura.

P. E invece?

R. C'erano. In *Michelson* e *Morley*, tutt'e due. Solo che lui non se n'é accorto e, fino ad oggi, neanche io.

P. E allora?

R. Semplice: Da *Michelson* prendi le equazioni, e *Sagnac* é la prova.

P. Vuoi dire...?

R. Che la Relatività é finita, finta da sempre.

P. E *Marinov*?

R. Io gliel'ho detto, ma lui era troppo occupato con la sua Macchina per farci caso. Poi ne ha combinata un'altra.

P. E sarebbe?

R. Qualche giorno prima, a quanto pare, aveva spedito una lettera al Console Britannico, con la quale minacciava di bruciarsi davanti all'Ambasciata se un tal *Maddox*, editore, non gli avesse finalmente pubblicata, come promesso, una sua lettera su Nature, una rivista. Me ne aveva accennato.

P. E allora?

R. Io, nel frattempo, l'avevo convinto a riscrivere il lavoro spedito a *Vigier* in una forma alleggerita da inutili divagazioni. Quella, come Referee, avrei approvato.

P. E lui?

R. Ha accettato. In un giorno l'ha scritta, mentre io mi stavo ristudiando l'intera questione. Era domenica. Lunedì mattina dovevo passare a ritirarla e a chiarirmi gli ultimi dubbi. Invece...

P. Cos'é successo?

R. Quando sono arrivato, attaccato alla sua porta c'era questo biglietto:

"La chiave é sotto!Roberto, sono arrestato e nella Psichiatria di Graz. Cerchi di trovarmi. Prendi l'articolo e lì c'é anche lettera per Vigier. S."

Secondo te cos'ho pensato?

P. I Conservatori della Quiete!

R. Elementare... poi, mentre riponevo nella borsa la lettera a *Vigier* e il suo lavoro, mi é venuta in mente quell'altra, all'Ambasciata.

P. Era per via di quella?

R. Proprio lei. Il console britannico l'aveva girata alla Polizia e, a quanto pare, in Austria c'é una legge per cui se uno "si minaccia" loro sono autorizzati a sbatterlo in Manicomio, per precauzione, in attesa di ulteriori accertamenti. Così l'hanno preso e lì l'ho trovato.

P. Dove?

R. Al Manicomio di Graz. Dove ho tentato di spiegare che, ovviamente, Marinov intendeva scherzare. Inutilmente. Ho spiegato allora che io ero lì per lavorare. Ora: dal momento che lui non poteva uscire, avevano niente in contrario a far entrare me? Gentili. Danke. Così abbiamo concluso, su un tavolino d'angolo, in fondo a un corridoio, crocevia di volti stralunati.

P. Vuoi dire che l'ultima parola sulla Relatività é stata detta in un Manicomio?

R. Proprio così, e i Relativisti diranno certamente: "Nel luogo più appropriato"!... Dopo ho avvertito un paio di giornalisti, suoi amici, che lo tirassero fuori, e son partito.

P.Lasciami prender fiato...
...Un Grande Bluff! La Relatività Finita! Sai che ci ho sempre creduto, ma a questo punto voglio capire bene, essere capace di spiegarla. Perciò, innanzitutto: cosa significa "Teoria della Relatività".

R. Vedi, ci sono due aspetti da considerare. Il primo consiste nella Teoria in sé con i suoi due Postulati. E la spiegazione di questo aspetto sta nella Nota Storica che segue. Il secondo, invece, ben più importante, consiste nel peso sociale, politico, culturale, morale, economico e infine tecnologico che la Relatività ha avuto in questo secolo... cominciamo con questo esempio.
Tutti senz'altro ricordano, o quantomeno hanno sentito parlare, dell'impatto che la Rivoluzione Copernicana ha avuto ai tempi di Galileo. Da lì si é avviata una radicale trasformazione che ha rotto gli argini dell' *Antica Società* segnando l'atto di nascita dei *Tempi Moderni*.
Bene. All'inizio di questo *Ventesimo Secolo* e per oltre ottant'anni é nata e si é sviluppata una sorta di Controrivoluzione Tolemaica. Una eclisse della ragione e un ritorno a dogmatismi e ad atti di fede sostenuti, questa volta, da una presunta *Razionalità*.
Questa *Controrivoluzione* si é posta come una barriera insormontabile a fronte di sempre nuovi e possibili sviluppi della Conoscenza. E' facile dunque immaginare le possibili conseguenze dell'improvviso crollo di questa immane *Diga*.

P. Esempi concreti.

R. Innanzitutto l'abitudine alla soggezione morale, intellettuale e quindi sociale e politica verso l'Ordine comunque Costituito.

P. Su questo siamo sempre stati d'accordo, ma cosa c'entra Einstein?

R. Vedi, quando qualcuno mi chiede che lavoro faccio, e io dico: *il Fisico*, odo spesso esclamazioni e attestati di grande meraviglia e ammirazione, subito seguiti dall'affermazione: *io di fisica non ho mai capito niente*. Ed io rimango invece sconcertato perché so bene che non c'é niente da capire, o che non sia possibile capire, in mezzo allo sfascio intellettuale e morale di persone che usurpano, godono ed abusano indebitamente del *Rispetto del mondo: gli Scienziati*.
E l'altra cosa che mi colpisce é l'implicita e complice soggezione intellettuale cui é abituata la maggior parte della gente.
Una abitudine alla soggezione verso qualcosa che si suppone li trascenda e si ponga al di là della loro capacità di comprensione, che é il presupposto sul quale si fonda l'accettazione acritica, passiva o rassegnata di tutti gli altri aspetti socialmente spiacevoli, razionalmente assurdi e moralmente intollerabili di questi *Tempi Moderni*.
Dalla accettazione del *Paradosso dei Gemelli*, dei *Buchi Neri*, dell' *Universo in Espansione o Inflazionario* a quella dei Paradossi sociali dello spreco e della distruzione delle risorse che crea povertà, fame e disoccupazione per gli uni nell'abbondanza degli altri, tutti conseguenti all'altro Paradosso per il quale la ricchezza non viene dal lavoro ma dalla ricchezza, e il denaro investito nelle speculazioni finanziarie rende più di quello investito per il bene comune, il passo é più breve di quanto si possa immaginare.

P. Una specie di *Teoria della Relatività Sociale* insomma.

R. Proprio così. Quando uno é pronto ad accettare che A+B é uguale ad A ed A-B é ancora uguale ad A, e dopo essersi spremuto le meningi conclude che l'accettazione di questo Paradosso é una grande scoperta scientifica e che lui stesso nel *capirlo* sta compiendo un grosso salto di qualità intellettuale, bene, questa persona é ormai pronta e disposta ad accettare qualsiasi sciocchezza o assurdità purché rivestita di adeguati Paramenti Istituzionali: é in questo modo che la grande maggioranza é giunta al punto in cui piuttosto che rischiar di riconoscere senza preventiva autorizzazione che il bianco é bianco e il nero é nero, si affida al giudizio dell'Esperto dichiarando la propria incompetenza in fatto di colori.
E' in questo modo che ad affermazioni del tipo *Ah, io di Fisica non ho mai capito niente* seguono *naturalmente* affermazioni del tipo: *Io non mi interesso di politica* o ancora *L'economia per me é arabo*. Proprio in relazione a queste malaugurate abitudini il Crollo della Relatività é l'occasione di una Rivoluzione Culturale paragonabile ad una nuova Rivoluzione Copernicana.

P. Un'altra cosa: quello che mi ha colpito da quando ho cominciato a seguire la faccenda é che la velocità della luce non é più un limite insuperabile: il che, correggimi se sbaglio, significa che la possibilità di girare per l'Universo potrebbe non appartenere più solamente ai films di fantascienza.

R. Infatti. Dopo Einstein ci si é abituati all'idea di essere confinati su questo pianeta, e sebbene ai nostri giorni racconti e films di fantascienza abbiano largo mercato, sono considerati, per tacita ammissione, il frutto di un sogno di evasione irrealizzabile. Tutti infatti *sanno* che non si può superare la velocità della luce... perché l'ha detto Einstein. E in questo senso la riscoperta dell'etere (che secondo Einstein

non esiste) é come, per un pesce, aver ritrovato l'acqua che gli era stata tolta. Si tratta, adesso, di ricominciare a nuotare.

P. Passiamo a un altro aspetto della faccenda, quello economico. Il Crollo della Relatività porta con sé, evidentemente, non solo conseguenze scientifiche ma anche, nell'ambito stesso della ricerca, conseguenze economiche di notevole portata, quali, ad esempio, una redistribuzione degli investimenti per la ricerca scientifica.

R. Infatti. Oggi una larga percentuale dei contratti di ricerca e dei conseguenti fondi sono distribuiti sulla base del giudizio o delle opinioni espresse da Illustri Cattedratici. E, praticamente, tutte le Cattedre di Fisica Teorica, di Astrofisica, e di altre branche della Fisica sono state dispensate, negli ultimi decenni, sulla base di meriti scientifici (ricerche, pubblicazioni, ecc.) direttamente o indirettamente legati alle Teorie Einsteiniane. Gli interessi Accademici e finanziari che circondano queste Teorie sono quindi enormi. Migliaia di miliardi sono oggi indirizzati a ricerche Astrofisiche o Cosmologiche e a ricerche nel campo della Fisica delle Alte Energie che, non a caso, si sono ricongiunte una all'altra come un cane che si morde la coda per dibattere su quello che é *realmente accaduto* nei primi 10 alla meno quarantasette secondi dopo la nascita dell'Universo o per costruire cattedrali nel deserto, sopra o sotto terra.
Come i *superacceleratori* (l'Eloisatron di Zichichi, per esempio) che dovrebbero far vedere il nuovo *supermondo* delle Particelle *ancor più elementari*, o la Grande Caverna del Gran Sasso, scavata per assistere alla morte di un Protone ogni miliardo di miliardi di anni.
Per tutti costoro il Crollo della Relatività é una specie di Great Crash (altro che Big Bang) nella Wall Street della Scienza Ufficiale.

P. Bene. E quali saranno, *per l'uomo della strada* i risvolti, le conseguenze tecnologiche della faccenda?

R. Chi non conosce il retroscena della ricerca scientifica contemporanea non può immaginare quante idee o indirizzi di ricerca che avrebbero potuto risolversi, anche rapidamente, in nuove tecnologie, e di conseguenza, potenzialmente, in un miglioramento della qualità della vita (a tutti i livelli, intellettuale e materiale) sono stati stroncati sul nascere o comunque ritardati e ostacolati dal momento in cui mostravano di andare a scontrarsi con l'Ortodossia... e gli interessi economici che le stavano dietro.
Molti ricorderanno, probabilmente, la storia di Galileo, che offriva il suo telescopio agli Aristotelici, perché ci guardassero dentro. E che costoro, molto coerentemente, rifiutavano.
Bene. Questa é oggi l'ordinaria amministrazione in campo scientifico. Quando il progresso della Conoscenza entra in conflitto con gli interessi di coloro che detengono il Monopolio delle Verità e del Potere Economico ogni Teoria non *in linea*, ogni Dissidenza, diventa Sovversione dell'Ordine Costituito, e viene trattata conseguentemente.

P. E' l'Ancien Régime che si rigenera come l'araba fenice... dove sia nessun lo dice... che ci sia ciascun lo sa.

R. Proprio così.

P. Bene. Ma potresti fare qualche esempio più specifico?

R. Vediamo... ancora la velocità della luce. Ma prima debbo fare una premessa.

Oggi, una parte troppo grande delle cosiddette *Teorie Unitarie* (teorie che cercano di riassumere *in una sola formulazione* l'intero Universo) é pura manipolazione matematica, imprecisa quanto i presupposti iniziali sui quali riposa, che permette ai vari autori di perdere di vista la complessità e le interdipendenze del mondo reale in un dedalo di simboli pretenziosi e inutili. Determinante, a questo riguardo, é certamente stata l'influenza del programma einsteiniano, finalizzato alla costruzione di una *Teoria Unitaria* anche quando divenne chiaro, dopo il 1935, che esistevano, in Natura, interazioni (forze) fondamentali diverse da quelle gravitazionali, elettriche e magnetiche.

Vedi, ad esempio a me non ha mai interessato chiudere porte alle mie spalle fino ad aprire l'ultima: la *Porta della Conoscenza Suprema*, per ritrovarmi *finalmente* di fronte a un vicolo cieco. Mi interessa invece riconoscere, senza incertezze quali stanno dietro di me per capire quali mi stanno davanti, e queste tenere bene aperte, e aprirne ancora.

Mentre i seguaci di Einstein continuano ad inseguire l'illusione di poter, finalmente, un giorno, racchiudere l'intero Universo in una sola Teoria. E assolutamente sprovvisti, da buoni Teorici, di *Fantasia Sperimentale*, essi cercano conseguentemente di chiudere tutte le *Vie Possibili* diverse dalla propria, ogni qualvolta ne incontrano una, e non ne ammettono neppure l'esistenza. Illusi come sono di poter cogliere tutti i frutti dell'Albero della Conoscenza nel breve arco di una vita, la Novità, l'Imprevisto, sono loro Nemici.

Da ciò due ordini di conseguenze: sprechi evidenti e possibilità mancate.

Vediamone appunto gli esempi concreti.

Prima dell'affermarsi della Teoria della Relatività, l'ordine di grandezza della velocità dell'interazione gravitazionale é stato oggetto di analisi da parte di Laplace (1845)[*]il quale, sulla base sperimentale del moto dei Pianeti, ricavò la relazione: *La velocità dell'interazione gravitazionale é almeno seicento milioni di volte superiore alla velocità della luce.* Tra il 1916 e il 1918 Einstein affronta, a sua volta, il problema costituito dalla definizione del modo di propagazione e della velocità delle forze gravitazionali. Le tre possibili combinazioni a disposizione erano: onde 1) Longitudinali; 2) Longitudinali-Trasversali; 3) Trasversali. Le onde di tipo 1) e 2) furono scartate perché risultava che esse, nell'ambito delle ipotesi Relativistiche, non potevano trasportare energia e potevano, invece, avere velocità arbitraria, mentre, secondo Einstein, nessun segnale può superare la velocità della luce. La sua soluzione non poteva essere, dunque, altro che la seguente: onde gravitazionali trasversali, con velocità c, sul *modello* elettromagnetico.

Questa, a quanto mi risulta, é la sola *reale* motivazione delle conclusioni einsteiniane aggravate dall'illusione, tipicamente metafisica, di poter determinare a priori, indipendentemente dall'esperienza, il valore numerico del coefficiente che, nell'equazione delle onde (longitudinali o trasversali che siano é la stessa), avendo dimensioni $[L/T]^{-2}$ (lunghezza diviso tempo alla meno due), rappresenta l'inverso del quadrato della velocità.

E la **Nota Autorevolezza di Albert Einstein** é l'unico presupposto sulla base del quale Amaldi in Italia ed altri nel Mondo, sono autorizzati a buttare miliardi nella ricerca delle *Onde Gravitazionali Einsteiniane*... ovviamente senza alcun risultato. Sebbene si vociferi che Amaldi, almeno, qualche risultato l'abbia avuto.

P. E sarebbe?   *Laplace died in 1827.

R. L'*Effetto Tram*. L'antenna gravitazionale di Frascati sentiva le *onde* di tutti gli autobus che passavano nelle vicinanze. Per questo l'hanno dovuta mettere sotto terra, in un luogo più tranquillo, a Ginevra.

P. Interessante. Ma torniamo all'uomo della strada. Che c'entra?

R. C'entra. I miliardi per i giocattoli di Amaldi li paga di tasca sua: l'equivalente in Tasse.

P. Non ci avevo pensato. Bisognerebbe coniare un altro termine, oltre a *Evasione Fiscale: Spreco Fiscale*... scientificamente organizzato!

R. Proprio così. Questo di Amaldi come quello di Zichichi. E un giocattolo tale, quest'ultimo (l'Eloisatron), da prosciugare per anni le risorse di molte Nazioni.
E dimmi ora: chi autorizza costoro a perseverare in questi sprechi?

P. La Nota **Autorevolezza** di Albert Einstein!

R. Bene. Un altro esempio.
Quando sono state scoperte le forze nucleari (le Interazioni Forti) quale velocità, secondo te, gli è stata attribuita?

P. La velocità della luce.

R. Bravo. Vedo che cominci a capire. E proprio per questo dopo anni di ricerche e di vantati successi nel campo della Fisica Nucleare siamo indubbiamente capaci di molte Chernobyl, ma non siamo in grado di capire, in realtà, come è fatto e come funziona un atomo di Idrogeno, che è il più semplice di tutti gli atomi. O, ancora, il motivo per cui ci si accorge oggi, dopo trent'anni di incredibili sprechi, che la Fusione **Termo-nucleare** controllata, frutto che sembrava ragionevole poter cogliere dopo la Bomba H, è in un vicolo cieco.

Ci si accorge, in altri termini, che l'eccessiva, conseguente attenzione verso la Fisica delle Alte Energie (la *Fisica del Caldo*, appunto) ci ha condotto a trascurare un campo ben più promettente e ricco di *ricadute* tecnologiche quale è quello della Fisica delle Basse Energie (la *Fisica del Freddo*). Talchè solo in questi ultimi tempi, dopo una scoperta del tutto casuale, si cominciano ad investire risorse adeguate nel campo della Superconduttività. E, a questo punto, ci si accorge che, a tutt'oggi, non sappiamo neppure cosa sia la Corrente Elettrica!
E tutti ormai sanno quali vantaggi potrebbero trarsi dalla Superconduttività.

P. E' vero. Anch'io ne ho sentito parlare, quando hanno dato il Nobel a quello Svizzero.

R. Proprio lui. Ma ora un altro esempio. L'ultimo.
Come ricorderai la Chimica è seguita all'Alchimia. E si è sviluppata perchè, a differenza della prima (in ordine di tempo) si contentava di *lavorare* con gli atomi, anzichè cercar di Trasmutarli.

P. Non mi dirai che Einstein ha fatto danni anche nel campo della Chimica!

R. E invece te lo dico. Indirettamente, ma ne ha fatti. Sta a sentire. Quando Lavoisier partì dal presupposto della immu-

abilità della Identità degli atomi (elementi) in una reazione chimica, Vauquelin studiava le galline.

P. ... saltate fuori *dal cortile che confina con l'Università*.

R. No. Non quel problema, e in questo caso lui ha fatto una domanda a una gallina.

P. E cioè?

R. Come faceva a fare le uova. Perchè gli risultava che c'era più Calcio, in un uovo, di quanto ne mangiasse una gallina.

P. E la gallina ha risposto?

R. Non a lui. A Kervran, molto tempo dopo: Trasmuta Magnesio in Calcio.

P. Che c'è di male?

R. Per me niente. Ma per Einstein sì. Vedi, il grande *merito* di Einstein, quello per il quale gli è stata *attribuita* la paternità di E = mc^2, secondo Born, è *aver stabilito il teorema dell'inerzia dell'energia nella sua completa generalità*.

P. E in cosa consiste questa *completa generalità*?

R. Nel fatto che, secondo Einstein, E = mc^2 vale per tutte le Trasmutazioni di massa in energia. E, in particolare, anche per tutte le *Trasmutazioni nucleari*.
In altri termini, anzichè tante *possibili* E = mv^2, ove v è una tra le velocità di propagazione delle diverse forze, per Einstein c'è *una sola E = mv^2*: E = mc^2, come se in Natura non possano darsi altre forme di energia radiante oltre quella

elettromagnetica. E da questo presupposto segue che una gallina che trasmuta Magnesio in Calcio dovrebbe essere una specie di *stufa* ad altissima temperatura, altamente radioa` .iva.

P. Le galline non sono radioattive!

R. Certamente. Così come continuano a non tornare i conti del *bilancio del Calcio in entrata e in uscita*.

P. E allora?

R. E allora grazie alla **Nota Autorevolezza di Albert Einstein** e ai *pregiudizi* contro l'Alchimia, il problema è stato ignorato. E noi abbiamo perso l'occasione di sviluppare una nuova possibile *Chimica delle trasmutazioni a debole energia*, basata sul fatto che molte reazioni *AlChimiche*, che coinvolgono cioè il nucleo stesso e non solo gli elettroni, fino ad oggi ritenute possibili solo ad alta energia, avvengono abitualmente, in realtà, a bassa energia all'interno delle cellule di un qualsiasi organismo animale o vegetale, *violando* il Principio della Immutabilità dell'Identità degli atomi in una *reazione chimica*.
E per il momento mi pare che basti.

P. In conclusione si potrebbe dire che quando sembrava che tutto fosse stato scoperto ci si accorge che tutto è ancora da scoprire.

R. E' vero. Da più parti ho sentito ultimamente affermare che eravamo ormai prossimi alla **Grande Unificazione** di tutti i fenomeni fisici, che avrebbe segnato, in un certo senso, *la fine della Fisica*.
In realtà siamo di fronte unicamente alla Fine della Relatività.

Editorial note.

Marinov was imprisoned in the psychiatric clinic of Graz on the 1 February 1988 after he has sent his telefax of the 30 January 1988 on Mrs. Elizabeth Hughes, the managing editor of NATURE (see TWT-III, p. 262).

The report of the biggest Austrian journal NEUE KRONEN ZEITUNG on Marinov's imprisonment is reprinted on p. 266 of TWT-III.

stefan marinov

dilatazione cinematica del tempo

Premessa

La dilatazione cinematica del tempo è il rallentamento che un "orologio" effettua quando la sua velocità nello spazio assoluto si avvicina alla velocità della luce. La dilatazione dinamica (o gravitazionale) del tempo è il rallentamento di un "orologio" trasferito in luogo in cui il potenziale gravitazionale è maggiore.

Secondo la teoria della relatività la dilatazione cinematica è un effetto relativo e deve essere sempre considerata nei riguardi di un osservatore inerziale.

Questa premessa falsa della relatività conduce al cosiddetto "paradosso dei gemelli". Dimostrerò come tale paradosso, dal punto di vista della teoria dello spazio-tempo assoluto, non esista mentre, dal punto di vista della teoria della relatività, sia irrisolvibile.

Introduzione

A tutt'oggi ho misurato la velocità assoluta della Terra mediante tre esperimenti ottici (Marinov 1974, Marinov 1980, Marinov 1986) ed un esperimento elettrodinamico (Marinov 1989).

Col secondo esperimento (Marinov 1980), che venne eseguito nell'arco di sei mesi, potei stabilire che la velocità assoluta del Sole è v = 303 ± 20 km/sec, con coordinate equatoriali dell'apice: declinazione δ = 23° ± 4° e ascensione retta α = 13^h 23^m ± 20^m. Questo risultato impone necessariamente una revisione di tutti i concetti introdotti dalla relatività einsteiniana.

Nel (Marinov 1976) analizzai la dilatazione gravitazionale (dinamica) del tempo alla luce della mia teoria dello spazio-tempo assoluto.

Il presente articolo è dedicato alla dilatazione cinematica del tempo. Devo osservare che sarebbe assai più corretto parlare di *rallentamento* gravitazionale e cinematico degli orologi che di *dilatazione* gravitazionale e cinematica del tempo, ma poiché questa formulazione si è imposta nel linguaggio scientifico la userò anch'io, sottolineando però che **non esiste** dilatazione del tempo, ma solo rallentamento degli orologi.

Larmor per primo, nel 1900, introdusse la nozione di *dilatazione del tempo* (cinematica), considerandola come un effetto assoluto. Nel 1905 Einstein, analizzando lo stesso fenomeno, a distanza di soli cinque anni lo considera come un effetto relativo.

Lorentz trattò questa dilatazione del tempo, in diverse pubblicazioni, anche da un punto di vista assoluto; poiché questo effetto segue anche dalle trasformazioni che da lui prendono il nome, io definirò la dilatazione cinematica del tempo: *dilatazione lorentziana del tempo*. Ed essendo stato Einstein il primo a considerare la dilatazione gravitazionale del tempo, definirò questa: *dilatazione einsteiniana del tempo*.

Il paradosso dei gemelli

Quando la dilatazione del tempo (non applicherò più l'aggettivo "cinematica") è considerata come fenomeno relativo ha origine il famoso "paradosso degli orologi" o"paradosso dei gemelli" che può essere così formulato: se uno di due gemelli rimane fisso in un sistema di riferimento inerziale mentre l'altro compie un viaggio a velocità prossima alla velocità della luce,

al ritorno di questi constateremmo che il gemello viaggiatore sarebbe invecchiato di meno del gemello rimasto a casa (consideriamo i gemelli come orologi).

Devo sottolineare che tale paradosso, nella teoria della relatività, non nasce dal fatto che nei sistemi materiali in moto nello spazio assoluto si verifica un rallentamento dei fenomeni periodici; esso deriva dall'affermazione dogmatica relativistica che questo rallentamento è assolutamente simmetrico per due sistemi, dipendendo solamente dalla loro velocità relativa. La difficoltà logica insormontabile nella teoria della relatività consiste nell'arrivare, da due effetti assolutamente simmetrici, ad un risultato asimmetrico. Sono state scritte migliaia di pagine, da persone considerate esperte, dedicate alla soluzione di questo paradosso **insolubile**.

Dimostrerò, ora, l'insolubilità del paradosso nell'ambito della relatività.

Supponiamo che i noti gemelli abbiano due orologi identici (potrebbero essere i loro cuori o due orologi atomici di estrema precisione). Ciò comporta che quando i gemelli sono in riposo in un sistema di riferimento inerziale le lancette dei loro orologi mostrano sempre la stessa ora.

Ogni trattato relativistico porta sempre alla conclusione che il gemello che ha "subito accelerazione" deve rimanere più giovane dell'altro. Qui si pone una domanda inevitabile: cosa succede a questo gemello (ed all'orologio che porta) durante il tempo del movimento accelerato? Per evitare questa domanda, poiché il rallentamento è un effetto che si produce durante il tempo del movimento inerziale, facciamo subire la stessa **accelerazione** ad entrambi i gemelli durante la loro separazione.

Questa ipotesi può essere realizzata come segue: al momento iniziale i due gemelli sono a riposo nel sistema di riferimento inerziale applicato alla loro casa; salgono quindi su due razzi identici cui vengono applicati moti identici; quando la velocità dei razzi raggiunge il valore v, essi spengono i motori bloccando con la propulsione; il razzo del secondo gemello continua il suo moto con velocità inerziale v, mentre il primo gemello riaccende i motori applicando una propulsione contraria, variando così la velocità da v a - v, e poi di nuovo invertendo la propulsione finché la velocità si riduce a v = 0 al momento dell'atterraggio al punto di partenza, che è la loro casa, dove il primo gemello aspetta il ritorno del fratello.

Secondo la teoria della relatività speciale, il paradosso dei gemelli è il risultato del volo libero. Durante il tempo in cui hanno subito accelerazione (tempo che è identico per entrambi, ovviamente) i gemelli possono fermare i loro orologi e confrontare il tempo del volo libero del secondo gemello con il tempo registrato dal primo che rimane a casa. Potendo anche ammettere che i tempi senza accelerazione siano molto più lunghi dei tempi con accelerazione, possiamo trascurare questi ultimi. Se chiamiamo t_1 e t_2 i tempi registrati dai due gemelli al momento dell'incontro, secondo la teoria della relatività (v. per es. Möller, 1955) dovrebbe essere:

$$t_2 = t_1 \left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}}. \qquad (1)$$

Sulla base della trasformazione di Lorentz, solo il primo gemello potrebbe scrivere questa relazione, mentre il secondo dovrebbe scrivere la relazione esattamente opposta.

Dobbiamo ammettere, allora, che la relazione (1) è scritta da una *mano assoluta*, da qualcuno che sta usando non solamente le formule della trasformazione di Lorentz, cioè da un

osservatore che sa che il primo gemello ha preso una sola volta una porzione di pillole contro il mal di moto accelerato, mentre il secondo le ha prese per tre volte; e che sa, inoltre, che l'orologio del gemello 2 è rimasto fermo tre volte per una durata complessiva pari a quella della fermata dell'altro orologio. Senza queste informazioni supplementari non si potrebbe arrivare ad alcuna conclusione, ma tali informazioni *non possono essere inserite* nelle trasformazioni di Lorentz.

Supponiamo che il secondo gemello abbia registrato nel suo libro di bordo il tempo t'', indicato dal proprio orologio nell'istante in cui ha acceso la propulsione contraria avendo deciso di tornare a casa; poniamo allora alla relatività speciale questa domanda: che relazione c'è fra il tempo t'' ed il tempo $t' = t_1/2$?

Secondo le formule della trasformazione di Lorentz, dal punto di vista del gemello 1 (vale a dire operando nel suo sistema di riferimento inerziale in cui il gemello 2 vola a velocità v) la relazione deve essere

$$t'' = t'\left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}} . \tag{2}$$

mentre dal punto di vista del gemello 2 (vale a dire operando nel suo sistema di riferimento inerziale in cui il gemello 1 vola a velocità v) la relazione deve essere

$$t' = t''\left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}} . \tag{3}$$

Le indicazioni t' e t'' dei due orologi non possono essere comparate direttamente in quanto fra i due gemelli si estende l'immensa distanza che li separa. Allo scopo di stabilire quale delle relazioni (2) e (3) sia vera, poniamo che nell'istante t' un terzo gemello venga lanciato dal primo gemello (nel suo sistema di riferimento) con una velocità 2v nella direzione in cui è partito il gemello 2; poniamo che questi ritorni a casa con un razzo ausiliario lasciando un quarto gemello - partito assieme a lui - a continuare il volo con il razzo principale alla stessa velocità inerziale v (nel sistema di riferimento del primo gemello).

Il gemello 3 raggiungerà il gemello 4 quando i loro orologi indicheranno rispettivamente i tempi t_3 e t_4; secondo la relatività speciale si dovrà avere

$$t_3 = t_4\left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}} . \tag{4}$$

in quanto il gemello 3 vola - rispetto al sistema di riferimento inerziale in cui il gemello 4 sta in riposo - con velocità -v durante la prima metà del suo viaggio, e con velocità v durante la seconda metà.

Ponendo nella formula (1): $t_1 = 2t'$, $t_2 = 2t''$, e nella formula (4): $t_3 = 2t'$, $t_4 = 2t''$, otteniamo rispettivamente le formule (2) e (3) che sono matematicamente contraddittorie. L'unica obiezione che si potrebbe avanzare è se le relazioni $t_2 = 2t''$ e $t_3 = 2t'$ siano giuste; ma questa è un'obiezione che non può essere posta dalla relatività speciale, in quanto per tale teoria uno spazio assoluto non esiste ed è importante solamente la velocità **relativa** dei gemelli. Per la relatività speciale, se il gemello 2 ha impiegato il tempo t'' per l'andata nel sistema inerziale del gemello 1, dovrà impiegare lo stesso tempo per il ritorno. Esattamente la stessa cosa avviene nel caso del gemello 3 che spende un tempo t' per l'andata (rimanendo in effetti con papà e mamma nella casa dei gemelli) che è lo stesso tempo che impiegherà per il ritorno nel sistema inerziale del gemello 4.

Il concetto di dilatazione del tempo nella teoria della relatività speciale si rivela dunque logicamente **inconsistente**, e chi voglia sostenerlo dimostra di non tenere in conto assolutamente la logica formale.

Secondo la mia teoria dello spazio-tempo assoluto (Marinov, 1972, 1975 - 76 - 77) i corsi degli orologi dipendono dalle loro velocità assolute, e si arriva pertanto alla seguente conclusione: se il gemello 1 sta in riposo nello spazio assoluto la relazione fra t' e t'' sarà espressa dalla (2); se il gemello 4 sta in riposo nello spazio assoluto la relazione sarà la (3). Come si può ricavare, in entrambi i casi la relazione fra t_1 e t_2 sarà espressa dalla (1) e la relazione fra t_3 e t_4 dalla (4).

Farò ora la dimostrazione per il caso in cui il gemello 1 stia in riposo nello spazio assoluto. In tale caso sarà valida la relazione (2) ed essendo $t_1 = 2t'$, $t_2 = 2t''$, si arriva subito alla (1). Avremo inoltre

$$t_4 = 2t'' = 2t'\left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}} \cong 2t'\left(1 - \frac{v^2}{2c^2}\right), \qquad (5)$$

e

$$t_3 = t' + t'\left(1 - \frac{4v^2}{c^2}\right)^{\frac{1}{2}} \cong 2t'\left(1 - \frac{v^2}{c^2}\right), \qquad (6)$$

e dalle (5) e (6) arriviamo immediatamente alla (4).

Se nello spazio assoluto il gemello 1 ha una velocità v ed il gemello 4 (equivalente al gemello 2 nella prima metà del suo viaggio) ha una velocità v'', la relazione fra t' e t'' sarà data da

$$\frac{t'}{\left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}}} = \frac{t''}{\left(1 - \frac{v''^2}{c^2}\right)^{\frac{1}{2}}}, \qquad (7)$$

come può essere verificato prendendo in considerazione un quinto gemello che rimanga in riposo nello spazio assoluto.

Dunque nella mia teoria dello spazio tempo assoluto, procedendo dalle trasformazioni di Marinov (Marinov, 1979), si arriva alla seguente affermazione: orologi che si muovono nello spazio assoluto con velocità diverse hanno corsi diversi. Affermazione che non porta ad alcuna conclusione paradossale. Il gemello viaggiatore sarà invecchiato di meno del fratello rimasto a casa, ma in questo caso dal punto di vista logico e matematico non c'è nulla da discutere. Che il corso di un orologio dipenda dalla sua velocità nello spazio assoluto può apparire un fenomeno strano per una persona qualunque, ma è un fenomeno naturale che si spiega con l'aiuto di un orologio di luce (Marinov, 1975, 1979), tenendo conto del carattere etereo - da me introdotto - della propagazione della luce, per il quale il periodo di un orologio di luce non dipende dall'orientamento del suo braccio (Marinov 1975, 1979).

Quindi la dilatazione del tempo assoluto non contiene più elementi paradossali di quanti ne contenga il principio di Archimede (quello scoperto nella vasca da bagno, per il quale un corpo immerso in un liquido perde peso proporzionalmente al volume).

L'esperimento con il decadimento dei muoni

Il primo esperimento che ha confermato direttamente la dilatazione del tempo è il cosiddetto "esperimento dei muoni", compiuto nel 1941 da Rossi e Hall.

Secondo la legge del decadimento radioattivo, se c'è un numero N_0 abbastanza grande di atomi radioattivi, dopo un determinato tempo una parte di questi (N atomi) decadrà in altri atomi e particelle elementari. Poniamo $N = N_0/e$ (dove $e = 2,718...$ è la base dei logaritmi naturali) e chiamiamo *tempo di dimezzamento*, designato con T, il tempo corrispondente.

Se questi atomi si muovono nello spazio assoluto con velocità v, il loro tempo di dimezzamento T_0 viene detto tempo di dimezzamento *proprio*.

A causa della dilatazione del tempo avremo

$$T_0 = \frac{T}{\left(1 - \frac{v^2}{c^2}\right)^{\frac{1}{2}}}, \qquad (8)$$

$$(9), (10)$$

e T si chiama tempo di dimezzamento *universale*.

Ambedue i tempi di dimezzamento sono misurati con lo stesso orologio i cui secondi saranno detti *universali*, se l'orologio è in riposo nello spazio assoluto, e *propri* se l'orologio è in moto.

Il tempo di dimezzamento, come il periodo di un orologio di luce, può essere infatti considerato come una unità di misura. Nello spazio assoluto, il tempo di dimezzamento degli atomi radioattivi in riposo è più breve (contiene meno secondi) di quello degli atomi in moto; per questa ragione la relazione che esiste fra i periodi di due orologi di luce vale anche per i tempi di dimezzamento di due identiche quantità di atomi che, sempre nello spazio assoluto, si muovono con velocità diverse.

La stessa legge di decadimento è valida anche per i μ-mesoni (muoni). Essendo il loro tempo di dimezzamento $T = 2,2 \times 10^{-6}$ sec, e potendo produrre muoni con velocità prossime a c, allora, conoscendo la velocità e misurando la distanza d che i muoni coprono fino al momento in cui una frazione 1/e di essi decade, possiamo stabilire il loro tempo di dimezzamento proprio $T_0 = d/v$. Tale tempo di dimezzamento proprio deve essere confrontato con il tempo di dimezzamento universale T, misurato quando i muoni sono in riposo nel nostro laboratorio (essendo piccola la velocità assoluta della Terra - circa 300 km/sec - può essere trascurata e si può considerare la Terra in riposo nello spazio assoluto), e dovrà essere verificata la relazione (8).

L'esperimento con il decadimento dei muoni venne eseguito da Rossi e Hall misurando, sia in cima ad una montagna che a livello del mare, le quantità dei muoni nei raggi cosmici (prodotti vicino all'entrata nell'atmosfera dal decadimento dei fasci di raggi cosmici primari) che hanno velocità prossima a quella della luce.

La riduzione della quantità dei muoni ha dimostrato con sufficiente precisione la validità della formula (8).

Un esperimento simile, con muoni positivi e negativi su un'orbita circolare in un acceleratore, è stato eseguito nel 1977 da Bailey ed altri; in questo caso la (8) è stata confermata con alta precisione.

Dunque "l'orologio dei muoni" è stato il primo a verificare la tesi che ogni orologio rallenta il suo corso se è in movimento nello spazio assoluto.

L'esperimento con gli orologi intorno al mondo

Allo scopo di verificare la dilatazione del tempo con orologi macroscopici, Hafele e Keating (1972) eseguirono il cosiddetto *clocks-around-the world experiment*: due aviogetti con orologi atomici a bordo hanno lasciato Washington, uno verso est e l'altro verso ovest; dopo un viaggio intorno al pianeta i due aviogetti sono tornati al punto di partenza; i tempi registrati dagli orologi sono Δt_E, per l'aereo che ha volato verso est e Δt_W, per quello che ha volato verso ovest; si confrontarono quindi le due misure con una terza, Δt, di un orologio rimasto a Washington.

Per una analisi teorica di questo esperimento devo assumere che i due aerei abbiano volato sullo stesso parallelo ed alla

stessa altezza cui è stato posto l'orologio di Washington; se gli aerei volano a quote diverse e se l'orologio di riferimento è sulla Terra devo operare una correzione che tenga conto anche della dilatazione gravitazionale del tempo.

Nell'esperimento citato gli aerei hanno compiuto diversi atterraggi (erano aerei commerciali), variando diverse volte il loro potenziale gravitazionale per cui nell'esperimento si trovano frammischiate le dilatazioni del tempo lorentziana ed einsteiniana.

Trascurando il movimento della Terra nello spazio assoluto - ininfluente sulle registrazioni finali degli orologi - prendo in considerazione la sola velocità di rotazione la quale porta ad una asimmetria nel movimento dei diversi orologi. Questa velocità rotazionale, riferita al parallelo di Washington, è

$$v_r = \left(\frac{2\pi}{T}\right) R \cos\phi , \qquad (11)$$

ove R è il raggio della Terra, ϕ è la latitudine di Washington e T la durata del giorno siderale.

Se v è la velocità degli aerei rispetto alla Terra, la velocità di quello che vola verso est sarà: $v_E = v_r + v$ nello spazio assoluto, la velocità di quello che vola verso ovest: $v_w = v_r - v$, mentre quella dell'orologio di Washington sarà v_r. Dalla formula (7) otteniamo, con una esattezza del secondo ordine,

$$\frac{\delta t_E}{\Delta t} = -\frac{2vv_r + v^2}{2c^2} , \qquad \frac{\delta t_W}{\Delta t} = \frac{2vv_r - v^2}{2c^2} , \qquad (12)$$

dove $\delta t_E = \Delta t_E - \Delta t$, $\delta t_w = \Delta t_w - \Delta t$.

Prendendo per la latitudine di Washington $\phi = 43°$ (cos 43° = 0,730), T = 86,200 sec, R = 6370 km, otteniamo $v_r = 340$ m/sec.

Dello stesso ordine è la velocità degli aviogetti commerciali e possiamo accettare l'approssimazione $v = v_r = 300$ m/sec.

Sotto queste condizioni semplificate avremo

$$\frac{\delta t_E}{\Delta t} = -\frac{3v^2}{2c^2} = -15 \times 10^{-13} , \qquad \frac{\delta t_W}{\Delta t} = \frac{v^2}{2c^2} = 5 \times 10^{-13} . \qquad (13)$$

Gli aerei di Hafele e Keating volarono non sempre sul parallelo di Washington e fecero parecchi atterraggi, diversi per i due aerei. Prendendo in considerazione i percorsi ed i potenziali gravitazionali alle diverse quote, Hafele e Keating calcolarono $\delta t_E = -59 \pm 10$ ns, $\delta t_w = 273 \pm 7$ ns, dove le incertezze teoriche provengono dalle imprecisioni dei percorsi. Le differenze misurate sperimentalmente dalle indicazioni degli orologi atomici erano $\delta t_E = -40 \pm 23$ ns, $\delta t_w = 275 \pm 21$ ns.

I calcoli di Hafele e Keating vennero eseguiti esattamente secondo le formule della mia teoria dello spazio-tempo assoluto.

Secondo la teoria della relatività speciale gli orologi sugli aerei devono perdere lo stesso tempo nei confronti dell'orologio a Washington. Hafele (1972), traversando la jungla della relatività generale arriva alle mie formule. Cionostante, quando negli anni '73 - 74 mi misi in corrispondenza con lui, non riuscii a convincerlo che il suo esperimento rappresentava una conferma della dilatazione cinematica del tempo assoluto. Anche a Keating, con cui parlai nel 1978, non riuscii a far accettare l'idea dell'esistenza di uno spazio assoluto. C'è da aggiungere che L.Essen, il realizzatore del primo orologio atomico al cesio, considera l'esperimento di Hafele-Keating una "truffa", in quanto gli orologi atomici non offrono la precisione che un esperimento del genere esige.

Scrive Essen in un recente articolo (1978):

The results obtained from the individual clocks differed as much as 300 nanosecond. This absurdly optimistic conclusion was accepted and given wide publicity in the scientific literature and by the

Fig. 1 - L'esperimento con gli orologi agli antipodi sulla Terra.

media as confirmaton of the clock paradox. All the experiment showed was the clocks were not sufficient accurate to detect the small effect predicted. [7]

D'altra parte, Briatore e Leschiutta (1976) insistono di aver verificato sperimentalmente la dilatazione del tempo lorentziana ed einsteiniana, comparando per alcuni anni le indicazioni degli orologi atomici posti in diverse parti della Terra e che hanno diverse velocità assolute (a causa delle diverse latitudini) e diversi potenziali gravitazionali (a causa delle diverse altitudini e della forma non perfettamente sferica della Terra).

L'esperimento di Briatore e Leschiutta ha quasi lo stesso carattere di quello di Hafele e Keating.

L'esperimento con gli orologi agli antipodi

L'esperimento con gli orologi agli antipodi è stato proposto da me (Marinov, 1977). Questo è un *experimentum crucis* fra la mia teoria dello spazio-tempo assoluto e le teorie relativistiche, in quanto con tale esperimento è possibile misurare la velocità assoluta del laboratorio (e non solo la velocità di rotazione attorno all'asse terrestre come fanno gli esperimenti di Briatore-Leschiutta o Hafele-Keating).

Poniamo due orologi atomici in due punti agli antipodi sull'equatore terrestre (v. fig. 1, in cui la Terra è proiettata guardando dal polo nord).

Designamo con v la velocità assoluta e con v_r la velocità di rotazione lineare dell'equatore. Supponiamo, per semplicità, che l'asse terrestre sia perpendicolare al piano dell'eclittica e consideriamo un giorno dell'anno in cui la velocità del Sole v_s (o almeno la sua componente sul piano dell'eclittica) è parallela alla velocità della Terra attorno al Sole v_g. Assumiamo come istante iniziale il momento in cui per il primo orologio (A) c'è la levata del Sole e per il secondo (B) il tramonto. A questa condizione le velocità assolute degli orologi A e B saranno

$$v_A^2 = v^2 + v_r^2 - 2vv_r \operatorname{sen}\frac{2\pi t}{T},$$
$$v_B^2 = v^2 + v_r^2 + 2vv_r \operatorname{sen}\frac{2\pi t}{T}, \qquad (14)$$

dove $v = v_s + v_g$ e T è la lunghezza del giorno siderale.

Notiamo che le relazioni (14) sono corrette anche quando l'asse terrestre non è perpendicolare all'eclittica (come è la situazione reale), ma solo durante i solstizi.

Quando per l'orologio A sia mezzogiorno e per l'orologio B mezzanotte (vale a dire per t = T/4) le loro velocità assolute saranno

$$v_A = v - v_r, \quad v_B = v + v_r, \qquad (15)$$

quando per l'orologio A sia mezzanotte e per l'orologio B mezzogiorno (vale a dire per t = 3T/4) le loro velocità assolute saranno

$$v_A = v + v_r, \quad v_B = v - v_r, \qquad (16)$$

e quando per i nostri orologi saranno l'alba ed il tramonto (vale a dire per t = 0 e t = T/2) le loro velocità assolute saranno

$$v_A = v_B = (v^2 + v_r)^{\frac{1}{2}}, \qquad (17)$$

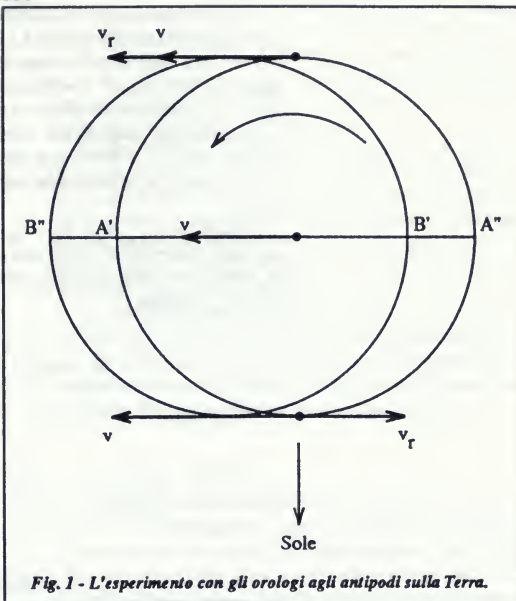Se usiamo le ultime tre formule nella (7), vediamo come i corsi dei nostri orologi siano diversi durante le diverse ore del giorno e coincidano solo durante la levata ed il tramonto del Sole.

Se possiamo confrontare le indicazioni dei nostri orologi **ad intervalli uguali di tempo** fra l'alba ed il tramonto, possiamo stabilire il carattere assoluto della dilatazione del tempo e misurare la velocità assoluta della Terra. Questa è l'essenza dell'esperimento con gli orologi agli antipodi.

Una realizzazione della sincronizzazione newtoniana fra due luoghi equatoriali agli antipodi appare problematica (v. finale dell'articolo) e per facilitarla montiamo i nostri orologi atomici su di una giostra (fig. 2).
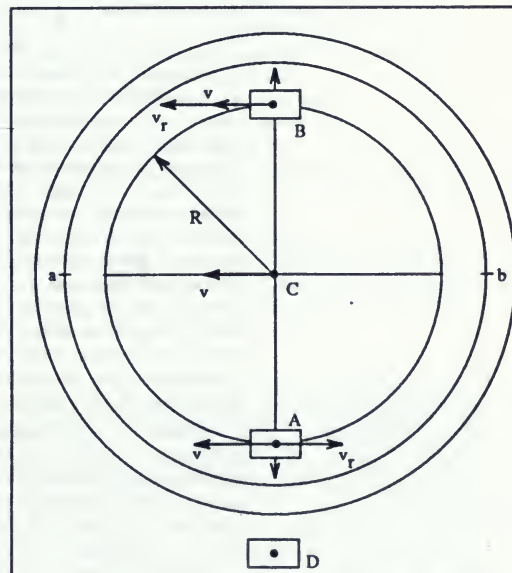

Fig. 2 - L'esperimento con gli orologi agli antipodi sulla giostra.

Consideriamo dunque una giostra al bordo della quale sono posti agli antipodi due orologi atomici A e B. Sia v la velocità assoluta della giostra e v_r la velocità di rotazione lineare dei punti al bordo ad una distanza R dal centro C. Prendiamo anche un anello che circonda la giostra e sta fermo nel laboratorio. Supponiamo che i punti a e b siano su di un diametro parallelo alla velocità assoluta del laboratorio. Designamo con t'_A, t''_A le indicazioni dell'orologio A al passaggio davanti ad a e b, e con t'_B, t''_B le indicazioni dell'orologio B al passaggio davanti a b ed a.

I tempi $\Delta t_A = t''_A - t'_A$ e $\Delta t_B = t''_B - t'_B$ corrispondono, ovviamente, allo stesso intervallo di tempo universale che designamo con Δt. Se Δt_D sarà il tempo registrato sull'orologio D che corrisponde ai tempi Δt_A e Δt_B, avremo

$$\Delta t = \Delta t_D \left(1 - \frac{v^2}{c^2}\right)^{-\frac{1}{2}} . \qquad (18)$$

D'altra parte

$$\Delta t = \int_0^{\Delta t_A} \left(1 - \frac{v_A^2}{c^2}\right)^{-\frac{1}{2}} dt \ , \quad \Delta t = \int_0^{\Delta t_B} \left(1 - \frac{v_B^2}{c^2}\right)^{-\frac{1}{2}} dt \ , \qquad (19)$$

Ponendo qui v_A, v_B dalla formula (14) e operando con un'esattezza del secondo ordine, otteniamo

$$\Delta t_A \left[1 + \frac{v^2 + v_r^2}{2c^2} - \frac{2vv_r}{\pi c^2}\right] = \Delta t_B \left[1 + \frac{v^2 + v_r^2}{2c^2} + \frac{2vv_r}{\pi c^2}\right] , \qquad (20)$$

avendo assunto per approssimazione

$$\Delta t_A \cong \Delta t_B \cong \Delta t = \frac{T}{2} , \qquad (21)$$

ove T è il periodo della rotazione.

Se scriviamo $\delta t = \Delta t_A - \Delta t_B$ e prendiamo in considerazione la (21) solo nei termini del secondo ordine, dalla (20) otteniamo

$$\delta t = \frac{4vv_r}{\pi c^2} \Delta t = \frac{4vR}{c^2} \qquad (22)$$

Assumendo v = 300 km/sec ed R = 3 m, arriviamo a $\delta t = 4 \times 10^{-11}$ sec. Supponendo che la giostra giri con 5 rivoluzioni al secondo (vale a dire $\Delta t = 0,1$ sec), troviamo $\delta t/\Delta t = 4 \times 10^{-10}$, mentre gli orologi al cesio usabili misurano il tempo con una esattezza relativa di $\pm 10^{-13}$.

La sincronizzazione newtoniana è una sincronizzazione momentanea. Una tale sincronizzazione l'ho realizzata con il mio asse rotante quando ho misurato la velocità della luce in una direzione (Marinov, 1974, 1980, 1986). Se tale sincronizzazione è effettuata con segnali ottici la chiamo sincronizzazione einsteiniana e in questo caso non è momentanea.

Ho dimostrato dianzi che l'esperimento con gli orologi agli antipodi produce un effetto positivo se la sincronizzazione degli orologi è newtoniana. Dimostrerò ora che se la sincronizzazione è einsteiniana non vi sarà effetto positivo, non sarà quindi possibile stabilire che gli orologi hanno corsi diversi.

Analizziamo la sincronizzazione einsteiniana riprendendo la figura 1. Supponiamo che l'orologio A trasmetta un segnale elettromagnetico quando si trova nel punto A' (cioè quando per questo orologio si leva il Sole) e designamo la sua indicazione con t'_A. Poiché la luce si muove con velocità finita, l'orologio B si muoverà con la Terra e riceverà il segnale quando si troverà nel punto B'.

Ora non prendiamo in considerazione la rotazione della Terra, in quanto - in questo caso - la velocità rotazionale della Terra v_r è perpendicolare alla velocità traslatoria v e, come si può dimostrare,

l'effetto prodotto dalla v_r sarebbe minimo e trascurabile rispetto al risultato finale.

Supponiamo, per semplicità, che il segnale elettromagnetico si stia propagando lungo il diametro terrestre la cui lunghezza è d = 2R. Si può dimostrare che se il segnale si sta propagando sulla superficie terrestre (come avviene realmente) gli effetti addizionali saranno cancellati dal risultato finale.

Nello spazio assoluto il segnale elettromagnetico coprirà la seguente distanza

$$d' = d\left(1 - \frac{v}{c}\right), \qquad (23)$$

che ho calcolato con una esattezza del primo ordine in quanto, come vedremo più avanti [formula (26)], un'esattezza maggiore non è necessaria.

L'orologio A trasmette il segnale finale quando si trova nel punto A" (cioè quando per esso tramonta il Sole) e designamo la sua indicazione con t'_A. L'orologio B si muove con la Terra e riceve questo segnale nel punto B". Nello spazio assoluto il segnale coprirà la distanza

$$d'' = d\left(1 + \frac{v}{c}\right), \qquad (24)$$

Designamo l'indicazione dell'orologio B con t'_B quando l'orologio A ha trasmesso il segnale iniziale e con t''_B quando l'orologio A ha trasmesso il segnale finale. Usando le formule (19), (20) e (21) possiamo scrivere

$$t''_A - t'_A = (t''_B - t'_B)\left(1 + \frac{4vv_r}{\pi c^2}\right) = t''_B - t'_B + \frac{2dv}{c^2}. \qquad (25)$$

Se designamo con T'_B e T''_B le indicazioni dell'orologio B al momento in cui sono stati ricevuti i segnali iniziale e finale, usando le (23) e (24) otteniamo

$$t'_B = T'_B - \left(\frac{d}{c}\right)\left(1 - \frac{v}{c}\right), \quad t''_B = T''_B - \left(\frac{d}{c}\right)\left(1 + \frac{v}{c}\right), \qquad (26)$$

Ponendo (26) nella (25) otteniamo

$$t''_A - t'_A = T''_B - T'_B, \qquad (27)$$

e ovviamente le indicazioni dell'orologio A, al momento della trasmissione dei segnali di luce, e dell'orologio B, al momento della loro ricezione, non ci consentono di stabilire la velocità assoluta della Terra.

Si deve sottolineare che l'esperimento di Briatore-Leschiutta, eseguito in modo simile, ha avuto un risultato positivo in quanto per la velocità rotativa degli orologi attorno all'asse l'effetto è additivo e può essere misurato per molte rotazioni.

Ora propongo una variante dell'esperimento con gli orologi agli antipodi che permette di misurare la componente equatoriale della velocità assoluta della Terra.

Prendiamo due telescopi, in un osservatorio posto all'equatore, con assi che possono alzarsi ed inclinarsi solamente sul piano parallelo alla direzione est-ovest.

Supponiamo che una stella equatoriale A - che si trova a 90° dalla proiezione dell'apice della velocità assoluta della Terra sull'equatore celeste - attraversi la linea di vista del primo telescopio nell'istante t'_A - indicato da un orologio preciso - e la linea di vista del secondo telescopio - il cui asse forma con l'asse del primo un angolo $\theta \cong \pi$ - nell'istante t''_A.

Supponiamo ora che una stella B, agli antipodi della stella A, attraversi la linea di vista del primo telescopio nell'istante t'_B e la linea di vista del secondo nell'istante t''_B.

Essendo la rotazione terrestre uniforme, i tempi $\Delta t_A = t''_A - t'_A$

e $\Delta t_B = t''_B - t'_B$ corrisponderanno allo stesso intervallo di tempo universale Δt, e potremo usare le formule (19), nelle quali v_A e v_B sono ricavati dalle (14). Introduciamo la designazione: $\delta t = \Delta t_A - \Delta t_B$. Se eseguiamo un'analisi come sopra otterremo il risultato (22). Ponendo in (22) v = 300 km/sec ed R = 6370 km arriviamo al risultato $\delta t = 8,5 \times 10^{-5}$ sec.

Se tale esperimento fosse eseguito nel corso di un anno, prendendo come "giostra" non la rotazione diurna della Terra attorno al suo asse ma la rotazione annuale attorno al Sole, nella (22) dovremmo assumere come v la componente della velocità assoluta del Sole sul piano dell'eclittica, ed il raggio dell'orbita terrestre come R. Prendendo dunque v = 300 km/sec e R = 150×10^6 km, troviamo un effetto $\delta t = 2$ sec.

Essendo l'ascensione retta dell'apice della velocità assoluta del Sole 13^{h}20^m (Marinov, 1977, 1986 p.76), se la approssimiamo a 12^h, possiamo concludere, effettuando le misure con un **orologio attaccato alla Terra, che la primavera e l'estate sono più lunghe di due secondi dell'autunno e dell'inverno.**

(*) *I risultati ottenuti dai singoli orologi differivano esattamente di 300 nanosecondi. Questo risultato, assurdamente ottimistico, venne accettato e gli venne dato ampio risalto nella letteratura scientifica e attraverso i media come conferma del paradosso degli orologi. Tutto quello che l'esperimento ha mostrato è che gli orologi non erano sufficientemente precisi per rilevare il piccolo risultato predetto.*

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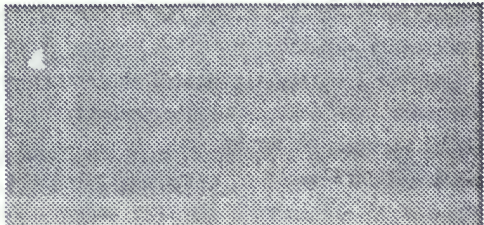
THE PLASMA GENERATOR OF FREE ENERGY HELIOS OF CYRIL CHUKANOV

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Abstract. In the last years many voices are heard which assert that plasma may
deliver more energy than the energy which is needed for its excitation
(heating). In the present paper I give information on the plasma heater of the
Bulgarian inventor Dr. Cyril Chukanov who asserts that his plasma heater is an
over-unity converter. I did measurements together with Chukanov. The observa-
tional data were uncertain and thus they were not convincing that his genera-
tor is, indeed, an over-unity converter. In the following paper of Farsky and
Janca on very similar energetic measurements with plasma heaters, where the ob-
servational data seem to be pretty certain, no energy excess has been observed.
Nevertheless I consider the plasma generators as an eventual source of free
energy deserving attention. After the paper of Farsky and Janca I give a re-
port of A. Samokhin on the plasma generator of the Russian inventor, Prof. A.
Chernetski, who also asserts of having observed an excess of energy.

1. Perpetua mobilia and energy converters

According to the energy conservation law, the total energy of an isolated material
system remains constant. As in the last time violations of the energy conservation law
have been observed, if the total energy of an isolated material system increases when
certain processes occur in the system, the excess of energy over the initial value is
called "winned energy" (or "FREE ENERGY"). If the total energy decreases, the deficiency
of energy is called "lost energy".

At the present time we cannot say with surety whether free energy is produced
from nothing, or it exists in the system, but we are not able to establish experimentally
its existence. Until the time when we will remain unable to establish experimentally
the previous existence of the winned in the system energy, we shall ACCEPT that the
winned energy is produced from NOTHING. In this case we have to accept the invalidity
of the energy conservation law.

If in the future methods will be found which would show that free energy exists
in "latent condition" previously in the system, i.e., if we have observed only some
energy transformation, we have to accept that the energy conservation law remains valid.
So at the beginning of the century one has supposed that the energy liberated at the
radioactive decay is produced from nothing, but later it was established that the ra-
dioactivity is only a process of energy transformation, as the liberated energy is the
previously existing potential energy of atomic bonds.

The possibility for producing free energy is demonstrated experimentally by diverse
machines. The most categoric demonstration for generation of free energy offers the
Swiss machine TESTATIKA (see TWT-V) which, if once set in rotational motion by an ini-
tial mechanic impulse, continues to rotate eternally and produces free energy in the
form of direct current electric energy. The free power, which the machine TESTATIKA

of middle size (with diameter of the counter-rotating disks of 50 cm) produces, is about 3 kW.

Thus the question whether a perpetuum mobile can be constructed is no more a question, as TESTATIKA IS a perpetuum mobile (it is the ONLY perpetuum mobile seen and tested by me and, according to my competent opinion, it is at present time the ONLY perpetuum mobile in the world). The machine TESTATIKA can function in vacuum and thus can be mounted on a satellite in the cosmic space, where it will present a completely isolated material system. During its functioning neither chemical processes do occur in the materials of which it is constructed nor some irreversible mechanical processes, besides a minimal mechanical wear due to the friction and to the extraction of electrons from the metal surfaces of the segmets which are in the electric circuit of the machine. Meanwhile the 3 kW electric power can be continuousiy extracted from the machine.

As energy can be produced from nothing, it is logical to assume that energy can also be transformed into nothing. I do not know about experimental observations of such energy "transformations", but I am sure that in many of the experiments where we assume that the whole "dissipated" energy has become heat energy, very probably, some part disappears, so that the quantity of dissipated energy is more than the quantity of the produced heat energy. The fact is that nobody until now has succeeded in demonstrating this experimentally.

So in Bulgaria some 20 years ago, I did many energetic experiments observing percussions of solid bodies and I became convinced that at the inelastic collisions there was no complete transformation of the lost kinetic energy into heat, as it is required by the energy conservation law. According to my opinion a part of the kinetic energy (very probably, even the overwhelming part) of the dissipated kinetic energy transformed into nothing.

My two machines violating the law of angular momentum conservation, the Bul-Cub Machine without Stator (see TWT-III) and the Rotating Ampere Bridge with Displacement Current (see TWT-IV) clearly demonstrate that angular momentum can be produced from nothing and angular momentum can be reduced to nothing.

Thus the production of free energy is an experimental fact, and if the overwhelming part of the physicists in the world assert that this is not possible, the reason is only one: all these people are not acquainted with some experimental facts.

I repeat, I assume that free energy is produced from nothing. If in the future some energetic source will be discovered, it will be characterized with the necessary physical parameters and a relevant name will be given to it. However to give NOW to such a HYPOTHETICAL energetic source such names as "vacuum energy", "space energy", "zero-point energy", "tachyon energy", "neutrino energy", etc. is not only completely sensless linguistic equilibristic but also a dangerous aberration.

If the system producing free energy represents a machine to which a power, P_{in}, is put in and a power, P_{out} goes out, it is called a converter. If $P_{out} < P_{in}$, as it is

the case with all known to humanity machines, it is called a below-unity converter. If $P_{out} > P_{in}$, because free energy is produced in the machine from nothing, it is called an over-unity converter.

The perpetuum mobile is an over-unity converter where a part, equal to P_{in}, from the power P_{out} is returned to the entrance, so that the machine maintains alone its operation, and a power $P_{out} - P_{in}$ can be extracted from the machine in the form of free energy. If when returning energy form the exit of the machine to its entrence there is some dissipation of energy, the free power which can be extracted is less than $P_{out} - P_{in}$.

2. The machine of Dr. Chukanov and my contacts with him

The Bulgarian inventor Dr. Cyril Chukanov has constructed a converter which according to his opinion is an over-unity converter. After hearing about this machine, I contacted Dr. Chukanov who showed an interest to accept me, so that I can become acquainted with his converter in action and draw my conclusions alone. Preliminary I sent to Dr. Chukanov my colleague, Dr. Maria Medareva, who saw the converter in action and warmly recomended it to me.

I visited Dr. Chukanov in September 1990 for several times in his house (ul. Dreneto
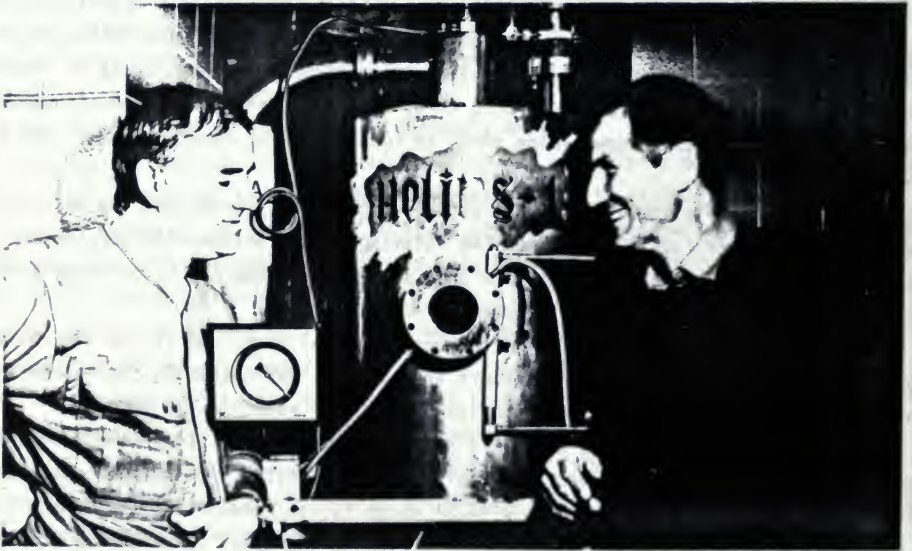

Fig. 1. Chukanov and Marinov with the converter HELIOS 1. One sees the looking hole through which one can look in the chamber of the plasma generator. The manometer at the left measures the pressure in the chamber.

13, Sofia - Boiana, Bulgaria) and he kindly presented first his theoretical concepts and then showed both his machines: Helios 1 (fig. 1) and Helios 2 (fig. 2). We did also experiments   heating the plasma in a flask (fig. 3) where the heat was delivered to a rod electrode and could draw conclusions about the produced heat directly by looking at the quick glowing of the electrode. Helios 2 was set in operation and I could carry out some measurements of P_{in} and P_{out}.

The machines of Chukanov are plasma generators of heat, where the plasma is excited (heated) by high-frequency electromagnetic energy (tenths of MHz). Dr. Chukanov believes that the plasma which he produces at pressures of parts of millibar to several millibar in the chamber of 120 l (Helios 1) and of 4 l (Helios 2), as well as in the flask, has the characteristic features of a ball-lightning.

3. The ball-lightning

A ball-lightning can be observed only under natural conditions, for short time and very seldom. Until the present time nobody has succeeded to produce a ball-lightning artificially. For this reason there is no some satisfactory theory on the ball-lightning and some of the concepts on it are excluding mutually one another.

The ball-lightning appears (I repeat, very rare) after linear lightnings, which all

Fig. 2. Chukanov with the converter HELIOS 2.

of us have seen at storms. The ball-lightning is a spherical (more rare a pear-form) gaseous body which lights readly, surrounded by a blue domain with unsharp contour. In certain cases the sphere can be glaring white with a clearly defined surface. According to some, the ball-lightnings move following the air currents, according to other, under the action of the existing electromagnetic fields, as a rule in parallel to the earth's surface. It is possible that the ball-lightning passes through a narrow orifice in the form of a string and then again it acquires its spherical shape. The ball-lightning can disappear as inconspicuously as it has appeared, or with a rumbling detonation. If it "falls" in a water basin, it can evaporate a considerable quantity of the water. To this fact Dr. Chukanov pays a special attention, as the ionization energy of the ball-lightning can in no way be sufficient to cover the heat energy of the evaporated water (let us not forget that the thermal action of the linear lightnings is due to the conspicuous currents which flow between "cloud" and "earth" where the potential differences are millions of volt).

According to Dr. Chukanov, the ball-lightning is a specific quantum plasma state (complete quantum object) as sometimes it reacts to external actions not as a gaseous sphere but as a solid body (one has observed that by hitting a ball-lightning by a stick, the former bounced as a solid body). According to Dr. Chukanov, the electrons

Fig. 3. The flask with the heated plasma of Chukanov. One sees on the right the metal spiral along which the high - frequency electromagnetic energy produced by the lamp generator (of which a small part is seen) is directed via the rod electrode to the gas at the middle of the flask.

in the ball-lightning exist long time separated from the gas ions; they do not recombine, but the electric neutrality of the sphere as a whole is preserved. On the other hand, the ball-lightning radiates light and electromagnetic energy without demonstrating some changes, thus the radiated by the ball-lightning energy must be thoroughly free energy. The last important generalization which Dr. Chukanov makes is that our Sun is nothing else than a ball-lightning.

First the Russian scientist P. Kapitza came to the conclusion that by the help of high-frequency electromagnetic energy, conducted to a closed vessel, one can obtain hot plasma which can be considered as a ball-lightning. In his Nobel speech (1978), Kapitza said the following /П. Л. Капица, ЭКСПЕРИМЕНТ, ТЕОРИЯ, ПРАКТИКА, изд. "Наука", Москва, 1987, стр. 108/:

... мы случайно нашли явление, при котором получалась горячая плазма. Нами разрабатывался мощный высокочастотный генератор непрерывного действия. В результате был осуществлен прибор, генерирующий высокую частоту при длине волны 20 см /частота 1500 Мхц/ с высоким к.п.д. и мощностью в несколько сот ват... Этот генератор был нами назван "Нинотрон". В процессе разработки этого генератора, начиная с 1950 г., при испытании одной из его моделей мы пропускали его излучение через кварцевый шар, наполненный гелием при давлении 100 мм рт. ст. /150 мбар/. При этом в нем вспыхнуло свечение, которое имело четкие границы. Все явление наблюдалось несколько секунд, так как на одном месте шар проплавился.

Эти наблюдения привели к мысли, что шаровая молния - тоже явление, создаваемое высокочастотными колебаниями, возникающими в грозовых облаках после обычной молнии. Эта гипотеза была опубликована в 1955 г. /П. Л. Капица, О природе шаровой молнии - ДАН СССР, 1955, т. 101, стр. 254/. Через несколько лет у нас появилась возможность возобновить эти опыты. В марте 1958 г. уже в шаровом резонаторе, наполненном гелием при атмосферном давлении, в резонансном режиме при интенсивных непрерывных колебаниях типа H_{01} возникал свободно парящий газовый разряд овальной формы. Этот разряд образовывался в области максимума электрического поля и медленно двигался по кругу, совпадающему с силовой линией.

Мы стали изучать более подробно такой тип разрядов, исходя из того, что плазма в этих разрядах непосредственно не соприкасалась со стенками резонатора, и мы предположили, что при этом плазма могла бы иметь высокую температуру. В продолжении нескольких лет мы изучали это интересное явление в различных газах при давлениях, достигавших десятков киловатт, и, конечно, также изучалось влияние на разряд магнитного поля, достигавшего в наших опытах 25 килоэрстед /2.5 Т/.

Translation:

... we accidentally found a phenomenon at which a hot plasma was obtained. We were working on a powerful high-frequency generator for continuous action. As a result, we constructed an apparatus which generated high frequency at a wavelength 20 cm (frequency 1500 MHz) with high efficiency of some hundreds kW... We called this

generator "Nigotron". During the process of perfection of this generator, some-
where in 1950, when testing one of its variations, we let its radiation pass through
a quartz sphere, filled with helium under pressure of 100 mm Hg (150 mbar). A
shine has gleamed which had sharp boundaries. The phenomenon was observed for
a couple of seconds, as at one spot the quartz sphere melted.

These observations led to the thought that the ball-lightning is also a pheno-
menon produced by high-frequency oscillations which appear in the stormy clouds
after the usual lightnings. In this way energy is conducted, necessary for the
longer maintenance of the shine of the ball-lightning. This hypothesis was pub-
lished in 1955 (П. Л. Капица, О природи шаровой молнии - ДАН СССР, 1955, т. 101,
стр. 254). After several years we had the possibility to resume these experiments.
In March 1958, now in a spherical resonator filled with helium at atmospheric
pressure, at resonance of intensive uninterupted oscillations of the kind H_{01}, a
freely hovering gas discharge of an oval form has appeared. This discharge was
located at the domains of maxima of the electric field and slowly moved around a
circle coinciding with the force line.

We began to study more attentively this type of discharges, proceeding from the
observation that the plasma in such discharges did not contact directly the walls
of the resonator and we supposed that the plasma could have a high temperature.
During several years we studied this interesting phenomenon in diverse gases at
pressures until tenths of atmospheres and at powers arriving until tenths of kW,
and, of course, we studied also the action on the discharge of the magnetic field,
arriving at intensities until 25 kilo-gauss (2.5 T).

The experiments of Kapitza and Chukanov are almost the same but Kapitza has not mea-
sured the heat energy produced by the "hot plasma" and Kapitza has not found a way to
make the production of this heat the highest possible. According to me, the discovery
of Chukanov consists exactly in this. And Chukanov tries to demonstrate that the heat
energy produced in his converter Helios superates the electric energy consumed for the
plasma excitation. Thus according to the firm conviction of Chukanov, the "hot plasma"
produced in his converter Helios which, he thinks, is a kind of ball-lightning, produ-
ces free energy.

4. Chukanov's plasma converter HELIOS 2

I shall describe in more detail the plasma converter HELIOS 2, of whose input and
output powers I did measurements (figs. 2 and 4).

The high-frequency lamp generator consumes nominal power 6 kW from the mains. It
has a fan for cooling the heat produced in the transformer for enhancing the tension,
as well as in the oscillating circuit. It was constructed in the Bulgarian town Gab-
rovo. The transformed tension is rectified and feeds the generator lamp and the oscil-
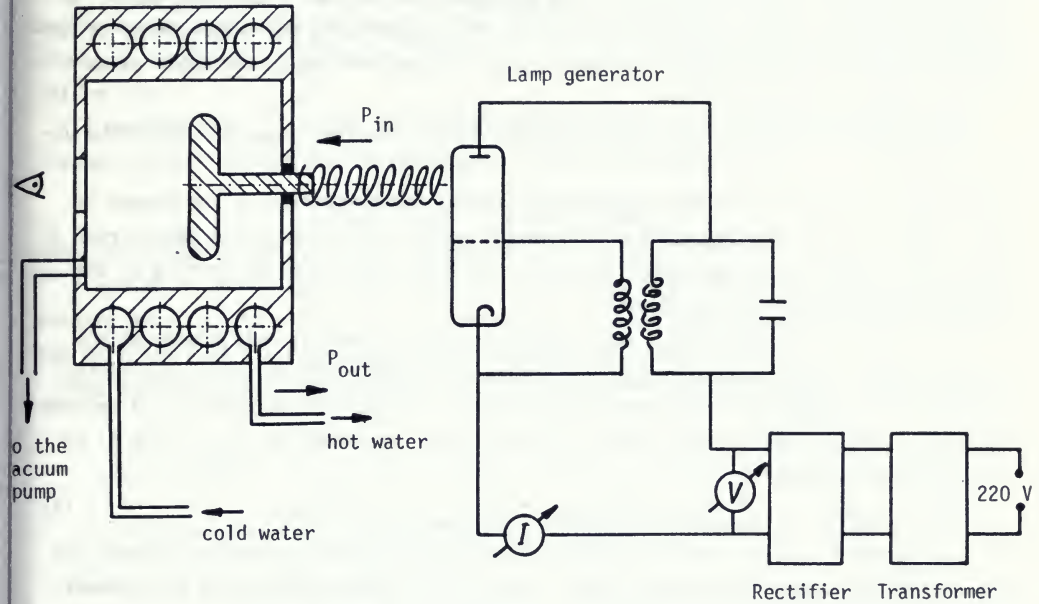lating circuit (which is the load in the anode circuit of the lamp) with a tension

Fig. 4. Diagram of the plasma converter HELIOS 2.

U = 4.3 kV. At no-load operation, i.e., when high-frequency electromagnetic energy is not conducted to the plasma chamber, the direct current consumed by the generator is $I_{no-load}$ = 0.4 A, and thus the no-load power consumed by the generator is

$$P_{no-load} = UI_{no-load} = 1.72 \text{ kW.} \qquad (1)$$

The electromagnetic oscillations with a frequency of 27.12 MHz are conducted to the plasma chamber along a metal spiral of about 10 windings, with diameter of about 10 cm and thickness of the wire of about 2 mm (see this spiral in fig. 3 on the right). One end of the spiral contacts the metal electrode which has the form of a mushroom and its "leg", relevantly insulated, goes out of the chamber. The other end of the spiral is free and the degree of the electromagnetic coupling with the oscillating circuit is established by putting this free end at different disctance with respect to the lamp generator. Thus the coupling with the oscillating circuit is inductive. At inductive coupling (no radiation) the transfer of electromagnetic energy is pretty good.

The chamber has a looking hole, through which one can observe the state of the plasma visually or by the help of a spectroscope (spectroscopic measurements have given for the "hot plasma" temperature of the order of $10,000^{\circ}$ C).

The chamber is evacuated by a rotational pump. For obtaining a higher evacuation, there is also a turbomolecular pump of the company Leybold-Heraeus. We set in action

only the rotational pump as the best effect is observed at a pressure of several milli-
bar. Chukanov has filled the chamber with hydrogene and helium. With these gases, he
told me, the efficiency is higher (this is to be compared with the measurements of Far-
sky+Janca in the following paper who have found a lower efficiency for noble gases -
see the graphs in their fig. 4).

The chamber is covered with a metal jacket in which cooling water may circulate un-
interrptedly.

At feeble coupling with the oscillating circuit, the luminosity of the plasma is
also feeble and has the character of a glow-discharge. At this coupling the current in
the anode circuit of the generator increases insignificantly and the heating of the
cooling water is also insignificant.

At a strong coupling with the oscillating circuit, a luminous oval plasma object
appears between the circumference of the mushroom electrode and the metal cylindrical
surface of the chamber. This plasma object ignites and extinguishes every 2 - 3 seconds.
Now the current in the anode circuit of the generator increases to I_{load} = 0.9 A, so
that the load power was

$$P_{load} = U(I_{load} - I_{no-load}) = 2.15 \text{ kW}. \tag{2}$$

At a stationary regime (20 - 30 minutes after the production of the hot plasma), we
have measured the quantity of water which passed in a second and by reading the tempera-
tures of the entring and exiting water we calculated an output power

$$P_{out} = 1.55 \text{ kW}. \tag{3}$$

Chukanov assumes (taking into account data given in the literature) that of the po-
wer consumed by a high-frequency lamp generator no more than 0.5 parts, in general no
more than 0.3 parts, can be transferred to another circuit, i.e., that the efficiency
of a lamp generator, in general, can be no more than η_{gen} = 0.3. Chukanov calculated
the power put in the plasma converter as follows

$$(P_{in})_{Chuk} = \eta_{gen} P_{load} = 0.645 \text{ kW}. \tag{4}$$

At such calculations, comparing (3) and (4), Chukanov concluded that his converter
is an over-unity converter with efficiency

$$(\eta_{con})_{Chuk} = P_{out}/(P_{in})_{Chuk} = 2.4. \tag{5}$$

To the same conclusion have come 12 Bulgarian high-quality specialists who have
signed a protocol (see fig. 5) of the carried out by them measurements which coincided
with the measurements done by me. These persons are lecturers in Sofia high-schools and
workers in scientific institutes in Sofia.

According to me, the efficiency η_{gen} must be taken as the ratio of the output power
and the whole power consumed by the generator when it is loaded

$$P_{no-load} + P_{load} = UI_{load} = 3.87 \text{ kW}. \tag{6}$$

Thus, according to me, <u>almost</u> the whole power increase P_{load} = 2.15 kW has been transferred to the plasma. Of course, as there are inevitable losses, one can with certainty assume that only 1.55 kW have been transferred to the plasma, so that the efficiency of the plasma converter will be n_{con} = 1, as it is required by the energy conservation law.

If considering P_{out} = 1.55 kW as the energy transferred from the generator to the plasma, we shall obtain for the efficiency of the plasma generator

$$n_{gen} = P_{out}/(P_{no-load} + P_{load}) = 0.4, \tag{7}$$

which is a number coinciding with that given in the literature.

I repeat, I do not consider myself as a specialist for lamp generators and[I] shall emphasize once more that according to Chukanov and the 12 Bulgarian high-rank specialists the efficiency of his plasma converter is about 2.4 (see eq. (5)).

One must take also into account that the measurements were not exact. The thermal

ст.н.с. ктн инж. Тома Тончев,ВМЕИ"Е.И.Ленин".....

ст.н.с. ктн инж. Ангел Бохотски,ВМЕИ"В.И.Ленин"...

ст.н. с. ктн инж. Александър Бъркушков,ВМЕИ"Ленин"...

доц. ктн инж. Кирил Янев, ВМЕИ "В.И.Ленин".........

инж. Светлин Пенков,зам.ген.директор НПО-БАН........

ктн инж. Кирил Чуканов,ЦВП-БИСА...................

н.с. I ст. инж. Томи Маджаров,ВМЕИ "В.И.Ленин"...

н.с. Iст.,инж. Васил Симов,Институт по оптика.....

н.с. I ст. инж. Митко Мирчев,Институт по стъкло..

инж. Петър Радизоев,ЦВП-БИСА.....................

физик Иван Димитров, СУ "Кл.Охридски" - София ...

доц. ктн инж. Стефан Минков, ВМЕИ "В.И.Ленин".....

Fig. 5. Signatures under the affidavit of the Bulgarian specialists who have carried out measurements on HELIOS 2 and confirmed that its efficiency is 2.4.

power surely is bigger than the calculated one, as we have measured only the thermal power transferred to the cooling water. Meanwhile some part goes radiated in the surroundings from the metal jacket of the apparatus. On the other hand, some part of the electromagnetic energy extracted from the generator goes radiated in the surroundings via the metal jacket, as one clearly sees ionization of the air near the sharp ends o the jacket.

In fig. 6 is shown one of the mushroom electrodes which has been melted by the hot plasma.

Concluding I shall say that although being personally not convinced that Chukanov's plasma converter is an over-unity converter, I consider such energetic plasma research as very interesting.

5. Experimentation with the quartz-flask converter

The experiments which we did with the quartz-flask converter (fig. 3) were not quantitative, but they were highly impressing. After evacuating the flask to a pressure o several millibar and conducting to it the electromagnetic high-frequency energy as shown in fig. 3, a white plasma object appeared at the end of the rod electrode. The increase of the current in the anode circuit of the lamp generator was almost insignificant (much less than 0.1 A), however the electrode become redly glowing in some 10 - 20 sec (temperature about 1000° C).

Fig. 6. The electrode of HELIOS 2 melted by the hot plasma.

6. Chukanov, Prof. Ohtsuki, the American

Chukanov has sent information on his experiments to Prof. Y. H. Ohtsuki of the Department of Physics, Waseda University, Okubo-3, Shikjuku-ku, Tokyo 169, Japan. I read in August 1990 in the biggest Bulgaria journal ДУМА (THE WORD) that Prof. Ohtsuki has repeated Chukanov's experiments observing also the effects claimed by Chukanov. However Prof. Ohtsuki has not mentioned the name of Chukanov.

In June 1989 in Japan has met the first international symposium on ball-lightnings. In June 1990 the second symposium on ball-lightnings has met in Budapest, Hungary (Prof. Ohtsuki took part at these symposia). Chukanov had to speak on the second symposium but because at that time he had important talks in Holland, he could not attend it.

My colleague Dr. H. Aspden, who is an old specialist in ball-lightnings, has visited the second symposium and I gladly offer to him the pages of TWT-IX for an information on this symposium and for his view-point to the over-unity plasma converters.

Dr. Chukanov was invited by American industrialists to visit USA. In the first days of September he left for the States where now he is rebuilding his converter. His wife, Angelina, joint him on the 2 November 1990 and brought him some of his Bulgarian details for the   apparatus which now he builds in America.

According to Chukanov at the cold nuclear fusion there is no fusion at all. This is another way for producing free energy.
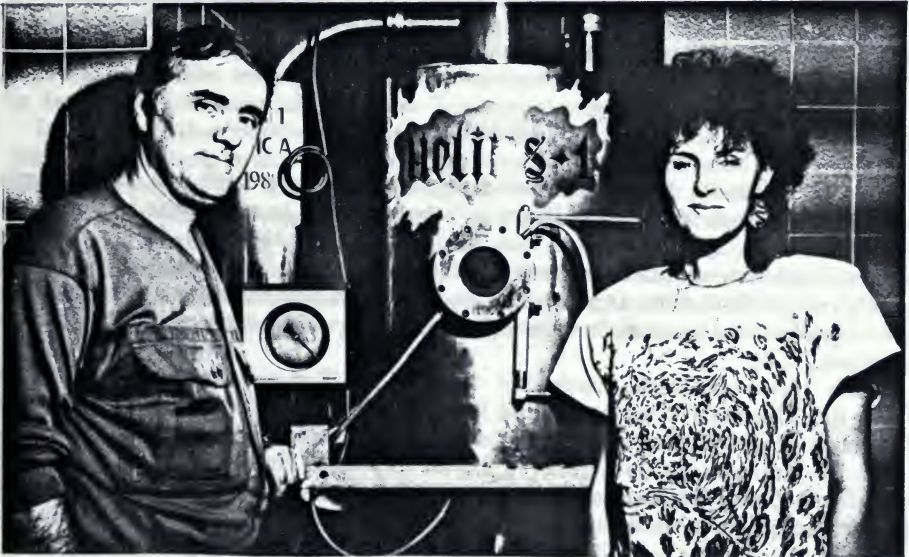

Fig. 7. Chukanov with his wife in front of the plasma converter HELIOS 1.

Energetische Verhältnisse im Plasma
einpoliger Hochfrequenzentladungen

Von

V. Farský und J. Janča

Energetic conditions in plasma of HF unipolar discharges.

In this paper a method and equipment for measurements of the total hf discharge power and its thermic power is presented. The measured difference of both powers is subjected to a theoretical analysis. Experimentally ascertained dependences are in good agreement with values, following from the theory. In molecular gases the supplied hf power is almost totally changed into thermic power. In monoatomic gases, in the contrary, the most part of the supplied power is changed into o from of electromagnetic emission irradiated from the plasma.

In der vorliegenden Arbeit werden eine Methode und eine Apparatur beschrieben, welche die Messung der Gesamtleistung einer Hochfrequenzentladung und ihrer thermischen Leistung ermöglicht. Der gemessene Unterschied der Leistungen wird einer theoretischen Analyse unterworfen. Die experimentell festgestellten Abhängigkeiten stimmen mit den theoretisch ermittelten Werten sehr gut überein.

In Molekulargasen wird die zugeführte Hochfrequenzleistung beinahe völlig in Wärme umgewandelt. In einatomigen Gasen wird demgegenüber der Hauptteil der zugeführten Leistung wieder in Form einer elektromagnetischen Strahlung vom Plasma zur Umgebung abgestrahlt.

Einleitung

Die hochfrequente Hochdruckentladung, welche öfters auch als „Fackelentladung" bezeichnet wird, ist in jüngster Vergangenheit immer mehr in der technischen Praxis eingesetzt worden. Am häufigsten wurde bisher [1, 2, 3, 4, 5] eine bei atmosphärischem Luftdruck und in Anwesenheit von Molekulargasen N_2, CO_2 und O_2 erregte Entladung angewendet. In der letzten Zeit wurde auch die Diagnostik einpoliger, in einatomigen Gasen (Ar, He) erregter Hochfrequenzentladungen betrieben [2, 6].

Durch spektroskopische Methoden wurden die Temperaturen des neutralen Gases, die Elektronentemperatur und ihre Konzentration bestimmt [1, 2, 6]. Mit Hilfe einer Hochfrequenzsonde wurde der Intensitätsverlauf des elektrischen Feldes im Entladungskanal gemessen [7]. Die an einpoligen Hochfrequenzentladungen bei hohem Druck ermittelten Resultate sind mit den an Zweielektrodenhochfrequenzentladungen bei hohem Druck gemessenen Werten vergleichbar [8].

Die Parameter der einpoligen Hochfrequenzentladungen hängen von der Frequenz der Erregerspannung, von der Gesamtleistung der Entladung, von der Geometrie und der Kühlung der Elektrode sowie von der Art und dem Druck des

V. Farský und J. Janča

Tabelle 1

Parameter der Entladung	Gas im Entladungsraum		
	Luft* [1, 2, 4, 10]	Argon** [2, 8, 18]	Helium** [6]
Gastemperatur T_{max} [$^{\circ}$K $\cdot 10^{+3}$]	3,8—4,2 (30 MHz) 5,3—5,6 (2400 MHz)	1,4—2,0	0,9
Elektronentemperatur T_e [$^{\circ}$K $\cdot 10^{+3}$]	4,5—6,0	6,0—8,0	25
Elektronenkonzentration N_e [cm^{-3}]	$5 \cdot 10^{10}$—10^{11}	$8 \cdot 10^{13}$—10^{14}	$5 \cdot 10^{14}$
Intensität des elektrischen Feldes E_e [V $\cdot$ cm^{-1}]	300—500	12—25	—

* In N_2, O_2 und CO_2 herrschen gleichartige Verhältnisse
* Die in Edelgasen gemessene Temperatur und alle übrigen Werte hängen weitgehend vom
* Verunreinigungsgrad durch Molekulargase ab.

Gases, in welchem die Entladung erregt wurde, ab. Eine Übersicht über die bisher ermittelten und veröffentlichten Werte wird in Tab. 1 wiedergegeben.

Die in dieser Tabelle angeführten Werte betreffen nur den sogenannten Entladungskanal. Alle bisher durchgeführten Messungen zeigen, daß sich T_{max}, T_e und N_e bei größer werdender Leistung praktisch nicht ändern. Bei ansteigender Leistung vergrößert sich nur das Volumen des Plasmas. Die Verhältnisse in der Elektrodenschicht und ihrer nächsten Umgebung sind sinngemäß komplizierter. E_e erreicht hier Werte bis zu 3000 V/cm [7]. Die Elektronentemperatur wird gleichfalls viel höher sein, wie die schon früher durchgeführten Messungen des Wertes T_e in der Fackelentladung mit Hilfe der Rauschspannungsmessung bezeugen [9].

In einigen früher veröffentlichten Arbeiten, z. B. [2, 9] wurde die Umwandlung der Gesamtleistung in eine thermische Leistung und eine elektrische Hochfrequenzleistung erwogen. Die Hochfrequenzleistung wurde mit dem leitenden Kanal der Entladung, welche die eigentliche Quelle der ausgestrahlten Hochfrequenzleistung darstellt, in Verbindung gebracht.

Die quantitative Auswertung dieses Hochfrequenzanteiles an der Gesamtleistung beschränkte sich bisher nur auf approximative Abschätzungen. Die Problematik wird im weiteren einer genaueren experimentellen und theoretischen Analyse unterworfen.

1. Meßmethode und Meßapparatur

Zur quantitativen Analyse der Aufteilung der Gesamtleistung in der Hochfrequenzentladung zwischen einer thermischen und einer elektrischen Leistung gemäß Gleichung

$$N_g = N_t + N_e \tag{1.1}$$

war es notwendig, eine Meßmethode zu entwickeln, welche die Ermittlung zweier von den drei angeführten Größen sicherstellen würde. In die Leistung N_e wird

auch die Lichtstrahlung einbezogen, da sie in ihrem Energieniveau unbedeutend ist. Sie beträgt maximal 1,5% der Gesamtleistung.

Ohne völlige Kenntnis des Frequenzspektrums der ausgestrahlten elektrischen Leistung ist eine direkte Messung der abgestrahlten Leistung praktisch nicht möglich. Zudem ist die Messung in geschlossenem Raum wertlos. Da das komplette
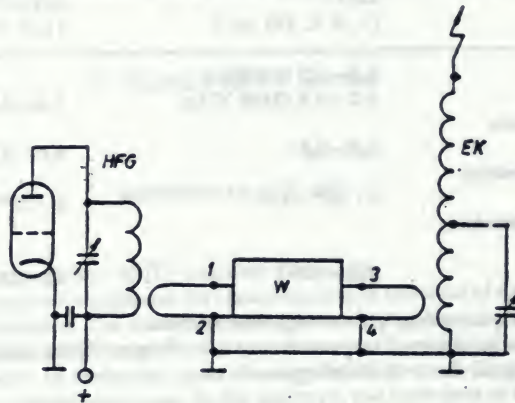

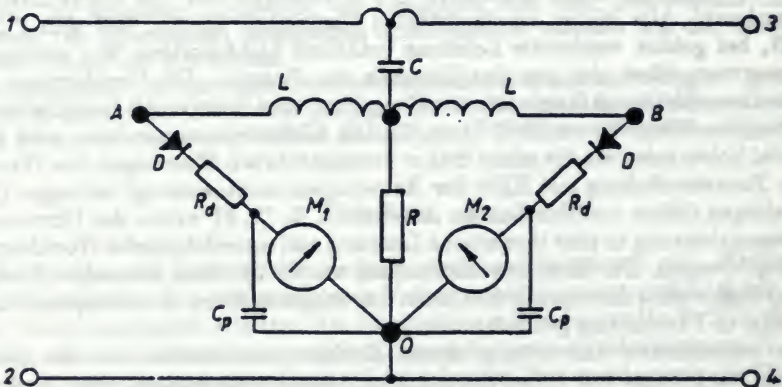Abb. 1. Einreihung des Hochfrequenzwattmeters in die Zuführungsleitung zwischen der Endstufe des Hochfrequenzgenerators HFG und dem Erregerkreis EK

Abb. 2. Netzwerkanordnung des Hochfrequenzwattmeters

Frequenzspektrum hochfrequenter Entladungen nicht zur Genüge bekannt ist und da die Messung von Hochfrequenzentladungen aus praktischen Gründen im freien Raum nicht durchführbar ist, mußte die Messung der Gesamtleistung und der thermischen Leistung sichergestellt werden.

Die Messung der Gesamtleistung wurde in den Arbeiten [11, 12] theoretisch, wie auch in der technischen Realisation gelöst. Die Messung der Hochfrequenzleistung wurde für technische Applikationen schon früher gelöst. Bekannt sind besonders die Methoden zur Messung der Leistung an Rundfunksendern [13, 14, 15]. Zur Leistungsmessung an Hochfrequenzentladungen konnten diese Methoden jedoch nicht direkt angewendet werden, da sie einen unveränderlichen Charakter und oft
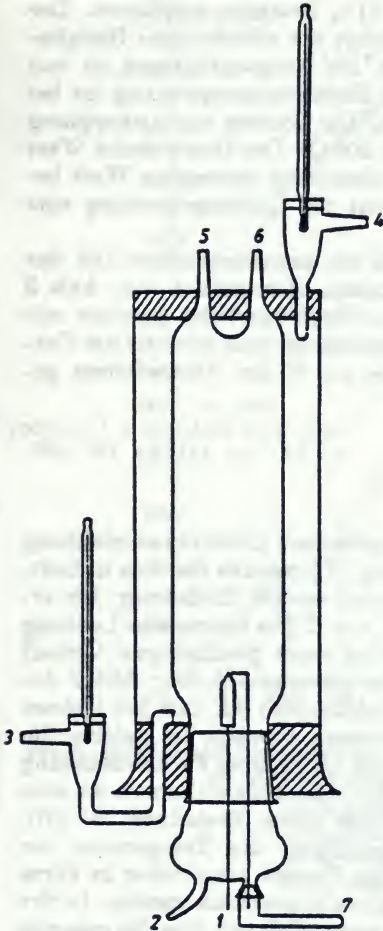
Abb. 3. Das Kalorimeter zur Messung der thermischen Leistung der Hochfrequenzentladung. Die in der Abbildung befindlichen Nummern bedeuten: 1 Zuführung zum Erregerkreis des HF-Generators, 2 zum Gasbehälter oder zur Saugpumpe, 3 Wasserzufuhr vom Behälter mit Überfall, 4 Wasserableitung zum Durchflußmesser, 5 zum Manometer, 6 Abführung des strömenden Gases bei Messungen mit Normaldruck, bei Messungen mit Unterdruck geschlossen, 7 Hilfselektrode zur Zündung der Entladung

auch eine unveränderliche Größe der Belastungsimpedanz, insbesondere der Antenne, voraussetzen. Bei Hochfrequenzentladungen ändern sich mit der Leistung der Charakter und die Größe der Ersatzimpedanz in weitem Bereich. Diese Änderungen, die ganz willkürlich verlaufen und die nicht immer durch Nachstimmung der Kreise eliminiert werden können, haben Phasenänderungen zur Folge, die bei größerem Phasenabbildungsfehler beträchtliche Fehler in die gemessene Leistung einführen können. Eine weitere Vorbedingung bei diesen Methoden ist ein harmonischer Verlauf der hochfrequenten Spannungen und Ströme. Diese Vorbedingung ist bei Leistungsmessungen, z. B. an Rundfunksendern, völlig berechtigt. Bisher war jedoch nicht bekannt, welche Verzerrung eine Hochfrequenzentladung verursacht, so daß auch eine Analyse ihres Einflusses auf die Genauigkeit der Leistungsmessung nicht möglich war.

Die entwickelte Meßmethode und Meßapparatur respektieren alle schon erwähnten Vorbedingungen, wie sie bei der Erregung von Hochfrequenzentladungen erscheinen.

Eine entsprechende Lösung befindet sich in ihrer Prinzipanordnung auf Abb. 1, wo HFG ein Generator mit entsprechenden Eigenschaften, W das eigentliche Wattmeter für Hochfrequenzleistungen in der Leitung mit geringer Impedanz sind. EK ist der Erregerkreis, an dem die Hochfrequenzentladung brennt. Der Stromlaufplan des Wattmeters befindet sich auf Abb. 2. Diese Anordnung wurde wegen ihres geringen Phasenfehlers und der geringen Empfindlichkeit für Verzerrungen gewählt. Der resultierende Wert der die Leitung passierenden Hochfrequenzleistung wird dann durch die Gleichung

$$N_s = \frac{e_1^2 - e_2^2}{4\omega^2 MRC} = \frac{e_1^2 - e_2^2}{K} \qquad (12)$$

bestimmt, wo e_1 und e_2 die Hochfrequenzspannungen zwischen den Punkten OA und OB des Hochfrequenzwattmeters bedeuten. M, R und C sind die Netzwerkparameter des Wattmeters, die Betriebsfrequenz ist $\omega = 2\pi f$. K bedeutet die Konstante des Wattmeters, welche zwar von den Netzwerkparametern abhängt, jedoch präzis durch eine Eichung der Apparatur feststellbar ist. Zur Eichung der Apparatur eignet sich sehr gut eine photometrische Methode.

Der Hochfrequenz-Leistungsgenerator wurde vierstufig konstruiert, seine Frequenz ist durch einen Schwingquarz mit $\pm 0{,}01\%$ Präzision stabilisiert. Der Endverstärker wird in Klasse C betrieben und liefert die erforderliche Hochfrequenzleistung bei einer Frequenz von 27,12 MHz.[*] Die Ausgangsleistung ist von 0 bis 350 W stetig regelbar. Die Verzerrung der Hochfrequenzspannung ist bei mittleren und großen Leistungen geringer als 2%. Die Leitung zur Ankopplung des Erregerkreises hat eine Impedanz von $Z_0 = 200\ \Omega$. Der theoretische Wert des Phasenabbildungsfehlers beträgt $1°24'$. Der tatsächlich gemessene Wert beträgt annähernd $5°$. Nach erfolgter Eichung weist die Leistungsmessung eine Genauigkeit von $\pm 5\%$, ± 5 W auf.

Zur Messung der thermischen Leistung wurde ein kalorimetrischer Teil der Apparatur entworfen und zusammengestellt, dessen Konzeption aus Abb. 3 ersichtlich ist. Die Eichung des kalorimetrischen Apparaturteiles geschah mit Hilfe einer Substitutionsmethode so, daß in den Entladungsraum anstelle der Entladung eine Heizspirale eingesetzt wurde, welche mit 50 Hz Wechselstrom gespeist war.

[*] Note that Chukanov's frequency (see p. 165) was EXACTLY THE SAME.

2. Meßresultate

Die Messung der der Entladung zugeführten gesamten Hochfrequenzleistung (N_g) und der thermischen Leistung der Entladung (N_t) geschah für eine in Luft, in N_2, H_2 und Ar, sowie bei verringertem Luftdruck erregte Entladung. Die ermittelten Meßresultate befinden sich auf Abb. 4a, b, c, d. Die thermische Leistung von in Molekulargasen erregten Entladungen weist einen gleichartigen Verlauf der Abhängigkeit von der gesamten zugeführten Leistung auf. Der Abfall der thermischen Leistung bei geringen Werten N_g erklärt sich so, daß bei kleinen Hochfrequenzleistungen nur eine Hochfrequenzkorona entsteht, in welcher die Temperatur des Neuraltgases wesentlich geringer ist als bei einer Fackelentladung [10]. Die Intensität des Hochfrequenzfeldes in der Hochfrequenzkorona ist sehr hoch (dies beweist besonders der Spektralcharakter dieser Entladung [5, 10]), ebenso übertrifft die Elektronentemperatur beachtlich die Temperatur der neutralen Moleküle, und ein beträchtlicher Teil der Energie kann daher in Form einer hochfrequenten, elektromagnetischen Strahlung abgestrahlt werden. In der in Stickstoff und Luft erregten Fackelentladung verwandelt sich fast die gesamte zugeführte Hochfrequenzenergie in Wärme. Eine etwas größere Streuung der bei Luft ermittelten Werte erklärt sich so, daß in der Entladung NO, NH, CN, O_3 entstehen und somit auch elektrochemische Reaktionen zustande kommen. Während der Messung ändert sich die NO-, NH-, CN- und O_3-Konzentration in der Entladung. In Wasserstoff ist im Hinblick auf die hohe Wärmeleitfähigkeit dieses Stoffes das Entladungsvolumen sehr gering, es ist bei Werten von $N_g < 200$ W wie eine Hochfrequenzkorona gestaltet. Hierdurch erklärt sich der Abhängigkeitsverlauf N_t von N_g in Abb. 4c. Bei Entladungen in Edelgasen ändern sich die Verhältnisse gänzlich (Abb. 4d – Ar). Mit steigender Leistung N_g vergrößert sich die thermische Leistung N_t nur sehr wenig.

Mit steigender Leistung verlängert sich nämlich in beachtlichem Maße der enge Entladungskanal und verhält sich dann wie eine Antenne [9]. Da im Argon die Elektronen ihre Energie an die Moleküle hauptsächlich mittels elastischer Stöße (Abschn. 3), d. h. sehr ineffektiv weitergeben, können die Elektronen nicht so schnell wie in Molekulargasen ihre Energie an die Moleküle weiter-
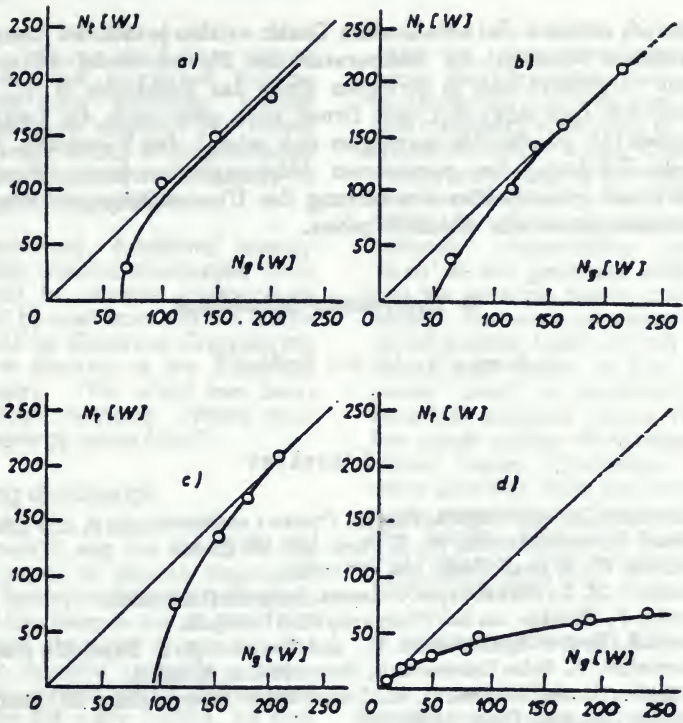
Abb. 4. Abhängigkeit der thermischen Leistung N_t von der Gesamtleistung N_g, die dem Plasma einpoliger Hochfrequenzladungen zugeführt wird

   a) In Luft erregte Entladung
   b) in Stickstoff erregte Entladung.
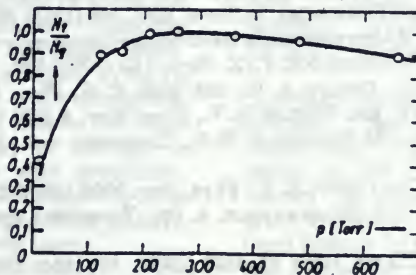   c) in Wasserstoff erregte Entladung
   d) in Argon erregte Entladung

Abb. 5. Relation N_t/N_g in Abhängigkeit vom Druck, bei welchem die einpolige Hochfrequenzentladung erregt wurde. In Luft erregte Entladung bei einer Leistung von $N_g = 80$ W

geben. Abb. 5 zeigt das Verhältnis N_t/N_g in Abhängigkeit vom Druck bei einer Entladung in Luft für $N_g = 80$ W. Der Wertanstieg von N_t/N_g im Bereich von 700—300 Torr wird so erklärt, daß gemäß Abb. 4 bei $N_g = 80$ W N_t nicht die

Werte von N_g erreicht. Bei abfallendem Druck werden jedoch die Reste der Hochfrequenzkorona beseitigt, die Temperatur des Plasma bleibt unverändert [16] und somit vergrößert sich in geringem Maße das Verhältnis N_t/N_g. Bei einem Druck von 200 Torr sinkt mit dem Druck auch sehr rasch die Temperatur des Neutralgases [4], gleichzeitig verringert sich schnell das Verhältnis N_t/N_g. Eine eingehende Erklärung der gemessenen Abhängigkeiten kann jedoch nur eine qualitative und quantitative Auswertung der Übertragungsgeschwindigkeit der Elektronenenergie auf die Moleküle geben.

3. Theoretische Behandlung

Pages 135 - 142 are omitted

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Vacuum energy:
a breakthrough

Classical physics cannot explain what happens when a plasma discharger placed in a Chernetskii circuit is started. For no apparent reason the ammeter pointer suddenly shows triple strength of current increase and energy output is several times more than input. The plant's efficiency is suddenly much more than 100% !

No magic is involved. Additional energy outputs at specific plasma discharges have been established in several independent "Expert Reports" by staff from the V.I. Lenin All-Union Institute of Electrical Engineering (Moscow) of the Ministry of the Electrical Equipment Industry. This effect has been checked by different methods. Where does this mysterious energy come from?

Self-generating discharge

Professor Chernetskii was the author of the first ever study paper on plasma diagnostic equipment, has twenty registered inventions to his credit and has been in the plasma field now for forty years. He never imagined what his probing into specific unstable plasma states at heavy current pulse discharges would lead to. In the early 1970's, he and fellow- researcher Yuri Gulkin worked on a fundamentally new type of high frequency plasma generator which, at high power, could do without unwieldy energy converters. In one test they suddenly discovered an input-output energy gap.

"I knew electron drift begins in plasma and sought to deduce a combination of variables in which fluctuating plasma instability emerged in discharge," Chernetskii says. "Gas- discharge plasma was meant to serve as a powerful stimulator of electromagnetic modes and, all of a sudden and in defiance of the law of conservation of energy, a strange energy imbalance was produced. Many experiments with different circuits proved that the energy output was always greater than the input in these cases."

The mysterious discharge stimulating additional energy extraction was called the self-generating discharge (SGD)". Measurements showed that part of the discharge power went back into the network as if two series-connected electromotive forces were at work.

The Tesla connection

In a bid to explain the experimental data, the researchers actually tried to prove the impossible and one of their proofs turned out to be very violent. The one megawatt substation at the Moscow Aviation Institute, where Chernetskii and Gulkin were staging an experiment with a powerful plasma unit, burned out. When the discharge currents reached criticality, superstrong current was "born" in the generator and went back into the network, playing havoc with the safety devices calculated for short circuit events. Later on, the two read in books that earlier in the century the power plant of prominent Yugoslav electrical engineer Nikola Tesla caught fire under similar circumstances in the United States. Chernetskii and Gulkin were sure that Tesla had carried out just such experiments, but did not publish the results. They are also convinced that vacuum energy can explain this mysterious effort.

A vacuum powered lamp

The researchers relied on the present-day quantum physics idea of "zero-point oscillations" in physical-vacuum. Such oscillations signify the birth and annihilation of virtual pairs, particle and antiparticle, distinguished from the normal elementary particles by a negligibly short lifetime of a mere 0.10×10 to the -21st power seconds. Emerging below the zero energy level from "nothing" and returning to "nothing", virtual particles seem to defy the law of conservation of energy.

Quantum electrodynamics explains this strange paradox through the Heisenberg Uncertainty principle under which all the precise particle features cannot be determined simultaneously and, therefore, one must not require from nature "punctual" abidance by the law in such a short time as the virtual pair lifetime. The observer fails to notice anything, while every virtual pair is more than real, carrying an energy of about half a million electron volts during its lifetime.

The energy potential in the electric bulb vacuum is enough to boil the Earth's

oceans, Americans John Wheeler and Richard Feynman have calculated. Until recently, the idea of vacuum energy was only a daring hypothesis bordering on science fiction.

The vacuum lamp powered in Chernetskii's basement laboratory in the center of Moscow has made it a reality. He explains his miraculous experiment thus: "The self-generating discharge emerges when the discharge currents reach a definite critical density, when the magnetic fields they create ensure magnetization of the plasma electrons and they begin to perform mainly cycloid movement. The interaction of currents with their magnetic field forces the electrons to deviate to the cylinder-shaped discharge axis and the electrical field emerges.

Polarized vacuum field

"It has been demonstrated that this switches on the physical vacuum. In this field the vacuum is polarized and consequently the virtual pairs begin to move in a definite direction, instead of chaotically. The virtual positrons accelerate plasma electrons, giving them part of their energy. The current in the circuit build up and additional energy is discharged on the resistor switched into the discharge circuit. Clearly, only part of the tremendous vacuum energy is extracted.

429% efficiency

"We've developed several circuit versions which can find application. In the latest experiment which had an input power of 700 watts, the generator produced three Kilowatts for (to) load the resistance, or nearly five times as much."

This is only the start and not the limit. The calculations for more powerful plants show that many megawatts of free electricity can be produced from a minimal power source.

Self-generating discharge (SGD) Plasmatron and space travel

Yuri Gulkin looks towards the future of their discovery: "It clearly has wide -ranging applications -- for example, engines based on the self-generating plasmatron . Until now, all attempts to use plasmatrons as engines have failed because strong electron and ion bombardment rapidly burns out the electrodes. SGD leaves them intact and such engines can be used to power aircraft, trains and automobiles. It would be logical to create a new environmentally-wise power industry. Portable hydroelectric, wind and solar battery power plants boosted by SGD could become an enormous source of electricity. With time, they would edge the costly, polluting and hazardous fuel-fired and hydraulic plants. Already we can now build an SGD plant which would supply electricity for a township or a factory. A vacuum power station comparable to the giant facilities now in existence could be designed from today. Our discovery could revolutionize cosmonautics."

Powering spaceships with 10 volts

Laboratory experiments have proved the possibility of using the kinetic effect of SGD for accelerating bodies in space. Gulkin has calculated the parameters of a SGD plasmatron that could serve as the propulsion engine of the future, replacing the present unwieldy and dangerous chemical rocket engines. Powered by a minor ten volt source, it could deliver power enough for the take-off of a large spaceship. Tapping the ambient space vacuum, it could fly across the cosmos eternally.

Unknown waves

"We didn't think of only industrial applications," Chernetskii recalls. "SGD turned out to produce wave radiation which was hitherto unknown, but evidently in existence at all times. Like sound waves, its waves have a longitudinal electric-field component and a high penetrability through conductive media, including metals. It turned out that these single waves, 'awakening' the hidden vacuum energy, can alter substance structure.

Biologically-sourced equivalents experiments

"The experiment was staged at the Burdenko Institute of Neurosurgery (Moscow) and showed that directional SGD radiation accelerated nuclear

beta-decay by 6.0%. This is only one result. We wondered whether longitudinal electric-field component waves like SGD-generator radiation could also be produced by living things, especially man. Comparative experiments were staged to establish the impact of individuals with extrasensory perception and of our device on different objects. In both cases, parallel impact on the high-stability quartz generator made its frequency 'drift' by several orders of magnitude. The filament resistance of the incandescent lamp decreased considerably. Probably we are on the road to explaining such mysteries as extrasensory perception, telekinesis and bioenergy."

Vacuum ether

Chernetskii's experimentally proven concept claims to be a theoretical breakthrough in the basic quantum physics idea of the energy structure of the universe. It is generally recognized among physicians that all elementary-particle interactions, and hence every [sic: all] existing phenomena, occur with the help of virtual-particle exchange. How does it occur?

"Full annihilation of virtual pairs cannot take place in the event of partial energy extraction in SGD, because a 'certain' virtual dipole must emerge -- two separate charges with a common negative energy. This means that together with energy extraction, vacuum structurization and ordering takes place. Actually, our concept is a return to the idea of the universal ether at an entirely new level. We say that the ordered dipole vacuum, or ether, is an all-penetrating energy medium in which processes occur which are related to virtual dipoles and subject to the uncertainty principle of quantum physics."

Chernetskii says, "If vacuum structurization is a constant process, isn't this an opportunity to state the Law of Conservation of Entropy in the universe in opposition to the idea of its steadfast and progressing increase? What if this work helps to clear up the nature of gravitation, which is as dim now as it was in the times of Newton? What if vacuum energy shows the road at last to the long-awaited Grand Unified Theory?"

"As an experimental physicist I won't say that our theory is 100% correct," Chernetskii cautions. "It's much more important now to have undisputed experimental data to establish the prospect of creating a fundamentally new power industry."

Self Generating Discharge (SGD) Plasmatron. In the early 1970's, they achieved a stable energy device where the output was considerably greater than the input, and that the superstrong current was not fed into the network.Only a rapid switch to new energy sources can save human civilization from ecological catastrophe. This research programme must be developed immediately and a Centre set up to coordinate cooperation on it between scientists worldwide. Natural and unlimited vacuum energy is at our doorstep.

Andrei Samokhin, Press-Agency
NOVOSTI, USSR

MARINOV'S COMMENTS ON THE PREVIOUS PAPER BY ANDREI SAMOKHIN

The report of A. Samokhin has been published in many world journals. I have read it in German and in Bulgarian. I reprint this report from the Newsletter of the Planetary Association for Clean Energy, edited by my friend A. Michrowski. The translation is bad but, as I have no time, I use it also in my book.

I spoke a couple of times on the phone with Prof. Chernetski. He invited me to visit him in Moscow (I intend to do this) and promised to demonstrate his generator in action so that I can persuade myself that the _electric_ input power is lower than the _electric_ output power. I asked him twice: "Input measured in electric watts and output measured in electric watts, too?" "Yes", answered Prof. Chernetski.

In the report of A. Samokhin there is plenty of uninteresting and irrelevant information, while the information-which the reader needs is pretty brief.

It is not said the way in which Prof. Chernetski excites (heats) the plasma. Is it a HOMOPOLAR plasma discharge (as is the case with the plasma generator of Chukanov and Farsky+Janca in the preceeding two papers), to which the high-frequency electromagnetic energy is conducted via a waveguide, or it is a BIPOLAR plasma discharge produced in a circuit along which high-frequency current flows. The assertion that "part of the discharge power went back into the network" speaks rather in favour of a bipolar plasma discharge.

The unnecessary information in Samokhin's article is the following:

1) Everyone who has done electromagnetic experiments has sometimes burnt out the fuses or even has produced a fire. It is comical to assert so many years that when once Tesla did so, this was a result of a very important physical discovery (which was then NEVER verified!). Helena Bonner said at the funerals of Sakharov when certain men, who a couple of years before were even afraid to greet Sakharov on the street, began to chant songs of praises in his glory: "Не делайте из Андрея икону." (Don't make an icon of Sakharov.) Neither of Tesla have we to make an icon. Tesla was a very clever man, he did many important inventions. But he has exaggerated many aspects of his research and very often he simply has lied (remember his apparatus with which he has "provoked an earthquake" and which he has destroyed with his own hands, or the car which he has "driven" stretching out only an antenna, etc. etc.). Thus to assert that Chernetski, by burning the fuses in the Moscow Aviation Institute, has replicated the "amazing" experiment of Tesla, is a miserable and bad taste literature.

2) At the present time we do not know with certainty whether Prof. Chernetski has violated the energy conservation law. Thus to search for an eventual explanation of this "violation" by "zero-point oscillations", by "virtual particles emerging from nothing and returning to nothing", by the "Heisenberg uncertainty principle", and so on is also a bad style literature. And when my friend John Wheeler and the late Richard Feynman have "calculated" that the energy stocked in a couple of cubic centimeters of vacuum might boil the water in the Earth's oceans, one can only say to them: "Boil it, boys, boil it."

3) According to Samokhin, the co-inventor of Chernetski, Yuri Gulkin, has said:"Already now we can build a Self-Generating Discharge (SGD) plant which would supply electricity for a township or a factory." Before bulding a plant, one must be able to build an SGD-apparatus which would supply with electricity a light bulb.

4) According to Samokhin, Gulkin will need a 10 V source for the take-off of a large spaceship. When one has a converter with a closed energetic circle, i.e., a perpetuum mobile, one does not need an external energy source. The energy delivered by the 10 V battery will be delivered by the energy excess of the self-generating discharge.

5) According to Samokhin, Prof. Chernetski is on the road for explaining the extrasensory perception by the "longitudinal" electromagnetic waves. Before trying to explain the extrasensory perception, whose existence until the present day is not experimentally confirmed WITH SURETY, one has to show experimentally that longitudinal electromagnetic waves DO EXIST. There is NO such demonstration.

6) Samokhin writes: "It is generally recognized among physicists that all elementary-particle interactions occur with the help of virtual-particle exchange." Virtual particles are introduced by the physicists who are unable to build a theory with real particles. The introduction of the "virtual" particles is only a sign of our inability to understand certain observed phenomena.

7) According to Samokhin, Prof. Chernetski has asserted that "gravitation is as dim now as it was in the times of Newton". Newton explained ALL gravitational phenomena by introducing the form of the gravitational energy of two masses m, m', separated by a distance r,

$$U = \gamma mm'/r,$$

where γ is the gravitational constant which depends on the choice of the measuring inits for U, m and r. We still explain ALL gravitational phenomena by the help of THIS formula. What is in gravitation dim?

One wishes to know WHAT has Prof. Chernetski done, which are the scheme, the parameters and the observed results of his self-generating plasmatron. By reading the report of A. Samokhin, one learns nothing else besides "virtual particles", "virtual positrons accelerating plasma electrons", "ordered dipole vacuum", "polarization of the vacuum" and similar bla-bla-bla. (A friend of me once said: "Polarization of the vacuum is the same thing as colour of a fart.")

In the same issue of the PACE-newsletter there is an article on Chernetski's plasma generator by Paul Czysz who repeats the bla-bla-bla of Samokhin, followed by an article of the SCHIZOPHRENIC T. E. Bearden who asserts since many years that the Soviets have discovered the "longitudinal" electromagnetic waves, about which Tesla has spoken, and that in 1986 by the help of such "scalar" waves the Soviets have shot down Challenger. According to Bearden, with the announcement of Chernetski's converter, the Soviets intend to divert the attention of the West from their secret superweapons based on the scalar waves. I give the advice to my readers to not read the schizophrenic stupidities of Thomas Bearden.

As Chukanov said me, the converter of Chernetski is mounted in a basement of a living house in Moscow (this is confirmed also by Samokhin). I had long conversations with Dr. Vladimir Berzhaty of the cosmic flight organizat.on ENERGIA (GLAVKOSMOS), the leader of the Soviet group which organizes the common Austrian-Soviet cosmic flight next year. Dr. Berzhaty has helped some years ago the experimentation of Prof. Chernetski. According to Berzhaty NEVER an excess of energy has been reliably observed. Consequently the cosmic flight organization ENERGIA has lost any interest in Chernetski's experiments and now Chernetski continues his research quite alone and at a lack of money. Dr. Chukanov has visited Chernetski's basement laboratory in December 1989, however Chernetski has not set his plasma generator in action and no measurements have been done.

I think there is no sense in narrating fables on Chernetski's converter. And it is silly to propose "theories" about the "zero-point energy" which is transformed in the plasma converter from virtual into real, as do Moray King and my friend George Hathaway in the same issue of the PACE-newsletter of Michrowski. First we need a CLEARLY WRITTEN report on WHAT Prof. Chernetski has done with a clear scheme of his setup and with observational data taken by an independent observer. As said above, I intend to visit Prof. Chernetski in the near future and my report will be published in TWT-IX.

THE NUCLEAR CLUSTER CONCEPT AND THE ELEMENTARY ELECTROSTATIC SIZE OF NUCLEI

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Abstract. A new physical quantity called the elementary electrostatic size (EES) of nuclei, which may be interpreted as their elementary wave of de Bróglie, is proposed. The cluster concept for the structure of the nuclei is considered in the light of this parameter. The stability of the nuclei is explained with the character of the clusters composing it.

The universal fine structure constant

$$\alpha = e^2/4\pi\varepsilon_o hc = 1/137$$

takes part in many physical relations. Well known is its relation to the Compton wavelength of the electron

$$\lambda_e = h/m_e c = r_e/\alpha,$$

where m_e is the electron's mass and r_e is its so-called classical radius

$$r_e = e^2/4\pi\varepsilon_o m_e c^2 = 2.8\times10^{-15} \text{ m}.$$

The same relation for proton leads to

$$r_p = e^2/4\pi\varepsilon_o m_p c^2 = 1.54\times10^{-18} \text{ m},$$

where m_p is the mass of the proton, and by analogy r_p can be called classical radius of the proton. The relation

$$\alpha = r_e/\lambda_e = r_p/\lambda_p$$

is common for both electron and proton and, if presented as

$$r_e = \alpha\lambda_e, \qquad r_p = \alpha\lambda_p,$$

one sees that for both electron and proton there is a certain size, 137 times smaller than their Compton wavelength,

$$r = \alpha\lambda = e^2/4\pi\varepsilon_o mc^2 \tag{1}$$

which can be called Elementary Electrostatic Size (EES).

But since α is a dimensionless quantity, the EES may also be considered as a certain elementary wavelength of de Broglie

$$r = \Lambda = e^2/4\pi\varepsilon_o mc^2 = H/mc, \tag{2}$$

defined, however, by a new action quantum

$$H = \alpha h = e^2/4\pi\varepsilon_o c = 7.7\times10^{-37} \text{ J s},$$

which also gives the direct relation bewteen the elementary charge e and the velocity of light c (in the sense that there is no intervention of α, as is the case for $h = e^2/4\pi\varepsilon_o\alpha c$).

If we use the action quantum H and accept that the velocity of light c is the maximum possible velocity,

$$r_p = \Lambda_p \quad \text{and} \quad r_e = \Lambda_e$$

will represent, respectively, the minimum possible waves of de Broglie (defined with the action quantum H) related to the proton and electron, being at the same time their EES.

Relation (1) shows which must be the radius of a particle with charge e and mass m, so that its full energy mc^2 could be given only by the potential energy of electrostatic interaction of its charge, if supposing that it is spherically distributed.

It is evident that, for each nucleus N with charge Ze and mass M, one may introduce an EES

$$R_N = Z^2 e^2/4\pi\varepsilon_o Mc^2 \tag{3}$$

with the same physical character. Relation (3) may be presented as

$$R_N = Z^2 H/Mc = Z^2\Lambda_N, \tag{4}$$

where Λ_N will be the minimum wave of de Broglie of the nucleus N.

If, by neglecting the difference in the masses of neutron and proton, we accept that the mass of each nucleus may be considered equal to

$$M = Am_p, \tag{5}$$

where A is the mass number, expression (4) may be simplified

$$R_N = Z^2 H/Am_p c = Z^2\Lambda_p/A. \tag{6}$$

In other words, every nucleus may be characterized by its EES, defined by its charge, mass number and the proton EES. Its EES presents the radius which a sphere with its charge and mass must have, in order its full energy (Mc^2) to be given by the potential energy of the electrostatic interaction; it will be its minimum wave of de Broglie, directly connected with the mass and the charge of the nucleus.

We know that each nucleus consists of protons and neutrons; the difference between their masses is extremely small and this allowed us to neglect it in (5). We do not know, however, something certain about the charge distribution and the size of the neutron. That is why we are tempted to assume that relation (6) gives generally proportionality between the EES of the nucleus and the EES of the particles it consists of.

If we should assume that the protons and neutrons of the atomic nucleus build specific conglomerations called clusters, and if we should ascribe to any such cluster its EES, denoting it by R_c, we can write $R_N \approx kR_c$, where k is a certain coefficient of proportionality. We can then write

$$\Lambda_N \approx k\Lambda_c, \tag{7}$$

and thus the elementary wave of de Broglie of the nucleus will be proportional to the elementary waves of the clusters of which it consists.

We would like to call attention to the fact, that our concepts are quite near to the cluster hypothesis developed lately by not a few investigators.

Relation (7) points to the seldom noticed connection between the elementary wave of de Broglie (its EES) and the same waves of the clusters of which it consists. The proportionality between them allows   to consider (7) from another point of view: as an analogue of the connection which must exist between the width of the infinitely deep potential hole, L, and the wavelength, λ, of an elementary particle closed in it. The width of the potential hole must be proportional to the wavelength

$$2L = n\Lambda,$$

so that the particle would not be able to leave the hole.

We have reasons to liken the <u>double</u> value of the EES to the width of an infinitely deep potential hole, where the particles of which the nucleus consists are closed. It seems that, in order the nucleus to be stable, in the double width of the hole there must be a whole number of times of elementary waves of these particles

$$2\Lambda_N = n\Lambda_c \qquad \text{or} \qquad 2R_N = nR_c. \tag{8}$$

As an attempt to illustrate the above presented approach, we shall first consider Table 1 presenting the EES, defined after formula (3) of the most elementary existing nuclei: proton, p, deuteron, d , and triton, t , which are also among the fundamental

constructive units of the rest of nuclei.

In the second column, after the name of the nucleus, is given the number of the protons, marked with Z; the third column presents the mass number of the nucleus A, and the fourth column shows the composition of the nucleus (protons and neutron). Next follows the EES, defined after formula (4), and then its twofold value (both in thousandths of fm, 10^{-18} m), where we have worked with $m_p = 1.673 \times 10^{-27}$ kg and $m_n = 1.675 \times 10^{-27}$ kg for the mass of the proton and neutron, respectively. The seventh column presents the stability, expressed by the half-life period, in the cases of unstable nuclei. The last column shows which EES of the clusters, of which the nucleus eventually consists, are contained in the twofold EES of the investigated nucleus a whole number of times, what is the condition for stability. It is shown for the unstable ones that they cannot contain a whole number of times of EES of the stable nuclei, of which they may be composed. Thus 1p in the last column on the line of the deuteron shows that the twofold EES of d contains one EES of proton or, all the same, one its elementary wave of de Broglie. 0.67p and 1.34d on the line of the triton mean that in its twofold EES, the EES of the proton enters 0.67 times and that of the deuteron 1.34 times.

The proton stays rather aside - it is the basic constructive unit. Exactly one proton EES enters the twofold EES of the deuteron: it is stable. The triton, which can consists only of proton or deuteron, does not contain in its twofold EES a whole number of times of EES either of proton or of deuteron - and it is unstable, indeed.

In the next table (Table 2) we have picked out the EES of those relatively not quite complicated stable nuclei which confirm the above stated assumption.

At first sight Table 2 shows that the requirement, the width of the potential hole to be proportional to a whole number of times of elementary wavelengths of de Bröglie of the stable clusters eventually composing the nucleus, is valid for a great number of nuclei. The first two (^{4}He and ^{6}Li) are of a sufficiently simple structure and, therefore, one can easily estimate of which clusters they may be composed, since here few or even no variations in the choice of the components are possible (if we assume that these clusters may be the most simple stable nuclei). For ^{12}C and ^{14}N, there is a new aspect: it is equally possible ^{12}C, which consists of 6p and 6n, to be constructed either

of 3 ^{4}He or of 2 ^{6}Li. The ^{14}N nucleus may be built of 7d, but other combinations are also possible:

$$^{14}N = 7d = d + 3\ ^4He = \ ^6Li + 2\ ^4He = \ ^6Li + \ ^8Be.$$

But:

$$2\ ^{14}N = 14d = 7\ ^4He = 4.64\ ^6Li = 3.5\ ^8Be.$$

This means that it is not probable ^{6}Li and ^{8}Be to be composing clusters of ^{14}N.

We think that the combinations

$$^{16}C = \ ^6Li + \ ^{10}B = \ ^{12}C + \ ^4He$$

also are not probable.

There is also an option for ^{20}Ne: its twofold EES is proportional to a whole number of times of EES of d and ^{4}He, but not of ^{8}Be and ^{10}B. This may be considered as an indication that it is most likely ^{20}Ne to be composed of deuterons and ^{4}He nuclei, but not of two ^{10}N nuclei or of a combination of ^{4}He and ^{8}Be, in spite that this seems possible

The width of the potential hole of ^{24}Mg is proportional to a whole number of times of elementary wavelengths of d, ^{4}He, ^{6}Li and ^{12}C. Of the combinations, shown in Table 2 all except the last one, where ^{10}B nuclei take part, are probable.

These examples raise at least two new questions.

What must be understood when we say that the width of the potential hole has to be equal to a whole number of times of the minimum wavelengths of de Broglie of the composing particles (relation 8)?

How must be resolved the problem on the probability a nucleus to be built of one or another cluster unit?

We shall try to give an answer based on elementary logical considerations.

We think that the width of the potential hole may be slightly larger - with a few percents, no more - than a whole number of times of elementary waves of de Broglie of the eventual structural units. If it is eventually smaller, then the difference has to be considerably smaller than parts of the percent.

Concerning the second question, according to our opinion, when relation (8) is more precisely fulfilled for certain clusters, then it is more probable that it is composed of them.

It would be fine, but quite strange, if everything was so simple. Among the incredibly large number of isotopes, exceptions appear at once. Table 3 shows the EES of some nuclei that make an exception: they should be stable from the point of what was said just now but, unfortunately, they are not.

Table 3 may be still supplemented with quite many examples from the beginning of the periodical system of isotopes. It is important that, for many of the unstable nuclei, the width of the potential hole may again contain a whole number of times of elementary waves of de Broglie, but of unstable clusters; this might be a cause for their instability.

It seems that some nuclei are unstable since, because of the unsuitable width of the potential hole, they cannot hold certain stable components for a long time. Other are unstable because they consist of unstable components. It is possible that this characterizes simply the relativity of the notion stability. The time scales in the nuclei and in our life differ greatly.

Impressing is the way, how the idea of stability, as an idea of the availability of a potential hole containing a whole number of times of elementary waves of de Broglie of the clusters composing the nucleus, turns almost at once into an idea of the necessity the elementary waves of de Broglie of the clusters composing the nucleus - stable or not - to enter a whole number of times in the width of the potential hole. This will then determine the stability or instability of the composed nucleus. Then it is logical to assume that the nucleus most probably will be composed of those clusters whose elementary waves of de Broglie satisfy relation (8).

However there are nuclei which decisively disobey relation (8).

In Table 4 we have chosen some of them, confining to relatively not very complex nuclei and remaining thus at the beginning of the periodical system of isotopes.

The first is the ^{3}He nucleus. In the width of its potential hole there is not a whole number of times of the elementary waves either of proton or of deuteron, which might be its only cluster units. It should be unstable but in fact it is one of the most stable nuclei. This fact impels us to assume that here perhaps we come across something like a new constructive unit. As it seems, the neutron realizes an exceptionally stable bond

between the two protons in ^{3}He and, further on, in some heavier nuclei, considerably separated deuterons.

We are tempted to consider this as an indication that the neutron is generally able to realize a very strong internuclear bond, which we would like to call neutron bond.

Let us take ^{11}B. In its twofold EES there is a whole number of times of elementary waves of deuteron. It seems that here the deutrons are already separated as cluster units, and the neutron realizes the bond between the deuterons.

All said up to now sounds unusual and the lack of experimental evidence is an additional obstacle for its acceptance. The cluster hypothesis, to which ours is very close unfortunately cannot help much in this respect. However, we would like to draw at once attention to the work of Katö et al.[1] They investigate the cluster structure of ^{24}Mg and propose the following

$$^{24}Mg = {}^{20}Ne + {}^4He,$$
$$^{24}Mg = {}^{16}O + {}^8Be,$$

at excitation energy $10 < E_{ex} < 20$ MeV, and

$$^{24}Mg = {}^{12}C + {}^{12}C$$

at $E_{ex} > 20$ MeV.

These authors think that in the excitation region 18 - 19 MeV there must be linear ^{4}He - ^{16}O -^{4}He clusters.

We think that this is in very good agreement with the results of our hypothesis.

Data from the cluster microscope investigation of ^{16}O confirm very well our concepts. These data are presented by Tohzaki - Suzuki[2] who thinks that the ^{16}O nucleus may be considered as crystal-like consisting of ^{4}He-clusters. Tozaka[3] who investigat ^{20}Ne also finds that it consists of ^{4}He-clusters.

It is quite natural to consider the results of this study of a relatively small par of the great variety of isotopes as preliminary. When thoroughly new suggestions are made, it is natural that their deep significance and importance can be not revealed at first.

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2. Akihiro Tohzaki-Susuki. Progr. Theor. Phys., **81**, 370 (1989.

3. Yoshikaru Tozaka, Yasuyuku Suzuki, and Kiyomi Tkeda. Progr. Theor. Phys., **81**, 379 (1989).

TABLE 1

R (in 10^{-18} m) for proton (p), deuteron (d) and triton (t)

Sign	Z	A	Composition	R	2R	Stability (in years)	Eventual clusters
p	1	1	p	1.538	3.176	-	-
d	1	2	p + n	0.769	1.538	-	1p
t	1	3	p + 2n	0.510	1.020	12.26	0.67p, 1.34d

TABLE 2

R_N (in 10^{-18} m) for some stable nuclei

Sign	Z	A	Composition	R_N	$2R_N$	Eventual clusters
^{4}He	2	4	$2p + 2n = 2d$	1.534	3.068	1p
^{6}Li	3	6	$3p + 3n = 3d = d + ^4$He	2.301	4.602	3.00 ^{4}He; 6.00 d
^{10}B	5	10	$5p+5n = 5d = d + 2\,^4$He $= ^3$He $+ ^7$Li	3.836	7.671	10d; 5 ^{4}He; 3.75 ^{3}He
^{12}C	6	12	$6p + 6n = 3\,^4$He $= 2\,^6$Li	4.603	9.206	12d; 6 ^{4}He; 4 ^{6}Li
^{14}N	7	14	$7p + 7n = 7d = d + 3\,^4$He $= 2\,^4$He $+ ^6$Li $= ^6$Li $+ ^8$Be	5.370	10.740	14d; 7 ^{4}He; 4.67 ^{6}Li; 3.50 ^{8}Be
^{16}O	8	16	$8p + 8n = 8d = 4\,^4$He $= 2\,^8$Be $= ^6$Li $+ ^{10}$B	6.137	12.274	16d; 8 ^{4}He; 3.999 ^{8}Be; 3.2 ^{10}B; 5.33 ^{6}Li; 2.66 ^{12}C
^{20}Ne	10	20	$10p + 10n = 10d = 5\,^4$He $= ^4$He $+ 2\,^8$Be	7.671	15.342	20d; 10 ^{4}He; 3.999 ^{10}B; 4.999 ^{8}Be
^{22}Ne	10	22	$10p + 12n = 2\,^{11}$B	6.974	13.947	9.09 ^{4}He; 3.99994 ^{11}B
^{24}Mg	12	24	$12p + 12n = 12d = 6\,^4$He $= 2\,^4$He $+ 2\,^8$Be	9.206	18.411	12 ^{4}He; 5.999 ^{8}Be; 4.79 ^{10}B; 8 ^{6}Li

TABLE 3

R_N (in 10^{-18} m) for some unstable nuclei obeying relation (8)

Sign	Z	A	Composition	R_N	$2R_N$	Stability (in sec)	Eventual clusters
^{6}He	2	6	$2p + 4n = 2t$	1.02	2.04	0.8	4t
^{9}Li	3	9	$3p + 6n = 3t$	1.53	3.06	0.168	6t
^{12}Be	4	12	$4p + 8n = 4t = 2\,^6$He	2.04	4.08	0.0114	7.98t; 4 ^{6}He
^{16}C	6	16	$2p + 10n = 2d + 4t = 2\,^8$Li	3.452	6.904	0.76	9d; 13.51t; 4.01 ^{8}Li
^{20}O	8	20	$8p + 12n = 4\,^5$He $= 2\,^{10}$Be	4.909	9.818	14	8.04 ^{5}He; 4 ^{10}Be

TABLE 4

R_N (in 10^{-18} m) for some stable nuclei not obeying relation (8)

Sign	Z	A	Composition	R_N	$2R_N$	Eventual clusters
^{3}He	2	3	$2p + n = d + n$	2.046	4.092	2.66p; 5.32d
^{7}Li	3	7	$3p + 4n = 3d + n$	1.972	3.944	2.57 ^{4}He; 5.17d
^{9}Be	4	9	$4p + 5n = 4d + n = 2\,^4$He $+ n$	2.726	5.453	7.11d; 3.56 ^{4}He
^{11}B	5	11	$5p + 6n = 5d + n = {}^4$He $+ {}^7$Li	3.487	6.974	9.09d; 4.55 ^{4}He; 3.55 ^{7}Li
^{13}C	6	13	$6p + 7n = 6d + n = $ ^{5}He $+ 2\,^3$He $= {}^5$He $+ {}^8$Be	4.247	8.494	17.07d; 4.15 ^{3}He; 6.93 ^{5}He; 2.77 ^{8}Be
^{15}N	7	15	$7p + 8n = 7d + n = {}^6$Li $+ {}^9$Be	5.012	10.024	13.06d; 4.36 ^{6}Li; 3.67 ^{9}Be; 3.53 ^{4}HE
^{17}O	8	17	$8p + 9n = 8d + n = 4\,^4$He $+ n$	5.776	11.552	15.06d; 7.53 ^{4}He

CORRESPONDENCE

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Mon in feldgasse 16

A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

27 February 1990

Prof. Wolfgang Rindler
Physics Department
The University of Texas of Da
Richardson
TX 75083 - 0688

Dear Prof. Rindler,

I send you copies of my Letter to the Editor of the AM. J. PHYS., submitted on the 20 December 1989 under the title "Relativity and Magnetism", of Prof. Romer's letter to me of the 9 January 1990 and of my letter to Prof. Romer of the 26 February 1990.

I shall be very glad if you will suggest to Prof. Romer that he publishes my Letter to the Editor.

For your information I send you also a copy of my "Childishly simple experiment violating the principle of relativity" which I submitted to the AM. J. PHYS. in the case that my Letter to the Editor will be published.

If you should be interested to receive some of my books, I shall gladly send them to you.

Hoping to receive your answer soon (in the case that you will suggest the publication of my Letter to the Editor, as well as in the case that you will not suggest its publication), I remain at your disposal for any questions which you would like to pose me.

Sincerely yours,

J. Marinov

Stefan Marinov

Editorial note. The above letter remained unanswered.

The three above mentioned documents are published, respectively, on pp. 221, 317 and 323 of TWT-VII.

Dr.Stefan Marinov

Morellenfeldgasse 16

A-8010 Graz

Austria

VAŠE ZNAČKA   VÁŠ DOPIS Z   NAŠE ZNAČKA   PRAHA 2.3.1990

VĔC:

Dear Dr.Marinov,

      I have to inform you that your paper No.2952 entitled "Action of constant electric current on electrons at rest due to the absolute velocity of the earth" was not recommended for publication in Czechoslovak Journal of Physics. We therefore send back the manuscript and enclose the referee's report.

                Yours sincerely,

                        Dr.J.Kratochvíl

Encls. : as stated above

Ref.No. : 198/90-2952   Deputy Editor-in-chief

Editorial note. The above mentioned paper is published on p. 110 of TWT-IV. Marinov answers this letter with his letter of the 8 March 1990.

In the paper of

Stefan Marinov

"Action of Constant Electric Current on Electrons at Rest due to the Absolute Velocity of the Earth"

the author uses a "new" electrodynamics in moving systems of reference to explain experiments of Sansbury, Edwards, Kennard and Marinov concerning the above problem.

Against this theory one can advance

1) The author gives no convincing arguments for changing the usual electrodynamics.

2) The new theory is not gauge invariant.

The main equation (2)

$$\vec{E} = -\text{grad}\phi - \frac{\partial \vec{A}}{\partial A} + \vec{v} \times \text{rot } \vec{A} + \epsilon\mu_0 (\vec{v}\vec{v}) \text{ grad}\phi$$
$$+ \vec{V} \times \text{rot}\vec{A} + (\vec{V}\text{grad})\vec{A}$$

describes "the force acting on a unit positive electric charge", ..."where ϕ and $\vec{A}$ are the laboratory electric and magnetic potentials ...".

Obviously equ. (2) and equ. (6) are not gauge invariant. But this problem is neither discussed nor another invariance group is mentioned.

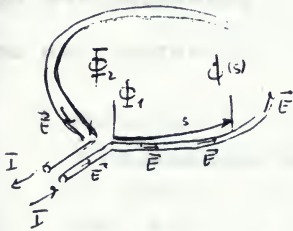For concrete calculations (p. 5) $\vec{A}$ is used in the usual form following by Lorentz gauge.

(But $\vec{E}$ in equ. (2) is *also* not invariant under the remaining restricted gauge group given by $\Box \chi = 0$, rsp. $\Delta \chi = 0$!)

3) In the case 1) on p. 2 a loop at rest with a constant current is considered.

By $\vec{J} = \sigma \vec{E}$, by the surface conditions

$$E_{tang.}^{(int.)} = E_{tang.}^{(ext.)}$$

and by

$$\vec{E} = -\text{grad}\phi ,$$

one has on the surface of a homogeneous wire

$$\phi(s) = \phi_1 - \frac{\phi_1 - \phi_2}{\ell} s$$

(ℓ is the length of the loop).

This established a Dirichlet problem for the electric potential ϕ in the exterior of the wire.

So one will receive a small ϕ of higher multipole structure.

For the investigation of the force on a charge it will be necessary to discuss the amount of this electric contribution also. The author's demand $\phi = 0$ is only a first approximation.

The experimental difficulties are described on p. 6.
Only the right ascension of the earth velocity apex could be
measured. The questions arising are:
Why only one measuring was performed (22. 1. 89)?
Was is[*]impossible to continue the measurements over a longer
time? How large are the error limits of the reported
experiment?

From a theoretical point of view a lot of questions are open.
The referee is thoroughly convinced that the author can not
overcome the difficulties of his theory.
In the present form the paper can not be published.
It should be rejected.

[*]it

INDIAN JOURNAL OF PHYSICS
INDIAN ASSOCIATION FOR THE CULTIVATION OF SCIENCE
JADAVPUR, CALCUTTA-700 032, INDIA

From : Dr. (Mrs.) K. K. Datta
Scientific Editor & Assistant Secretary,
Indian Journal of Physics.

Ref. No. 5B/90N.

Date 7-3-90

To : Dr. ~~Doctor of~~ Stefan Marinov,
Morellenfeldgasse 16,
A-8010 Graz - Austria.

Sub : On the absolute - - - - - -
- - - - - - - - - - - interactions.

[✓] The above paper has been found unsuitable for publication by the referee.
The paper (with original diagrams) is being returned with referee's comments.

The author belongs to the class of people who question the
well established Einstein's theory of relativity. He goes a step further
to question Maxwell's theory of electromagnetism. All of
them have one common characteristic that they lack in
scientific rigour which is also indicated by their work not
being published in the research journals, so much so that
the authors have to publish their work all by themselves (in
this case, the title 'Thorny way of Truth' is perhaps the
author's own publication).

I do admire the spirit of questioning the established facts
and theories but there should be good scientific reasons for
this. Einstein did question the Newtonian concepts. We
should always keep the room open for such people but the
present author does not belong to this class.

The work in question is based on the author's theory which
is unacceptable and hence it must be rejected outright.

Editorial note. Marinov answers this letter with his own of the 20 June 1990.

Sꓘ.   ⵯꓥOV
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    Tel. 0316/377093

8 March 1990

Dr. J. Kratochvil
CZECH. J. PHYSICS
Na Slovance 2
CSR-180 40 Praha 8

Dear Dr. Kratochvil,

Thank you very much for your letter of the 2 March 1990, although the rejection of my paper

ACTION OF CONSTANT ELECTRIC CURRENT ON ELECTRONS AT REST
DUE TO THE ABSOLUTE VELOCITY OF THE EARTH

was, of course, not pleasant for me.

I show in my answer to the referee's comments, which is enclosed, that the objections of the referee are WRONG. Thus, I beg you to give my paper to an arbitrator who has to decide whether I or the referee is right.

In his letter to me of the 31 January 1990, whose acceptance I now acknowledge, Dr. J. Fischer wrote the following: "I nevertheless maintain the general conviction that a controversial subject should be discussed in a journal specialized in the corresponding field." - I show since 20 years that the relativity theory and conventional electromagnetism are wrong. I submit papers to ALL physical journals of the world. The "specialized" journals, as well as all leading relativists in the world know since many years that I am right and that relativity is dead (Dr. Langer could persuade himself about the situation on the GR-conferences in the last years where we met). However they close the doors of their journals for my papers, because they know that by publishing my papers they will lose their prestige. Thus I can publish my papers only in journals which are not in the hands of the "relativistic lobby". I hope that the CZECH. J. PHYS. is one such journal. If Dr. Langer[*] has rejected during the last 20 years my papers, this was NOT because he takes part of the "relativistic mafia" but because he has not understood that I am right (this is the case with many of the space time specialists in the world who are not "consecrated").

As the referee has raised the question about the gauge invariance, I now submit also my paper

THE ELECTROMAGNETIC EFFECTS ARE DETERMINED BY THE
POTENTIALS AND NOT BY THE INTENSITIES,

and I RESUBMIT my paper

ACTION OF CONSTANT ELECTRIC CURRENT ON ELECTRONS AT REST
DUE TO THE ABSOLUTE VELOCITY OF THE EARTH.

The last paper was examined in your office ONE YEAR. Meanwhile the referee's opinion has been written in no more than 10 minutes. I hope that now, after so many years of stagnation, people in Czechoslovakia have understood that without expeditivity, fairness and GLASNOST human society cannot progress. Hoping this time to receive your decision in due time,

[*]Dr. Langer is NOT my present referee.

Sincerely yours, Stefan Marino

AUTHOR'S ANSWER TO THE REFEREE'S COMMENTS ON THE PAPER
"ACTION OF CONSTANT ELECTRIC CURRENT ON ELECTRONS AT REST DUE
TO THE ABSOLUTE VELOCITY OF THE EARTH" by Stefan Marinov

1) The first objection which the referee raises against my paper is the follwoing:
"The author gives no convincing arguments for changing the usual electrodynamics".

The results of the <u>rotational</u> Kennard experiment (1917) and of the <u>inertial</u> Kennard experiment carried out by me and reported in the present paper present ENOUGH experimental evidence for discarding the theory of relativity and conventional electromagnetism and for accepting my absolute space-time theory.

In the rotational Kennard experiment (fig. 2) there is a double circular current loop and a piece of wire put along the radius between both circular loops. The inertial Kennard experiment (fig. 1) is a "topological" transformation of the inertial one where there is a <u>prolongated</u> rectangular loop with a piece of wire between its bases. I give the effects observed by Kennard and by me, and the predictions of Einstein and of me. I leave place, so that the referee gives also HIS predictions. "Yes" signifies appearance of electric tension along the wire, "no" signifies that electric tension does not appear.
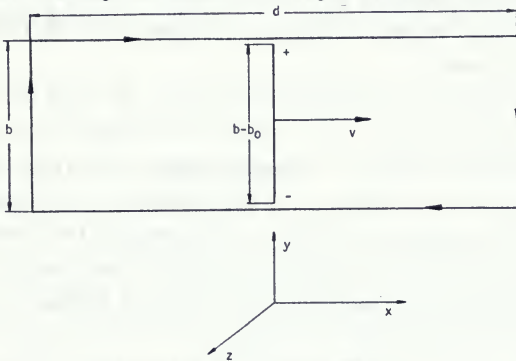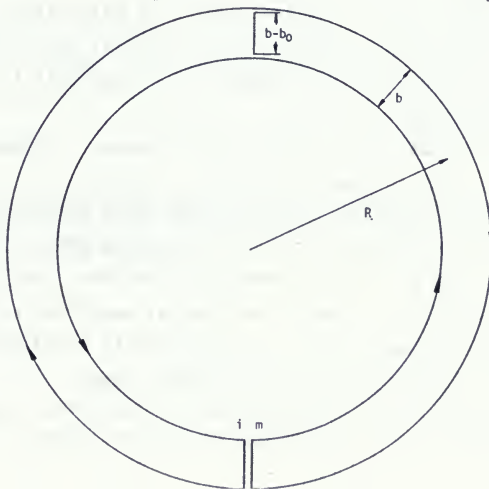

Fig. 1

Rotational Kennard experiment (fig. 2)	Observed effect	Einstein's prediction	Marinov's prediction	Refer predi
a) loop at rest, wire rotating	yes	yes	yes	
b) wire at rest, loop rotating	no	yes	no	
c) wire and loop rotating together	yes	no	yes	
Inertial Kennard experiment (fig. 1)				
a) loop at rest, wire moving	yes	yes	yes	
b) wire at rest, loop moving	no	yes	no	
c) wire and loop moving together	yes	no	yes	

In the rotational Kennard experiment the velocity of rotation is with respect to ab-
solute space. In the inertial Kennard experiment the rectiliner velocity of propulsive
movement is with respect to absolute space.

If the referee will not give his predictions (and I am SURE that he will have not
the courage to do this), I think that the referee must be disqualified and my paper is
to be given to another referee. If also the second referee will not dare to give his
predictions, the paper is to be given to a third referee. If also he will be unable
to answer the questions of the "Kennard's sphynx", my paper is TO BE PUBLISHED. Other-
wise science can not make progress.

2) The second objection which the referee raises against my paper is the following:
"The new theory is not gauge invariant".

Yes, my theory is NOT gauge invariant. I showed that gauge invariance can be experimen
tally violated. Thus, according to my theory, all calculations in electromagnetism are
to be done by the help of the potentials and NOT by the help of the intensities. I show
to the errors of the "gauge invariant" conventional theory in my paper "The electromag-
netic effects are determined by the potentials and not by the intensities" which is en-
closed and which now I submit to the CZECH. J. PHYS.

The Aharonov-Bohm effect also shows that only when calculating by the help of the po
tentials, the right answers can be obtained. Conventional physicists think that this is
true only in quantum electromagnetism, meanwhile I showed that this is true also in
non-quantum electromagnetism.

3) The third objection which the referee raises agianst my paper is the following:
If in a closed loop, at rest in absolute space, a constant current flows, then an elec-
tric potential, different from zero, exists in the space outside the loop. This asser-
tion is true and such an effect was observed by Sansbury (REV. SC. INSTR. **56**, 415, 1985
However the explanation (given by me elsewhere) is not this one given by the referee.
This effect is proportional to I^2 and is the same at any orientation of the rectangular
loop, while the effect observed by me is proportional to I and depends on the orienta-
tion of the loop if the laboratory moves in absolute space.

At the end of his comments the referee poses the following "experimental" questions:
a) Why one measurement only was performed (in the 22.I.89)? - I have performed mea-

surements also on other days, but I give the observation for the 22 January 1989, as on the different days (because of the difference between solar and sideral times) the hours of the day when observing maximum effects are different.

b) How large are the error limits of the reported experiment? - The error limits can be not precisely enough settled. I observed maximum opening of the leaves (when the axis of the apparatus pointed "north-south") for about 1.5 hours. Thus, the reported right ascensions are to be taken with an incertitude of about $\pm 45^m$.

Finally the referee writes that from a theoretical point of view a lot of questions are open. The referee can find the answers to ALL his questions by reading my books. My CLASSICAL PHYSICS is an encyclopaedic book and in my series THE THORNY WAY OF TRUTH all experiments which contradict conventional electromagnetism, the theory of relativity and the laws of conservation of energy and angular momentum, known at the present time, are analysed.

At the end of my comments I should like to point to the recent AMAZING fact. In the December-1989 issue of the AMERICAN JOURNAL OF PHYSICS the well-known relativist Prof. Wolfgang Rindler asserts that for the case of a magnet moving with velocity **v** and generating at a reference point the magnetic potential **A**, the induced electric intensity is not

$$E = - v \times rotA, \tag{1}$$

as conventional electromagnetism assert, but

$$E = (v.grad)A, \tag{2}$$

as <u>I assert</u>.

The question now is FOR WHICH of the above two formulas will the referee vote. If he will vote for formula (1) he will enter into contradiction with Prof. Rindler and the results of the rotational and inertial Kennard experiments. If he will vote for formula (2), he destroys all arguments raised by him against my submitted paper.

Thus if the referee will not dare to vote for one of the formulas (1) or (2), my paper is to be given to a second, then to a third referee, and if none of them will dare to vote for one of these formulas, my paper is to be published. If at indecisive answers of THREE referees my paper will be rejected, one can say only one: "Jdete do prdele!"

<u>Marinov's note</u>. The above letter and comments remained unanswered, cim mohli bychom prispet k vysledku, ze redaktor a recenzent sami se rozhodli zahajit odhod do prdele.

FYZIKÁLNY ÚSTAV

Centra elektro-fyzikálneho výskumu Slovenskej akadémie vied

Dúbravská cesta 9, 842 28 Bratislava

> Dr.S.Marinov
> Inst. for Fundamental Physics
> Morellenfeldgasse 16
> A–8010 G r a z
> Austria

Vaša značka   Naša značka   Bratislava 12.3.1990.
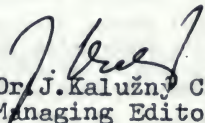
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Dear Dr.Marinov,

Enclosed please find the referee report on your paper "Physical Essence of the Maxwell–Lorentz Equations". The referee did not recommend to publish the paper in our journal. Thus Editorial Board of the journal Acta Physica Slovaca decided that the mentioned paper wuold not be published in our journal.

I am sending you back the manuscript.

Thank you very much for sending the paper to our office.

With best regards

Yours sincerely

RNDr. J.Kalužný CSc.,
Managing Editor

Editorial note. Marinov answers this letter with his own of the 16 March 1990. The above mentioned paper is published in TVT-I, p. 323.

ACTA PHYSICA SLOVACA

Reference report on the paper by S.Marinov: Physical essence
of the Maxwell-Lorentz equation (51/89).

In the paper, author tries to elaborate a non-conventional theory of electromagnetic phenomena, based on instantaneous interaction between charged particles. Unfortunately, his approach suffers from number of serious shortcomings (sóme of them are mentioned below), which make this theory physically inacceptable.

First of all it concerns the conservation laws of energy, momentum and angular momentum, which were firmly confirmed by thousands of experiments and any theory, dealing with electromagnetism must take them into account. In conventional theory, the set of Maxwell equations together with the Lorentz force equation describes closed system consisting of moving charged particles and corresponding electromagnetic fields. In such system, the energy, momentum and angular momentum can be transferred from particles to fields and vice-versa, however, the total sum of energy, momentum and angular momentum remaines unchanged, except for ammounts, that are carried away from the system by a radiation part of the fields, in accordance with the corresponding conservation laws. Because electromagnetic fields can carry energy, momentum and angular momentum, they have an existence, totally indepedent of charges and currents.

In the paper, because of instantaneous character of interaction between charged particles, the concept of field is not used. One could accept this, however, one cannot accept, that the special theory of relativity is ignored as well. Therefore it is not surprising, that in such theory the violation of conservation laws may occur.

Author asserts, that in his theory, radiated energy propagate with the velocity of light. It is not clear, by means of what the radiated energy is carried, if there is no field in this theory.

The derivation of the Lorentz force equation from the law of energy conservation is not correct, because magnetic field does not contribute to the change of kinetic energy of the moving charge.

In conventional theory, the Lorentz force equation and the Maxwell equations are mutually independent and cannot be derived one from another. This is due to fact, that while Maxwell equations describe the electromagnetic field itself and its generation by the moving charges, the Lorentz equation describes force exerted on the charge from the side of the field. Therefore it is very dubious to attempt to derive Maxwell equations from the Lorentz force equations as it is done in the paper.

The above mentioned defects are very serious and cannot be removed by minor changes of the theory, which, in consequence, cannot be taken seriously as describing the electromagnetic phenomena. Therefore I do not recommend this paper for publication.

SILENT MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

14 March 1990

Dr. Petr Beckmann
GALILEAN ELECTRODYNAMICS
Box 251
Boulder
CO 80306

Dear Dr. Beckmann,

Thank you very much for No. 1 of your journal which you kindly sent me and which was received in Graz a couple of days ago.

I read this first issue with great interest. The journal seems to become very interesting.

One of the most important informations was DISSIDENT NEWS about Silvertooth's experiment. I am very glad that you will inspect Silvertooth's experiment. Then it will be fine if you will present your impressions in your journal. If Silvertooth, indeed, has succeeded in measuring the Earth's absolute velocity by an optical arrangement where there is no rotating axle and no Newtonian time synchronization is realized, his experimental success will be of a very high value. Silvertooth's experiment in my quasi-Michelson variation is pretty easy for repetition and if his quasi-Wiener experiment works, then my quasi-Michelson experiment, REPEATED IN THE SAME LINES AS IN THE LAST SILVERTOOTH'S PUBLICATION, can be carried out in numerous optical laboratories. Thus we must do our best to spread the information on Silvertooth's experiment.

I however established that Silvertooth's experiment, where one looks for a spacing displacement between the nodes, DOES NOT WORK. In the enclosed note I show that, according to me, also the experiment where one looks for a phase difference in reaching maximum illumination at the different nodes also CANNOT WORK, at the present state of experimental technique, if using light waves. I think, however, that if using radio waves (where the period is not so short as for light), effects can be observed. Thus the discussion on Silvertooth experiment must be SPREAD OVER THE WORLD.

I beg you to elaborate your decision on the rejection/acceptance of my contribution AS SOON AS POSSIBLE. In the case of rejection I should like to submit my paper to another journal. If you will accept the paper, then I should beg you to compose it in Boulder (I shall gladly pay for the work).

Hoping that with the time you will begin with the publication of my other papers, and looking forward for your prompt answer,

Sincerely yours,

S. Marinov

Stefan Marinov

PS. I have the text of a note which John Maddox will publish on me and on the rotational and inertial Kennard's experiments on the 22 March (but one can never believe to the promises of Dr. Maddox, may be it will appear a week or two or three later). The important fact is that after more than a year the promised by him CHRISTMAS PUZZLE is written and composed by him. Thus on the pages of NATURE my figures from NEW SCIENTIST, **112**, 48, (1986) will appear with the comments of Dr. Maddox. As very prudent, Dr. Maddox does not give his predictions on the issues of the Kennard and quasi-Kennard experiments. However, I think that the only fact that this PUZZLE appears under the signature of Dr. Maddox will bring in a couple of months whole relativity to collapse.

I have heard that you have invited Fr. Müller from Miami to submit papers to your journal. Müller is one of the most brilliant physicists on this Earth!

PS. I hope that you have received my last letter from January (or February).

Editorial note. This letter is answered by Dr. Beckmann with a letter of the 25.III.90.

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     TOV
  Ne.    .   . 15        16 March 1990
A-8010 GRAZ — AUSTRIA
   Tel. 0316/377093
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Dr. J. Kaluzny
ACTA PHYSICA SLOVACA
Dubravska cesta 9
CSR-842 28 Bratislava

Dear Dr. Kaluzny,

Thank you very much for your letter of the 12 March 1990, although the rejection of my paper

PHYSICAL ESSENCE OF THE MAXWELL-LORENTZ EQUATIONS

was not, of course, pleasant for me.

I found a very well written referee's report (what is not the case with the other physical journals as PHYS. REV., J. PHYS., IL NUOVO CIMENTO, etc.), however, unfortunately, the concepts of the referee (and of conventional physics) are WRONG. I give apart my objections to the referee's comments. In the light of these comments, I allow me to submit again my paper and to ask that you give the whole correspondence to an arbitrator who has to decide publication or rejection. In the case of publication, I should invite the referee to appear with a criticism on my concepts and experiments in the press.

Almost all referees who criticize my papers do not know the whole extent of my theoretical and experimental research and think that I do some "student" errors. To make more clear to you and to your referees the extent of my reasearch in the last 30 years, I submit to ACTA PHYSICA SLOVACA also the following THREE papers:

1. VIOLATIONS OF THE LAWS OF CONSERVATION OF ANGULAR MOMENTUM AND ENERGY.

2. REPETITION OF WHITEHEAD'S EXPERIMENT FOR DEMONSTRATING THAT DISPLACEMENT CURRENT IS A PURE MATHEMATICAL FICTION.

3. MAXWELL'S ILLUSION: THE DISPLACEMENT CURRENT.

Of course, you can publish only one or two of these papers, but I should like that the referee and the arbitrator have all three papers under the hand.

I should like to note that I am an "old friend" of Czechoslovakia where I spent some of my most happy years. I send you two newspaper clippings on my demonstration on Venceslao square in 1978 and two Czech poems from my collection of poems LIST OTBRULEN, edited in Washington, D.C. in 1978. I am in contact with the CZECH. J. PHYSICS since 20 years and I visited twice its editorial office and my referee Dr. Josef Langer in Prague (in 1978 and in 1979). On the other hand Bratislava is so near to Austria that one can say, we are in the same boat (your letter of the 12 March was received by me on the 14 March).

I have little hopes that you will publish a paper of me. But the recent events in Czechoslovakia have shown that every one of us, on the place on which one is, must take ALONE decisions thinking with his own head. Otherwise society falls in stagnation. The only authority in life (and in science) is the EXPERIMENT.

Hoping to receive your acknowledgement for the reception of this letter and then in due time also your final decision on the acceptance/rejection of my papers,

Sincerely yours,

J. Marinov

Stefan Marinov

Editorial note. This letter is answered by Dr. Kaluzny with a letter of the 6 June 1990

AUTHOR'S ANSWER TO THE REFEREE'S COMMENTS ON THE PAPER

"PHYSICAL ESSENCE OF THE MAXWELL-LORENTZ EQUATIONS" by S. MARINOV

I express my cordial thanks to the referee for his critical comments, as one sees that he has attentively read my paper and has perfectly well understood its essence.

Unfortunately, I cannot agree with the referee's comments as they are WRONG, i.e., the referee's concepts (as well as the concepts of conventional electromagnetism) ARE NOT ADEQUATE TO PHYSICAL REALITY.

I shall give my remarks to all critical comments of the referee.

1) The referee is perfectly right by asserting that "the conservation laws of energy, momentum and angular momentum were firmly confirmed by thousands of experiments". I shall even add, not by thousands but by millions of experiments, as, for example, every electromagnetic motor or generator gives confirmation of these laws. However, now, at the end of the XXth century, certain electromagnetic machines have been constructed which violate these laws. The most important of them is the machine TESTATIKA which is an electrostatic perpetuum mobile producing (in one of its variations) 3 kW FREE ENERGY, i.e., energy from nothing. The Vth volume of my sequence THE THORNY WAY OF TRUTH is dedicated to the machine TESTATIKA where there are many photographs of the different models. I have also a 30-minutes video on it. The machine TESTATIKA is constructed in the religious commune METHERNITHA in the village Linden, 30 km southeast of Bern. I am a member of the commune, where there are no money, hierarchy, exploitation and the ruling slogan is "every for all, all for every one". --- Then come the experiment of Graham and Lahoz (NATURE, **285**, 154, 1980), which violates the angular momentum conservation law, and my Bul-Cub machine without stator (see the enclosed paper "Violations of the laws of conservation of angular momentum and energy") and my Rotating Ampere Bridge with Displacement Current (RABDC) (TWT-IV, p. 126), which also violate the angular momentum conservation law. I have observed violations of the energy conservation law in my machines ADAM and MAMIN COLIU (see the enclosed paper "Violations of the laws..."), however the observed effects there are small. Maybe, I am in error and my experiments do not violate the laws of conservation. I explain their effects only as VIOLATION of these laws. If somebody would be able to explain the effects in these experiments in the frame of the laws of conservation, HE MUST DO THIS. Moreover, the violation of the angular momentum conservation law in my machines Bul-Cub without stator and RABDC can be explained BY EVERY CHILD on the basis of the Lorentz force equation (which I call the Newton-Lorentz equation). The professors all over the world evade to recognize that the Newton-Lorentz equation violates Newton's third law (as I showed in my books EPPUR SI MUOVE and CLASSICAL PHYSICS, only the full Newton forces, $m\mathbf{u} + (q/c)d\mathbf{A}/dt$, are equal and oppositely directed according to the Newton-Lorentz equation, but the simple kinetic forces, $m\mathbf{u}$, where $\mathbf{u}$ is the acceleration of a particle with mass m which is at a space point where the magnetic potential of the surrounding system is $\mathbf{A}$). But this violation was established by Grassmann in 1845, and when considering the interaction of two current elements according to the Newton-Lorentz equation, one arrives at the formula of Grassmann. As for the interaction of closed current loops Newton's third law is preserved, conventional physics (following Maxwell) has saved this law by asserting that ALL circuits are closed, as if there is a condenser in the circuit, the so-called displacement current flows between its plates. According to Maxwell, this displacement current has exactly the same magnetic character as conduction and convection currents, i.e., it must generate its own magnetic intensity field, acting thus with potential magnetic forces on other currents, and react with kinetic forces to the potential magnetic action of other currents. However, this assertion of the Maxwellians IS NOT TRUE! THE EXPERIMENT SHOWS EXACTLY THE OPPOSITE! Thus the circuits with condensers are OPEN circuits and for such circuits the Newton-Lorentz equation leads to violation of Newton's third law. As said above, I have carried out experiments demonstrating violation of the angular momentum conservation law and the effects observed coincided with those calculated by the help of the Newton-Lorentz equation.

2) The referee writes:

> In conventional theory, the set of Maxwell equations together with the Lorentz force equation describes closed system consisting of moving charged particles and corresponding electromagnetic fields. In such system, the energy, momentum and angular momentum can be transferred from particles to fields and vice-versa, however the total sum of energy, momentum and angular momentum remains unchanged, except for amounts, that are carried away from the system by a radiation part of the fields, in accordance with the corresponding concervation laws. Because electromagnetic fields can carry energy, momentum and angular momentum, they have an existence, totally independent of charges and currents.

The truth, however, is the following: The èlectromagnetic fields are of three kinds: potential, radiation and radiation reaction "fields". The potential fields "appear" MOMENTARILY in whole space, they have no energy and momentum density. The potential fields depend on the charges and their velocities and are inversely proportional to the second power of the distance from the generating particles. The radiation fields PROPAGATE with a velocity c from the generating particles. The radiation fields depend on the charges and their accelerations and are inversely proportional to the first power of the distance from the generating particles. The radiation reaction fields depend on the charges and their super-accelerations and are inversely proportional to the zero power of the distance from the generating particles, i.e., they act on the radiating particles. If the referee knows of some experiment where somebody has established experimentally the existence of energy and momentum in potential electromagnetic fields, I beg him to cite this experiment. Unfortunately HE CAN'T. However the referee can immediately carry out an experiment to see that potential electromagnetic fields have no momentum density. He has to take a permanent magnet generating the magnetic intensity $\mathbf{B}$ and a charged condenser generating the electric intensity $\mathbf{E}$. If he will put a wall in perpendicular to the vector $\mathbf{\Pi} = (c/4\pi)\mathbf{E} \times \mathbf{B}$ he will NOT register a pressure equal to this vector. If, however, $\mathbf{E}$ and $\mathbf{B}$ will be the electric and magnetic intensity in a RADIATION FIELD, he will measure the above pressure (such experiments were done first by Lebedev in 1905).

3) The referee writes:

> In the paper, because of instantaneous character of interaction between charged particles, the concept of field is not used. One could accept this, however, one cannot accept, that the special theory of relativity is ignored as well.

I have shown by numerous experiments that the theory of relativity is WRONG: my rotating axle experiments with whose help I have measured a couple of times the absolute velocity of the Earth in a closed laboratory by light signals (one such experiment was published in CZECH. J. PHYS. **B24**, 965, 1974), my inertial Kennard experiment with whose help I measured the same velocity by electromagnetic effects (the paper is now in consideration in CZECH. J. PHYS.), my inverse Rowland experiment with whose help I showed that if a charged body moves, there is a torque on a magnetic needle at rest, but if a magnetic needle moves and a charged body is at rest, there is NO torque (TWT-VII, p. 331), etc. Thus one can take SERIOUSLY a physical theory not when it takes into account the theory of relativity but only when it clearly and DEFINITELY rejects it. Any physical theory based on the WRONG principles of relativity and equivalence must be rejected AUTOMATICALLY as wrong, as these principles are NOT TRUE.

4) The referee writes:

> The author asserts that, in his theory, radiated energy propagates with the velocity of light. It is not clear, by means of what is radiated energy carried away if there is no field in this theory.

Radiated energy are PARTICLES called photons. Whether the photons are of light or of radio waves, the difference between them is only one: the difference in their wavelengths. There are NO other differences. I should not like to discuss here the particles-waves dualism, as this problem is TOO LONG.

5) The referee writes:

The derivation of the Lorentz force equation from the law of energy conservation is not correct, because magnetic field does not contribute to the change of kinetic energy of the moving charge.

In the enclosed paper "Violation of the laws..." I give MATHEMATICALLY CORRECT deduction of the Newton-Lorentz equation when proceeding from the definition equalities of electric and magnetic energies (the laws of Coulomb and Neumann) and the energy conservation law. I beg the referee to show WHERE my deduction is not correct. If he will be unable to find some mathematical error (and he surely will not find), he has to accept that the Newton-Lorentz equation can be deduced on the basis of the energy conservation law (by the way, ALL equations in contemporary physics are deduced on the basis OF THIS AXIOMATICAL LAW!). The referee is right that the magnetic intensity B does not contribute to a change in the kinetic energy of a test particle, but the magnetic energy W DOES.

6) The referee writes:

In conventional theory, the Lorentz force equation and the Maxwell equations are mutually independent and cannot by derived one from another. This is due to the fact that, while Maxwell equations describe the electromagnetic field itself and its generation by the moving charges, the Lorentz equation describes force exerted on the charge from the side of the field. Therefore it is very dubious to attempt to derive Maxwell equations from the Lorentz force equation as it is done in the paper.

The truth, however is the following: The fundamental equation in electromagnetism (the equation of motion) is the Newton-Lorentz equation. The fundamental "field" quantities in electromagnetism are the potentials Φ, A, and not the intensities E, B. The intensities are MATHEMATICAL PRODUCTS of the potentials being defined as follows

$$E = - \text{grad}\Phi - (1/c)\partial A/\partial t, \qquad\qquad B = \text{rot}A. \qquad (1)$$

If the QUANTITIES E and B are defined in this way as functions of the PHYSICAL QUANTITIES Φ and A, they must be BY FORCE connected by the mathematical relations called Maxwell equations. The referee can see how one can obtain the Maxwell equations from the above definitions equalities on pp. 44 and 45 of the enclosed paper "Maxwell's illusion: the displacement current". In the rejected paper I showed that besides the definition equalities (1), one has to take also the MATHEMATICAL RELATIONS following from the DEFINITION EQUALITIES for the potentials Φ, A and for the charge and current densities ϱ and J. For stationary systems this relations are

$$\Delta\Phi = - 4\pi Q, \qquad\qquad \Delta A = - (4\pi/c)J \qquad (2)$$

and for dynamic systems they are

$$\square\Phi = - 4\pi Q, \qquad\qquad \square A = - (4\pi/c)J. \qquad (3)$$

For quasi-stationary systems the Maxwell equations have the form

$$\text{rot}E_{tr} = - (1/c)\partial B/\partial t, \qquad\qquad \text{div}B = 0 \qquad (4)$$

$$\text{rot}B = \partial E/c\partial t + (4\pi/c)J \qquad\qquad \text{div}E = 4\pi Q \qquad (5)$$

and E_{tr} in equ. (4) is DIFFERENT from $E = E_{coul}$ in equ. (5). Only for radiation fields these two electric intensities coincide.

STEFAN MARINOV
Morellenfeldgasse 16   21 March 1990
A-8010 GRAZ -- AUSTRIA
Tel. 0316/377093

Dr. John Maddox
NATURE
· 4 Little Essex Street
London WC2R 3LF

Dear Dr. Maddox,

I send you TWT-VII, which you ordered yesterday, begging you to send me the price
IN CASH in a letter (Ł 20), as from a cheque I lose one third part. I enclose also the
covers of TWT-IV, V, VI. If you would like to order also some of these books, send Ł 20
for any of them.

As I said you, your fax with your article was received so badly that I could not de-
cypher it and I have not included it inTWT-VII. But yourpaper in NATURE which will appear
on the 29 March (as you promised me yesterday on the phone) will be printed in TWT-VIII
Now I shall not criticize your paper, as I am sure that you will change the text. Thus
I shall comment then your PRINTED paper. I hope that on the 29 March also my LETTER TO
THE EDITOR will appear (as you promised me yesterday on the phone).

I wish now to put an order in our relations and to no more sacrifice your and my time
for inconclusive phone conversations. Thus I beg you to answer WRITTENLY the following
SIX questions:

1) When will appear my correspondence with Tiomno? Please, determine the date of pub
lication.

2) Will you publish my COMPOSED paper EXPERIMENTAL VIOLATIONS OF THE PRINCIPLES OF
RELATIVITY, EQUIVALENCE, AND CONSERVATION OF ENERGY AND ANGULAR MOMENTUM? This paper ha
to appear on the 18 August 1988, then on the 13 October 1988 (see your letter of the
29 July 1988 on p. 330 of TWT-III). Then its publication was postponed for undetermined
time. Now, after the publication of your puzzle, when the whole world will see that re-
lativity is DEAD, you have either to determine the date of publication or to REJECT the
paper. I CANNOT MORE AWAIT. If you will reject publication, I beg you to present some
motivations. For this paper I phoned you at least 300 times and came FIVE TIMES TO LON-
DON. The last trip was paid by you but the other four trips were paid from the pocket
of a poor groom.

3) During my visit in London in December 1988, you said that a friend of you will
reconstruct my Rotating Ampere Bridge with Sliding Contacts (RABSC) described in my pa-
per PROPULSIVE AND ROTATING AMPERE BRIDGES VIOLATE THE PRINCIPLE OF RELATIVITY. If it
will rotate, as I assert, you will publish both the reports of your friend and of me.
"Your paper will be printed first" said you, looking above your spectacles. I suggested
that you give my paper for examination also to Prof. Graneau who is an expert in the
field and who has spoken with you on the phone on this very day. In about a month later
you said me on the phone that your friend has reconstructed the RABSC and it DOES NOT
rotate. I begged you to send me the photograph and the report of your friend "so that I
can publish them in TWT and show to the world that England which once was a leading
technical nation has totally degenerated and there one is unable to construct even a chi
dishly simple gadget" (the text in quotation marks is taken from my PREPAID advertise-
ment THE PERPETUUM MOBILE IS DISCOVERED - TWT-VI, p. 305 - whose publication was REJEC-
TED BY YOU). You promised to send me the photograph and the report but they NEVER rea-
ched me. Then you said me on the phone that Prof. Graneau has sent a critical report on
my experiment from which it is not clear whether he suggests publication or rejection.
I met Prof. Graneau at a conference in Perugia in September 1989 (you can see a common
picture on p. 214 of TWT-VII). He said me that he NEVER has received a letter from you
with my paper. Thus I beg you to answer: "Will you publish this paper or you will rejec
it?" If you further insist that according to you the RABSC cannot rotate, I am ready
immediately to bring my machine, under the condition that if it will rotate, you will
pay me the trip (Ł 500).

4) Will you publish my paper THE MYTHS IN PHYSICS or you will reject it?

5) Will you publish my paper ACTION OF CONSTANT ELECTRIC CURRENT ON ELECTRONS AT RES
DUE TO THE ABSOLUTE VELOCITY OF THE EARTH or you will reject it?

6) Will you publish my paper CHILDISHLY SIMPLE EXPERIMENT VIOLATING THE PRINCIPLE
OF RELATIVITY or you will reject it?

Sincerely yours,   *S. Marinov*

Stefan Marinov

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

21 March 1990

Prof. Bengt Nagel
Nobel Committe for Physics
Sturegatan 14
S-114 36 Stockholm

Dear Prof. Nagel,

I send you the VIIth volume of my series

THE THORNY WAY OF TRUTH.

I beg you very much to acknowledge the reception of this book.

As you may see also from this volume, I try with ALL physical journals of the world but there is NO possibility to print my papers in which crucial experiments violating the principles of relativity and equivalence and the laws of energy and angular momentum conservation are reported. I address you once more with the question: Would the Nobel committee suggest to PHYSICA SCRIPTA to examine some of my papers? Otherwise the scientific community will further remain without information on my experiments, as, unfortunately, the circulation of my books is very restricted.

There is a FUNCTIONING PERPETUUM MOBILE in the community Methernitha. The first prototype was constructed 10 years ago. I have examined the machine a year ago. I did ALL what is in my possibilities to bring the information on this machine to the world and I can't. Is our world not an IDIOTIC world? Answer, please, this question. The responsibility of the Nobel committee of not having helped me to inform the world that the energetic crisis is solved is BIG.

Hoping to receive not only an acknowledgement for reception of my book but also some answer to my questions,

Sincerely yours,

Ɔ. Marinov

Stefan Marinov

PS. I have a 30-minutes video on the perpetuum mobile TESTATIKA. If I should be invited by the Nobel committee, I shall gladly come to Stockholm (FOR MY ACCOUNT) to present the film to members of the committee.

Editorial note. The above letter is answered by a letter of the Nobel committee of the 2 April 1990.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

23 March 1990

The Director
IL NUOVO CIMENTO
via L. degli Andalo 2
I-40124 Bologna

Dear Sir,

I submit to IL NUOVO CIMENTO B the following two papers

1. REPETITION OF WHITEHEAD'S EXPERIMENT FOR DEMONSTRATING
   THAT DISPLACEMENT CURRENT IS A PURE MATHEMATICAL FICTION.

2. MAXWELL'S ILLUSION: THE DISPLACEMENT CURRENT!

The PACS numbers are 03.50, 41.10.

All charges will be paid by myself.

Here I transfer the copyright for these papers to IL NUOVO CIMENTO.

I wish to inform you that on the 11 July 1988 I submitted my paper (Nr. 9718 NCBR)

   VERY EASY DEMONSTRATION OF THE VIOLATION OF THE ANGULAR MOMENTUM
   CONSERVATION LAW AND OF THE FAILURE OF CONVENTIONAL ELECTROMAGNETISM.

This paper received a referee opinion in which the referee did not raise objection against my experiment but only about the interpretation of the results (on the 9.I.89)

I presented my objections on the 14.I.89. Every author who has constructed an original and PUZZLING experiment has the right for his own interpretation of the results. I affirm that my BUL-CUB MACHINE WITHOUT STATOR, described in that paper VIOLATES the law of angular momentum conservation. If the referee pretends that it does not violate this FUNDAMENTAL PHYSICAL LAW, he has to present his objections IN THE PRESS. However more than a year I do not receive the decision of the vice-director, Prof. Remo Ruffini, although having asked many times with letters addressed directly to Prof Ruffini.

I beg you very much to investigate this case and to inform me: will my paper be publiched or it will be rejected. In the case of rejection I should like that motivations should be presented.

The effects in the BUL-CUB MACHINE WITHOUT STATOR and the effects observed and discussed in the papers submitted now are tightly connected. I have established (my conclusions are based also on some historical experiments) that THERE IS NO DISPLACEMENT CURRENT (neither in vacuum nor in dielectrics). I have demonstrated this by clear and EASILY REPEATABLE experiments. I wish to see my papers published. If somebody will raise objections against the interpretation of the results of my experiments I beg him to appear in the press but not to STOP the information on these CRUCIAL EXPERIMENTS.

Hoping to receive your acknowledgement for the reception of these two papers and then your decision on all THREE papers submitted by me,

Sincerely yours,

Ω. Marinov

Stefan Marinov

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

23 March 1990

Dr. Hermann Feshbach
ANNALS OF PHYSICS
Deptm. of Physics, Room 6-318A
M I T
Cambridge
MA 02139

Dear Dr. Feshbach,

Thank you very much for your letter of the 26 December 1989, although the rejection of my paper "Violations of the laws of conservation of angular momentum and energy" was, of course, not pleasant for me. In your letter of rejection you wrote:

"We find no evidence that your paper is incorrect, but must reject it on account of limited space."

In my paper I gave a pretty detailed account on my BUL-CUB MACHINE WITHOUT STATOR which VIOLATES the angular momentum conservation law. If you have the opinion that this paper is CORRECT, it must be published BEFORE ALL OTHER PAPERS, as I report on a VIOLATION OF A LAW OF CONSERVATION. Thus I beg you, when rejecting my papers to be more attentive in the "rejection formulations". Of course, if you do not wish to maintain contacts with me, you can write this clearly, as many of the leading physical journals of the world have done   (PHYSICAL REVIEW, IL NUOVO CIMENTO, EUROPHYSICS LETTERS, ACTA PHYSICA POLONICA, AMERICAN JOURNAL OF PHYSICS). I prefer to receive such a letter for "braking the diplomatic relations", than   "diplomatically written" rejections, as this will spare your and my time. But if you would like to receive papers from me, then make BETTER rejection formulations.

Now I submit to your journal my paper (in one copy)

THE MYTHS IN PHYSICS.

I beg you that you read NATURE of the 29 March[*] where Dr. Maddox, after twenty years of contacts, five visits to London and about 1000 phone conversations, finally will point HIMSELF at one of the present myths in physics.

Hoping to receive your acknowledgement for the reception of my paper and then in due time also of your final decision,

Sincerely yours,

Ŋ. Marinov

Stefan Marinov

PS. Enclosed are the covers of the seven volumes of my series THE THORNY WAY OF TRUTH. You can receive any of these volumes by sending me § 25 (+ § 10 for air mail).

[*]If, of course, Dr. Maddox will not again deceive me and postpone the publication. In any way, if you are interested to read the paper of Dr. Maddox, I can send you the text, which Dr. Maddox faxed to me on the 13th March.

<u>Editorial note</u>. The answer to the above letter is given with the letter of Dr. Feshbach of the 17 April 1990.

GALILEAN ELECTRODYNAMICS
Box 251
Boulder, CO 80306
tel. 303-444-0841

25 March 1990

Dear Dr Marinov:

Many thanks for your letter of 14 March, received yesterday. (You received only no. 1 of GE? You should have received no. 2 also.)

I cannot take your contribution, because as a letter commenting on my "Dissident News" it is far too long, and as an article it is too chatty. As you will see I am extremely short of space and can bring only articles that have condensed "meat" in it: brief arguments supported by mathematics with a minimum of prose, and no personal experiences or judgements at all. The composition of such a paper for people who have no access to word processors is no problem, I will do it for them, though it is a chore.

Yes, I did get your reply to my preceding letter. I cannot give you the names of the two persons who made the allegations. In both cases they were letters somebody else and sent to me by third parties. I do not feel that I have the right to make their names public.

I have no objection to printing papers by you if they qualify under what I have said above. However, for the reasons stated previously, I cannot take the risk of publishing anything using the results of your experiments.

With best wishes,

Yours sincerely,

Petr Beckmann
Editor

P.S. I checked your calculation that the node of a standing wave does not change its distance from the mirror with the direction of the ether wind, and I find the same result by a slightly different method.

PHYSICS LETTERS A

26/3/90   (26 March 1990)

V1280a / V1281a

Dear Dr Marinov,

I enclose a report on your ms V1280a. It has not proved possible to elicit a report on V1281a. I have looked through both papers and I cannot find in either of them material deserving rapid publication. I regret therefore that I cannot accept your papers for publication.

Yours sincerely,

JPV

TRANSCRIPTION.

Dear Dr. Marinov,
I enclose a report on your ms V1280a. It has not proved possible to elicit a report on V1281a. I have looked through both papers and cannot find in either of them material deserving rapid publication. I regret therefore that I cannot accept your papers for publication.
                                          Yours sincerely, JPV

Editorial note. Marinov answers this letter with his own of the 9 April 1990.

Comments on V 1280 a:

CALCULATION OF THE PUSHING FORCE WHICH ACTS ON THE AMPERE BRIDGE

by Stefan Marinov

I do not understand this paper. There seems to be a lot of infor mation missing. If the author provides this, I will be glad to revie a more complete paper. My questions are:

1. Since references 1 and 2 are not readily available, how di Marinov derive equ.(1)?

2. The shape and area of the cross-section of the conductor ar known to have a marked effect on the measured force on the Amper bridge. Why does Marinov expect his calculations for an infinitel thin conductor agree with experiment?

3. Why does the author talk about "pushing forces" when the Grass mann force actually "pulls" the Ampere bridge from the front. H would be justified to speak about pushing forces had he used th Ampere formula.

4. What does the author mean by "edge force"? His infinitely thi conductor is all edge!

5. To make it easier to oversee the various experimental results they should be collected together in a table. Marinov ignored th measurements by Roper, Cleveland, Graneau, and Peoglos. They shoul also be included in the table.

6. Magnet designers have found the same difficulties with integratio singularities which Marinov mentions. For this reason they develope the "stick model". The author should compare his method of calcula tion with the stick model.

7. From magnet calculations I know that the shape of the "shoulder has a significant effect on the calculated Grassmann force. Measure ments apparently have not revealed this shape dependence. Comment please.

8. The author's diagrams do not show the important parameters o 'a' and 'a0'.

Editorial note. The above paper is published in TWT-VII, p. 165.

From : Dr. (Mrs.) K. K. Datta
Scientific Editor & Assistant Secretary.
Indian Journal of Physics.

Ref. No. 54 B/89

Date 25.3.90

To : Stefan Marinov,
Institute for Fundamental Physics,
Morellenfeldgasse 16, A 8010 Graz, Austria

Sub : Violations of the laws of - - - - -
- - - - - - - - - - - - - energy.

[✓] The above paper has been found unsuitable for publication by the referee.
The paper (with original diagrams) is being returned with referee's comments.

Yours sincerely,

K. K. Datta

Referee report on the paper, 'Violations of the laws... and energy', by Dr. S. Marinov: (published in TWT-III, 33)

In the first part of his paper, Dr. Marinov describes a new derivation of the equations of electromagnetism from three axioms. There is nothing new or enlightening in this derivation. His claim of the discovery of a new kind of induced electric intensity is just a misunderstanding.

The rest of the paper deals with non conservation of angular momentum and energy in electromagnetism. These are well tested principles and any gross violation, as he claims to have observed, necessarily implies that most of the physics as we know is wrong. Dr. Marinov has not brought out his points and arguements, experimental or theoretical, convincingly in this paper. Also, the general style and language are such that the ideas are obscure. Therefore, I suggest that this paper may not be published in the Indian Journal of Physics.

Editorial note. See Marinov's answer in his letter of the 20 June 1990.

NOBEL COMMITTEES FOR PHYSICS AND CHEMISTRY
THE ROYAL SWEDISH ACADEMY OF SCIENCES
Box 50005, S-104 05 Stockholm, Sweden

April 2, 1990

Dr Stefan Marinov
Morellenfeldgasse 16, A-8010 Graz

Dear Sir,

This is to acknowledge receipt of your book "The Thorny Way of Truth", part VII.
I am sorry to say that the Nobel Committee cannot suggest to you where
your papers should be submitted for publication.

Yours sincerely,

Anders Bárány, Secr. to the Physics Committee

Editorial note. With this letter the Nobel committee answers Marinov's letter of
the 21 March 1990.

PHYSICS LETTERS A

.OFESSOR V.M. AGRANOVICH

titute of Spectroscopy
;SR Academy of Sciences
oitsk
oscow 142092
;SR

2 апреля 1990 г.

Дорогой Стефан Маринов,

В ответ на Ваше письмо от 10.12.89 г. могу лишь сообщить, что рецензенты категорически отказываются что-либо писать по поводу Ваших статей. Я думаю, что уже исчерпал свои возможности помочь Вам. В этой ситуации прошу Вас более не присылать мне Ваши статьи. Попробуйте поискать рецензентов за океаном.

Желаю успехов.

Профессор *А2раиович* В.М.Агранович

TRANSLATION

Dear Stefan Marinov,

Answering your letter of the 10 December 1989, I can inform you that the referees categorically reject to write anything on your papers. I think, I have exhausted my possibilities to help you. At this situation I beg you to not send me more your papers. Try to find referees over the ocean.

Wishing you successes,

Prof. V. M. Agranovich

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

7 April 1990

Dr. Petr Beckmann
GALILEAN ELECTRODYNAMICS
Box 251
Boulder
CO 80306

Dear Dr. Beckmann,

After returning from Switzerland (where I saw the big machine TESTATIKA which will produce 30 kW FREE ENERGY) I found your letter of the 25 March in my post.

Yes, shortly after having written you my letter of the 14 March, I received No. 2 of GAL. ELECTROD. Thank you very much. I find your journal VERY INTERESTING. Just a journal which the world needs.

As you have not accepted my paper ONCE MORE ON SILVERTOOTH'S EXPERIMENT, I shall submit it to SPEC. SC. TECHN., where Silvertooth's paper and my criticism on it have been published. I still am looking forward to read your comments on Silvertooth's experiment after you should see it.

I think that my papers:

THE ELECTRODYNAMIC EFFECTS ARE DETERMINED BY THE POTENTIALS AND NOT BY THE INTENSITIES

and

A COMMENT ON W. A. SCOTT MURRAY'S ARTICLE,

submitted to your journal on the 12 October 1989, match the requirements which you settle for my papers in your last letter. Thus, if you will accept them for publication, write me as soon as possible. If they will not take the hurdle, I shall submit some other paper (I have dozens and dozens of papers which await to be published).

Next year with Prof. Bartocci and Dr. Monti (who organized the conferences in Bologna and Perugia in 1988 with Silvertooth and Aspden) we organize a big conference GALILEO BACK IN ITALY III (the first and the second were the mentioned conferences in Bologna and Perugia). If you would like to publish information on that conference in your journal (or if you will be only interested in it), I shall gladly send you relevant information. There is money and we intend to make a representative conference and to put finally an end to the nonsensical relativity. Constructors of machines which violate the laws of conservation will be invited at the conference.

Hoping to receive your answer soon,

Sincerely yours,
S. Marinov
Stefan Marinov

PS. I am in the organizing committee of the Russian conference on which Prof. B. Wallace reported in No. 2 of your journal (the next Russian conference will meet on 1991). Russian scientists will be invited at the GALILEO BACK IN ITALY III. The first Russian scientist who was invited by me at an anti-relativity conference was Dr. S. Pankratov, the editor of NAUKA U ZHIZN', at our conference in Munich in 1988.

Editor's note. This letter is answered by Dr. Beckmann with his own of the 6 July 1990.

STEFAN MARINOV
Morellenf lg ne 15
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

9 April 1990

Prof. J. P. Vigier
PHYSICS LETTERS A
11 Rue Pierre et Marie Curie
F-75231 Paris Cedex 05

Dear Prof. Vigier,

Thank you very much for your letter of the 26 March 1990 which I found yesterday in my post returning from Switzerland where I saw the big TESTATIKA machine with 30 kW output of FREE ENERGY (for more information see my book TWT-V).

The rejection of my papers V 1280 a and V 1281 a was, of course, not pleasant for me.

I show with the enclosed comments that the referee's criticism on my paper V 1280 a is UNTENABLE. Thus I beg you either to give this paper (together with the referee's criticism and my comments) to an arbitrator or to send my paper for publication. If you will decide nevertheless to reject my paper DEFINITELY, I beg you to inform me about your decision AS SOON AS POSSIBLE, as I should like then to submit my paper to another journal.

Concerning my second paper V 1281 a you write in your letter:

It has not proved possible to elicit a report on V 1281 a. I have looked through the paper and I cannot find material deserving rapid publication.

In my paper I give the report on an experiment where according to conventional physics there MUST BE an effect. Meanwhile I established EXPERIMENTALLY that THERE IS NO EFFECT. And you assert that the report on SUCH an experiment does not deserve rapid publication. I AM WONDERING! To show to you how important is my experiment, I pose to you the following question:

Will, ACCORDING TO YOU, the ring in fig. 4 of my paper rotate when filled with dielectric or not?

If you will not answer this question by "yes" or by "no", I remain with the opinion that you have NOT read my paper. Thus I shall accept the rejection of my paper, only if you will answer the above question by "yes" or by "no" (or by "I do not know").

I beg you VERY MUCH to acknowledge the reception of this letter and to inform me whether you DEFINITELY reject my papers or you will send them to referee and to arbitrator. I repeat, a definite rejection of the second paper must be accompanied by your "yes-no" answer to my question.

I beg you to understand that the scientific community will accept my experiments where I demonstrate violations of Newton's third law and of the angular momentum conservation law only when they will understand that the displacement current is NO current. My paper V 1281 a is dedicated to an experiment which can be repeated by CHILDREN and which patently shows that the displacement current is NO current.

Please, acknowledge the reception of this letter AS SOON AS POSSIBLE.

Sincerely yours,

S. Marinov

Stefan Marinov

Editorial note. About the answer of Prof. Vigier to this letter, see Marinov's letter to Prof. Vigier of the 7 May 1990.
Paper V 1280a is the paper "Calculation of the pushing force..." published in TWT-VII, p.165.   Paper V 1281a is the paper "Repetition of Whitehead's experiment..." published in TWT-VII, p. 26.
Prof. Vigier answers the above etter with his letter of the 2.V.90.

OBJECTIONS OF STEFAN MARINOV TO THE REFEREE'S COMMENTS ON
MARINOV'S PAPER "CALCULATION OF THE PUSHING FORCE WHICH ACTS ON THE
AMPERE BRIDGE"

I find all referee's objections UNTENABLE. Here are the proofs of my assertion.

1. The referee writes that he cannot understand how formula (1) is derived, as reference 1 is not available. The derivation of formula (1) is a problem which EVERY STUDENT can solve in three minutes. Indeed, let us have a Π-form Ampere bridge with legs of lengths L and shoulder of length a. Let us take a plane reference frame xOy with origin at the left corner of the bridge, with abscissa along the shoulder and with ordinate directed oppositely to the left leg. At a point on the shoulder, distant x from the origin, a current I in the left leg (which I assume flowing towards the shoulder) will generate, according to the definition of the magnetic potential, the following magnetic potential

$$A = \int_0^L (\mu_0 I dr/4\pi)\hat{y}, \tag{A}$$

where r is the distance between the current element dr along the leg to the point on the shoulder distant x from the frame's origin. As $dr = dy$, $r = (x^2 + y^2)^{1/2}$, we obtain

$$A = (\mu_0 I/4\pi)\int_0^L (x^2 + y^2)^{-1/2} dy\,\hat{y} = (\mu_0 I/4\pi)\text{Arsinh}(L/x)\,\hat{y}. \tag{B}$$

Grassmann's formula for the force with which a current element I'dr' acts on a current element Idr, when the distance between them is r (pointing from dr' to dr), is the following one

$$df = (\mu_0 II'/4\pi r^3)dr\times(dr'\times r) = Idr\times rot(\mu_0 I'dr'/4\pi) = Idr\times rot(dA). \tag{C}$$

Grassmann's formula can be found in any textbook published before WWI and in one of 77 textbooks published after WWII (according to my statistics). Any student can in one minute deduce Grassmann's formula from the Lorentz equation which can be found in any textbook on electromagnetism published before, between and after the world wars.

For the force with which a wire with length L acts on the current element Idr we have to substitute dA in (C) by A taken from (B), writing $Idr = I dx\hat{x}$. After integration for $a_0 \leq x \leq a$, where a_0 is a very small quantity, as for $a_0 = 0$ there is a singularity, we obtain for the net force acting on the whole bridge, taking into account that there are two legs,

$$f = 2\int_{a_0}^a Idx\hat{x}\times rotA = (\mu_0 I^2/2\pi)\int_{a_0}^a L(x^2 + L^2)^{-1/2}(dx/x)\hat{y} \cong (\mu_0 I^2/2\pi)\int_{a_0}^a (dx/x)\hat{y} = (\mu_0 I^2/2\pi)\ln(\frac{1}{a}$$

where, as it can be easily calculated, for a bridge with a/L < 0.33 the approximation leads to an error not bigger than 5%.

This is the formula (1) from my paper about which the referee is wondering where from it may come.

But if the referee is puzzled by formula (1), I ask him: How he does calculate the for acting on a Π-form Ampere bridge? - The answer of the referee surely will be: "I do not know how." Thus the first question of the referee reveals the fact that he NEVER has calculated and NEVER has seen the analytical expression of the force acting on a Π-form Ampere bridge. Indeed, formula (1) can be found in NO textbook on electromagnetism all over the world, as it CLEARLY SHOWS that the Ampere bridge moves by the action of interna forces and it thus PATENTLY VIOLATES Newton's third law. I am asking the referee: "Is, according to him, the Ampere bridge violating Newton's third law?", but I know with suret that the referee will be AFRAID to give an answer to this question.

2. The referee knows (I presume) that the electric potential of a sphere charged homogeneously (in its volume or on its surface) with electric charges does not depend on the radius of the sphere (at any point of space outside the sphere). I showed in my paper that the force acting on a topsyturvy U-form Ampere bridge does not depend on the radius of curvature. It is thus CLEAR that the magnetic pushing force acting on a U-form Ampere bridge will NOT depend on the thickness of the wire and it is CLEAR that the force on ALL U-form Ampere bridges will be the same as that one calculated for an infinitely thin wire.

3. In our world there are NOT "pulling" forces. When one goes through a door on which it is written "pull", one PUSHES the door by one's hand in a direction OPPOSITE to one's movement. But the force acting on the Ampere bridge is NEITHER a pushing force, as the Ampere bridge is a SELFPROPULSING object set in motion by INTERNAL FORCES, in a drastic violation of Newton's third law and of all concepts of humanity about the forces according to which always a body A acts on a body B. The Ampere bridge is a body A which ACTS ON ITSELF.

4. The edge forces are the forces acting on the wire elements with lengths a_0 at both edges (corners) on the shoulder of the Π-form bridge. For a Π-form bridge of an infinitely thin wire the forces acting on these small current elements are INFINITELY BIG. The referee can read how Pappas, J. APPL. PHYS. **59**, 19 (1985) (Pappas is the FIRST person who has done QUANTITATIVE measurements of the forces acting on the Ampere bridge) evades in his CALCULATIONS and MEASUREMENTS the availability of these "edge forces".

5. In my paper I do NOT compare theoretically calculated forces acting on the Ampere bridge with experimentally measured forces, as I show that even for the MOST SIMPLE U-form bridge the calculation leads to an improper integral, and I was UNABLE to establish MATHEMATICALLY whether the integral is converting to a final number or to infinity. Thus it has no sense to cite the numbers of different authors obtained for different Π-form Ampere bridges. (LATER - see this volume, p.14) I found that the integral converges to infinity!)

I do not know the measurements of Roper but, surely, this Roper, if he really does exist, has not made some valuable measurements, as nobody of the scientists treating the "Ampere-bridge problems" in the last years makes reference to Roper. I shall be, however, infinitely thankful to the referee if he will supply me with reference to Roper's experiments. Cleveland has done only QUALITATIVE but not QUANTITATIVE measurements of the forces acting on the Ampere bridge. Graneau, NUOVO CIMENTO **D7**, 31 (1986) and Peoglos, . PHYS. **D 21**, 1055 (1988) obtain quasi the same number as Pappas

$$f = 10^{-6} \ N/A^2.$$

All these authors search for different tricks to resolve in their experiments the problem with the "edge forces".

In my experiments (S. Marinov, THE THORNY WAY OF TRUTH, East-West, Graz, 1988 and 1989, Part III, p. 74 and Part IV, p. 140) where the contact between the bridge and the wires which are solid to the laboratory is realized by BALL BEARINGS, I found

$$f = (5 \pm 2.5) \times 10^{-6} \ N/A^2.$$

However I was UNABLE to calculate the force even on the MOST SIMPLE tupsytorvy U-form bridge, as my mathematical abilities are LIMITED. Meanwhile a GOOD mathematician MUST BE ABLE to establish whether the first integral on the right side of formula (9) in my paper is convergent to infinity or to a certain final number. The referee of PHYSICS LETTERS surely must be a GOOD mathematician and I do not doubt that he will be able to calculate the integrals (9). I shall be infinitely thankful to him if he will do this. As the calculations will take some time, I shall gladly pay him $ 200 for the work. The money can be PREPAID to the Editor of PHYSICS LETTERS in the case that the referee will promise to do the job. (I repeat, LATER - see item 5 above - I found that the integral converges to infinity.)

6. The "stick model" has absolutely no reference to the calculation of the force acting on a U-form Ampere bridge. The "stick model" is applied when there are conductors with FINITE cross-sections. My conductors are with INFINITELY SMALL cross-sections. For more information, see A. Ludwig, IEEE Antennas and Propagation Society Newsletter, p. 40, February 1989, where there are many other references treating the above topics.

7. I showed mathematically that the pushing force on the U-form Ampere bridge does no depend on the radius of curvature of the half-circular "shoulder". From here one MAY con clude that the force will not depend on the shape of the bridge. However, I do not permi to me to make such a generalization, as I am unable to calculate even the force acting on the U-form bridge. With my paper I intend to turn the attention of the scientific com munity to this HIGHLY INTERESTING problem.

8. The parameters a and a_0 appear only for the Π-form bridge. The ONLY parameter of the U-form bridge is the radius of curvature of the "shoulder" (at infinitely long legs)

CONCLUSION. If an author has given answers to ALL questions of the referee and has shown the untenability of ALL critical remarks of the referee, the paper must be either given to an arbitrator or sent for publication. Otherwise science cannot progress.

I wonder that Prof. Vigier has sent my paper to an AMERICAN referee. The Americans ar IGNORENT AS CHILDREN concerning basic problems in physics. The Americans are scientists WITHOUT MEMORY, as one cannot find old European journals in the American libraries. They accept the dogmas which the European scientists have forged during centuries as Hegel's absolute truth and can progress only in the frame of these dogmas. Papers of basic re-search are to be sent only to EUROPEAN referees. Exactly the same is to be said about th Japanese scientists. However in applied physics areas Americans and Japanese are EXCEL-LENT.

J. P. WESLEY, *Ph.D. Physicist*

Weiherdammstr. 24, *7712 Blumberg, West Germany,* Tel: 07702-658

12 April 1990

Dear Stefan,

Enclosed is the first 1/3rd of the book I am writing with the pretentious title ADVANCED FUNDAMENTAL PHYSICS. Any help, suggestions, corrections, changes, alterations, etc. that you can send me would be most greatly appreciated. Please note especially Chapter 4. Here I lean heavily on your "hitch-hiker" model for light propagation in a moving medium. I feel it is correct to second order in v/c, and not merely to first order, like the Fresnel ether drag idea. I hope you see the beauty of Hoek's experiment! It is sort of like sitting back and scratching your navel to discover truths of the universe.

You say on page 216, line 13 etc., of your THORNY WAY OF TRUTH PART VII that with independent toothed-wheels one cannot know the relative phase of the two spinning wheels. You are wrong!!! One can, of course, know and adjust the relative phase as one wishes. If you had actually bothered to study my paper you would see that the relative phase is measured very accurately by a method of triangulation. Two beams from wheel one, converging slightly, pass through two gaps in wheel two. Measuring relative intensity differences to 10^{-5} then gives the *(to the desired accuracy)* relative angular position of the two toothed wheels (which are turning at the same rate accurate enough for the determination of the absolute velocity of the laboratory).

You, like all the "true believers" in "special relativity", invent problems that don't exist in order to support your thesis. You have some nonsensical idea, like all the other believers in "special relativity" that clocks must be "synchronized"; therefore, N O experiment is possible that can possibly refute your dogmatic, unwarranted, stupid claim! You close your mind completely! You cannot even read a simple experiment to see what it involves. And what is more irritating is your perpetual claim to be against "special relativity". You are stuck at the Builder level of "special relativity". You, like Prokovnik,* are an apologist for "special relativity". If you would admit your rigid religious conviction that "special relativity" is right, then one could more readily understand what you try to say. This is the reason, I assume, for your refuting the obvious experiments to claim the Biot-Savart law; it is the law necessary for "special relativity". Etc. I wish you were not so rigid and could learn. I wish you were able to see when you are wrong. I have changed my mind a dozen times about things since I have known you; I cannot recall that you have ever changed your mind about a single thing! Your stance is charaterized by your "axiomatic physics"; no science can be "axiomatic". No physics theory can ever be correct in any absolute sense; it is only correct to within experimental error and or else to within certain convenience criteria. One has to be ready to change with the changing evidence!

Also see inclosed a preprint showing that terrestrial Bradley aberration cannot be observed. I have changed my mind! I was wrong!

Yes, count me in on the Milano Conference. It is good that you are so charming, talented, intelligent, amusing and that you are a gentleman (I am not!); so that I can value you as a good friend; because I do not approve of much of your physics.

*Prokhovnik

J. P. WESLEY, *Ph.D. Physicist*

Weiherdammstr. 24, *7712 Blumberg, West Germany,* Tel: 07702-658

13 April 1990

Francisco J. Müller
8470 SW 33rd Terrace
Miami, FL 33155

Dear Francisco,

I finally got around to studying your new experiments that you describe in your letter of 16 January 1990. The linearization of the displacements (instead of rotations) helps to close a possible loop hole. I was satisfied with the rotations; but believers in "special relativity" might need the extra information.

The removal of the semicircular magnets (one or both) (which you label cm/2) does not seem very interesting to me. When these ends are removed the situation becomes too complicated or "dirty". I cannot see that it represents anything fundamental. I am not going to try to predict what the removing of these ends should produce. There will now be local time changes of the magnetic field **B** in the yoke. One might see the situation as a change in the <u>reluctance</u> of the magnetic circuit when the magnets without ends are moved. The reluctance will decrease as the magnets are displaced from their central symmetric position. Moving the magnets in time then produces a time rate of change of the reluctance, which in turn produces a time rate of change of the magnetic flux through the upper yoke, which then finally produces a time rate of change of the flux through the electric circuit IECR, which then yields a time changing emf. This seems like all straightforward Maxwell theory. I see no fundamental principle that is being tested (????!)

I hope your formal education does not brain wash you into accepting too much of the traditional establishment physics that is fundamentally wrong. Most professors are too timid to think for themselves; they usually hide behind a textbook which safely represents the establishment views - which are, as you know, wrong in some very important fundamental ways.

I also hope you do not take Stefan Marinov too seriously. I would not call him a "non-relativistic physicist"; as he believes in "time dilation", the Biot-Savart law, and (as far as I can tell) most of the rest of the "special relativity" nonsense. He does, however, recognize the need for <u>absolute</u> space, which is not too unusual these days for some apologists for "special relativity", such as Builder and Prokhovnik. It is difficult to discover exactly what Marinov believes; but it seems to me that he is now rejecting experimental result which do not happen to agree with his ideas. As you know, he no longer believes in Newton's third law, the conservation of linear momentum, nor the conservation of energy. I no longer know what he <u>does</u> believe in. So don't take him too seriously.

Enclosed is a review paper on Weber electrodynamics. The mathematics may be a bit advanced for you (?). The Weber theory when generalized to fields fits <u>all</u> of the known experimental facts. It predicts results that the Maxwell theory and the Biot-Savart law cannot predict. And the Weber theory satisfies Newton's third law, the conservation of linear momentum, and the conservation of energy.

Keep me informed of your progress and your experiments.

best regards Paul

copies to Marinov and Pappas

ANNALS OF PHYSICS

April 17, 1990

Dr. Stefan Marinov
Morellenfeldgasse 16
A-8010 Graz Austria

Dear Dr. Marinov:

I regret to inform you that the Editorial Board has decided not to publish
your paper, "The Myths in Physics," which you kindly submitted to the
ANNALS OF PHYSICS. We feel that your field of research is unsuitable for
our journal.

Thank you for having considered us. We are returning the manuscript to you
under separate cover.

Sincerely,

Herman Feshbach/eos

Herman Feshbach
Editor

HF/eos
Reference 62354

Editorial note. With this letter Prof. Fesh-
bach answers Marinov's letter
of the 23 March 1990.

I. námestník ministra
školstva, mládeže a telesnej výchovy
Slovenskej socialistickej republiky

Ján PIŠÚT

Stefan Marinov
Morellenfeldgasse 16
A-8010 Graz, Osterreich

Bratislava, April 17,1990

Dear Mr.Marinov,

thank you for your letter of March 20th. I apologize
very much for the delay, but I am temporarily serving at the
Slovak Ministry of Education and only once per 10 days or so
I am picking up my mail at the Dept. of Physics at the
University.

I am very sorry that the reception of your paper at the
Bristol office of EJP was not acknowledged. As far as my
experience goes the EJP office in Bristol has been working
always in an excellent and correct way. I must confess I do
not remember ever having seen your paper "The
demonstrational Muller-Marinov machine" - but that may be
due to failures of my memory.

Please submit your paper again to the Bristol office
and asked them to have it refereed in the standard way.
Unfortunately, I cannot be helpful in this matter, since my
term of office as the Honorary Editor of EJP came to the end
in Dec.89.

Thanks also for your poems. I found them very nice. In
what concerns my daughter* and your paper I shall try to
persuade her to read the paper - the problem is not the
language, she only pretends that she is always completely
occupied.

Best wishes
Sincerely Yours

Ján Pišút

---

*
Marinov's note. For saving place I do not reproduce here my letter to Dr. Pisut.
Concerning Dr. Pisut's daughter (who still is visiting a college)
I wrote the following:

Some time ago you wrote in the EUROP. J. PHYS. that your daugher said once to
you, after returning from school, that in physics "there is no fun". I think
that in my paper there IS fun. Moreover, I beg you not to send my paper to some
professor for examination BUT TO YOUR DAUGHTER.

WORLD SCIENTIFIC PUBLISHING CO. PTE LTD

18 April 1990

Prof Stefan Marinov
Morellenfeldgasse 16
A-8010 Graz
Austria

Dear Prof Marinov

RE: MODERN PHYSICS LETTERS A (MPLA)

I am sorry to inform you that your paper entitled

"PROPULSIVE AND ROTATING AMPERE BRIDGES VIOLATE THE PRINCIPLE OF RELATIVITY".

has not been recommended for publication.

Please find enclosed a copy of the referee's report. I wish to thank you for submitting your paper to our journal and hope that we will be able to publish some further work by you.

Yours sincerely,

Editorial note. Marinov answers this letter with his
letter of the 25 April 1990.

E H Chionh (Ms)
Editor (MPLA)

Comments on the paper
Propulsive and Rotating Ampere Bridges Violate Principle of Relativity
by S. Marinov

1. This paper reads like it has been taken straight from a earlier book/monograph of the author, with certain referencing not consistent with a research paper.

2. The paper also reads like one more suitable for an engineering journal than Modern Physics Letters – there are lots of details, but yet the reader does not get totally convinced by the claims in the paper. There is simply too much unorthodox jargon about the experimental setup, which is at least acceptable if there are sufficient *calibration studies* – i.e. certain known results can be shown to be reproduced by the setup.

My general feeling is that I cannot recommend this paper for publication in the Modern Physics Letters, partly because I am not convinced of the claim of the violation of the principle of relativity made (and supposedly verified) in the paper, and partly because the nature of the paper is not in line with the editorial policy.

SOCIETÀ ITALIANA DI FISICA

IL NUOVO CIMENTO

VICE DIREZIONE «B»

April 20, 1990

Prof.S. MARINOV
MORELLENFELDGASSE 16,
8010 GRAZ, AUSTRIA

1175 NCBR

Dear Prof.   Marinov,

I have read your paper " repetition of whitehaed'd etc. "with great
interest and attention. I regret to say, however, that ideas you present on
issues of physical theories generally regarded as well-settled appear to
be more appropriate for journals specifically conceived for that purpose,
like *Foundations of Physics, Speculations in Science and Technology,* or
International Journal of Theoretical Physics.

It is the opinion of this new editorial board that *Il Nuovo Cimento "B"*
will mainly address topical physical issues.

1176 NCBR

I have read your paper " MAXWELL' S ILLUSION etc. "with great
interest and attention. I regret to say, however, that ideas you present on
issues of physical theories generally regarded as well-settled appear to
be more appropriate for journals specifically conceived for that purpose,
like *Foundations of Physics, Speculations in Science and Technology,* or
International Journal of Theoretical Physics.

It is the opinion of this new editorial board that *Il Nuovo Cimento "B"*
will mainly address topical physical issues.

Cordially,

Editorial note. With this letter Prof. Ruffini
answres Marinov's letter of
the 23 March 1990.

Remo Ruffini
Vice-Director

PHYSICS ESSAYS

23 April 1990
PE2253/kla

Dr. Stefan Marinov
Institute for Fundamental Studies
Morellenfeldgasse 16
A-8010 GRAZ.
AUSTRIA

Re: Manuscript: PROPULSIVE AND ROTATING AMPERE BRIDGES VIOLATE THE PRINCIPLE OF
RELATIVITY, by Stefan Marinov, submitted for publication in Physics
Essays (received 19 July 1989)

Dear Dr. Marinov:

Please find enclosed a review of your paper.

As you probably know, the Editorial Policy of "Physics Essays" prescribes that authors
should take an objective and careful look at the reviewers' reports in order to see if there are
elements of value that can be used to improve the quality of their papers, on both the aspects of
correctness and of clarity of exposition, and this is what I am encouraging you to do now.

In particular, I believe you should address the serious questions raised by the reviewer
because they seem to indicate that your experiments were not capable of proving your point.

Since I would like to keep a tight schedule on this process of revision, I would therefore
like to have your revised manuscript, in triplicate, back to me by 8 June 1990, at latest.
Moreover, in retyping the manuscript, please follow the Instruction to Authors here enclosed.

I thank you for having submitted your paper to us.

Sincerely yours,

Editorial note. Marinov answers this letter with
his letter of the 3 May 1990.

E. Panarella

Encl.

EP/kla

Comments on:

PROPULSIVE AND ROTATING AMPERE BRIDGES VIOLATE THE PRINCIPLE OF RELATIVITY

by Stefan Marinov

Marinov claims <u>selfpropulsion</u> for the linear and rotational motion of an Ampere bridge (U-shaped conductor). If the operative force law were the Grassmann law, his claim would be valid. There is now ample experimental evidence available proving that, in selfcontained circuits, the reaction forces between parts of the circuit are governed by Ampere's law and not by Grassmann's. Twelve of these experiments are described in Graneau's book [P. Graneau, <u>Ampere-Neumann electrodynamics of metals</u>, Hadronic Press, Nonantum MA, 1985].

The Grassmann law fails precisely in the situations which are being considered by Marinov. All experiments performed with metallic conductors in the 165 years since Ampere formulated his law have been in agreement with this law.

The Ampere law easily explains the various motoring actions shown in this paper. The propulsion forces are reaction forces between parts of the circuit which can move relative to each other. Consider figure 1. When ABC and FGH are stationary and sliding contacts are provided at C and F, the bridge will move in the f3-direction, but the f3-forces are not acting on DE. They are longitudinal forces in CD and FE produced by repulsion across the corners from BC and GF.

The same longitudinal forces also explain the rotation of the motor when BC and GF are metal discs with sliding contacts to CD and FE.

The Ampere force law predicts that Marinov's motor will not rotate if C and F are welded conductor junctions and AB and GH are made very long compared to DE. The current from a battery or other source would then have to be introduced with slipping contacts at A and H. Also the battery leads would have to be kept well away from the motor. If the motor still rotates with these modifications, then I accept Marinov has proved his point of selfpropulsion.

STEFAN MARINOV
Morellenfeldgasse 16   25 April 1990
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

Ms. E. H. Chionh
Editor of MPLA
Farrer Road
P.O.Box 128
Singapore 9128

Dear Ms. Chionh,

Thank you very much for your letter of the 18 April, although, I must confess, the rejection of my paper PROPULSIVE AND ROTATING AMPERE BRIDGES VIOLATE THE PRINCIPLE OF RELATIVITY was, of course, not pleasant for me.

I consider the objections of the referee untenable. Here are my motivations.

I refer to certain books published by me. The referee objects that such a referencing is not consistent with a research paper. He has no right for such an assertion, as any author can refer to ANY PUBLISHED BOOK. It is true that I have a WHOLE series of papers (with an extreme importance) which are published only in my books. All these papers have been SUBMITTED to research journals and rejected as they contradict the theory of relativity which is accepted as true by the establishment. All my papers are based on EXPERIMENTAL evidence. This evidence DRASTICALLY contradicts the theory of relativity. The only way to bring the information on my experiments to the scientific community is the publication of my papers in research journals.

The second objection of the referee is that my paper is "more suitable for an engineering journal than Modern Physics Letters". My paper shows the untenability of a fundamental PHYSICAL theory. This problem is NOT an engineering problem. The fact that my experiment is childishly simple and any engineer can reconstruct it is NOT a motivation that the paper is an "engineering paper".

Another objection of the referee is that "the reader does not get totally convinced by the claims in the paper". If the referee has certain questions or some aspects of my experimental setup and theoretical explanation of the effects is not clear to him, he has to ask me. Otherwise his assertion is GROUNDLESS. If the referee would like to have some "calibration studies" of my experimental setup, he has to note WHICH.

I know very well that the claims of my experiment will be UNPLEASANT for ANY supporter of the principle of relativity. But an experiment is AN EXPERIMENT. And its results MUST BE ACCEPTED.

I can further discuss with the present referee only if he will answer the following four questions (by "yes", "no", or "I do not know"):

1. Will the Rotating Ampere Bridge with Sliding Contacts rotate when current will be sent ?

2. Is the rotation of the rotor due only to internal forces?

3. Is there an interaction of the currents in the conductors solid to the laboratory with the currents in the conductors solid to the rotor supporting the rotation?

4. If the referee has answered the first question by "yes", will be a back electromotive tension induced in the rotor's conductors? (Of course, also questions 2 and 3 must be answered only if question 1 is answered by "yes".)

I repeat, if the referee will not answer these four questions, I do not like to maintain any contact with him. As I am sure that the referee will deny to give answers to these questions, I beg you to send my paper (with the referee's comments and my present letter) to an arbitrator who has to decide whether the paper must be rejected or accepted.

I submit also another VERY IMPORTANT paper (in a single copy)

CHILDISHLY SIMPLE EXPERIMENT VIOLATING THE PRINCIPLE OF RELATIVITY.

The PACS number is 03.30. Additional numbers 03.50, 41.10.

Please, acknowledge the reception of this letter and send me in due time your final decision.

Sincerely yours,

Stefan Marinov

Editorial note. This letter is answered by Ms. Chionh with her letter of the 14.IX.90.

STEFAN MARINOV
Morellenfeldgasse 16   Den 1 Mai 1990
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

An den Genossen
Michael Garber
Chefredakteur
DIE VOLKSSTIMME
Höchstädtplatz 3
A-1206 Wien

Kopie: An die Organisation
der KP, Graz

Sehr geehrter Genosse Garber,

Ich schicke Ihnen meinen Artikel

DEM CHRISTLICHEN KOMMUNISMUS ENTGEGEN,

um ihn (zusammen mit dem Artikel MEINE VORSCHLÄGE ZU DEM 27. PARTEITAG) in der VOLKS-
STIMME zu publizieren.

Das Bild von der Maschine TESTATIKA muß etwa in der angegebenen Größe erscheinen.
Der Artikel "MEINE VORSCHLÄGE..." muß im Rahmen erscheinen.

Als ich den Artikel "MEINE VORSCHLÄGE..." in November zum Druck in der VOLKSSTIMME
vorgelegt hatte, habe ich, außer mit Genossen von der Grazer Organisation, auch mit
dem Genossen Eduard Danzinger   gesprochen und ihm angeboten, nach Wien zu kommen,
um ein längeres Gespräch mit den Genossen von der ZK zu führen. Mein Angebot wurde
nicht angenommen.

Wenn es nötig wäre, ich werde auch diesmal gern nach Wien kommen. Ich stehe aber
fest, den Artikel genau in diesem Wortlaut zu veröffentlichen. Also ich werde nicht
kommen um über den Inhalt des Artikels zu handeln, sondern die Genossen über die Hinter
gründe meines Artikels (Stalinismus, Metherñitha, TESTATIKA) zu informieren.

Es ist Zeit die österreichische KP von der Isolation herauszuziehen und als eine
angesehene Partei in das Parlament zu bringen. Ich hoffe, daß die Leute, die das tun
können,   nicht aus der Partei entlaßen werden.

Mit freundlichem Gruß:

J. Marinov

Stefan MARINOV

PS. Ich werde mich freuen die Korrekturen des Artikels, IN DER FORM IN DER DER ARTIKEL
    erscheinen wird,   zu bekommen. Ich tue das mit ALLEN meinen Artikeln!

Ich bitte Sie auch, nach dem Druck mir das Foto der Maschine TESTATIKA zurückzu-
schicken, weil ich es dringend brauche.

Ich werde mich sehr freuen, wenn mein Deutsch wird sprachlich ausgebeßert.

Editorial note. This letter is answered by the letter of the journal DIE VOLKSSTIMME
            (CC of ACP) of the 21 May 1990 and by the letter of the journal DER
            STANDARD of the 26 June 1990.

DEM CHRISTLICHEN KOMMUNISMUS ENTGEGEN

In November 1989 hab ich den in Rahmen gedruckten Artikel der Parteizeitung DIE VOLKS-STIMME vorgelegt und die Grazer Organisation und das ZK der KPÖ gebeten mich, Mitglied der KPÖ, an den Parteitag in Januar 1990 zuzulaßen, um die Botschaft der Kommune METHER-NITHA zu verkünden. Der Artikel wurde abgelehnt und mir wurde nicht erlaubt dem Parteitag beizuwohnen.

Seit November geht der Zerfall des Weltstalinismus mit raschen Schritten weiter. Der Tyran Ceausescu und seine besessene Gattin sind erschoßen. In der Sowiet Union, in der DDR und in Bulgarien enthüllt man die Massengräber, wo tausende und aber tausende unschuldige Menschen begraben wurden. Aber die Reste von vielen der Opfern wird man nie finden. Wie jetzt eine Kommission des bulgarischen Ministeriums für Innere Angelegenheiten mit Vernahme von Zeugen (Häftlinge und Aufseher) festgestellt hat, hat man in den mehr als 50 bulgarischen Vernichtungslagern mit den Leichen der erschoßenen, aus Hunger, Krankheiten und Leiden gestorbenen, und bis zum Tode verprügelten Leute riesiege Lagerschweine gefüttert, deßen Fleisch nachdem auf den Märkten Bulgariens verkauft wurde, so daß eine ganze Bevölkerung, ohne es zu wissen, in Anthropophagen verwandt wurde. Im Vergleich mit den bulgarischen stalinistischen Degeneraten, die Hänker Himmlers mit dem Badetheater in den Gaskammern und mit ihren komischen Verbrennungsofen treten nur als elende Dile-tanten vor.

Wenn wir die letzten Nachrichten von Albanien hören, können wir sagen: Der Stalinis-mus in Europa ist tot.

Die österreichische KP muß auch das letzte Kreuz über ihre stalinistische Vergan-genheit legen, die, dank daß die KP an die Macht nicht gekommen war, die Greueltaten ihrer östlichen "Bruderparteien" nicht begangen hat. Um dieses letzte Kreuz zu legen, schlage ich vor, den Vorsitzenden der Partei in den letzten Jahrzehnten, den Genossen Franz Muhri von der Reihen der Partei auszuschließen.

Viele von den kommunistischen stalinistischen Parteien haben sich umgenannt. So die bulgarische kommunistische Partei hat sich sozialistische Partei genannt. Die Leiter der Partei sind aber die alten Aparatschicks geblieben. Das ist nur ein widerliches Zirkus, von Leuten gedreht, die nur ein einziges Ziel in ihrem Leben haben - an die Macht zu bleiben.

Ich wende mich nochmals an die Mitglieder der österreichischen KP mit dem Aufruf: Macht Schluß mit dem Stalinismus und mit der Akrobatik das zu retten, was nicht zu retten ist, weil furchtbar stinkt. Benennt unsere Partei in christlich-kommunistische Partei und verkündigt den wahren christlichen Kommunismus, der uns von dem Unsinn der kapitalistischen Ausbeutung von Erde und Mensch retten kann. Mit der Entdeckung der Ma-schine TESTATIKA haben wir Energie in Hülle und Fülle, reine, saubere Energie aus NICHTS. Luft, Wasser und Boden sind auf unserer Erde genug. Wir brauchen nicht von Morgen bis Abend wie verrückt zu rennen um dem grausamen Mammon zu dienen und die Luft, das Wasser und die Erde zu versuchen, sondern in Frieden, in Demut und in gemeinsamer Verständi-

gung das zu genießen, was die Gnade Gottes uns geschenkt hat.

Wenn dieser Artikel bis zum 15 Mai nicht gedruckt wird, werde ich ihn der freien Presse vorlegen und von der Reihen der österreichischen KP, aus Protest gegen die Verbleibsel der antidemokratischen "Mundbindung" von der jungsten Vergangenheit, austreten.

Stefan MARINOV, Graz.
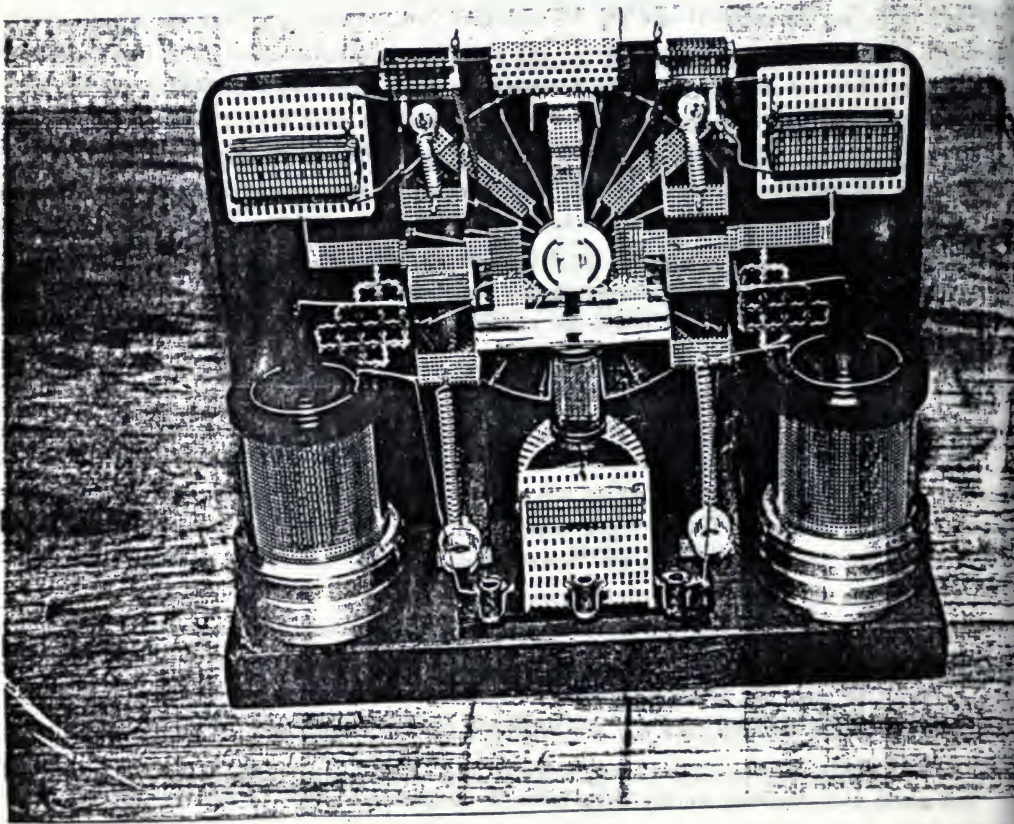
Ein kleines Modell der Maschine TESTATIKA, die Energie aus nichts (oder aus einer u bekannten Quelle) produziert. Die etwa größere Modelle liefern 3 kW freie Energie der Form von Gleichstrom von 10 A unter Spannung von 300 V). Eine große Maschine (Scheibendurchmesser 2 m), die fast gebaut ist, wird 30 kW freie Energie liefern.

MEINE VORSCHLÄGE ZU DEM 27. PARTEITAG DER KPÖ.

von Stefan Marinov, Graz

> Noi arriviamo da lontano.
>
> E. Berlinguer

Ich komme in die Reihe der österreichischen Kommunisten nach einem langen Weg. Mein Vater war noch vor dem I. Weltkrieg Mitglied der bulgarischen Partei der engen Sozialisten (die erste, seit 1903, kommunistische Partei der Welt). Die Schwester meiner Großmutter war Mitglied des ZK der illegalen kommunistischen Partei Bulgariens zwischen den beiden Weltkriegen. Während des zweiten Weltkrieges als Kind half ich meinen Eltern in ihrem Kampf gegen den Faschismus in Bulgarien. Viele Angehörigen meiner Familie, die in Bulgarien, Spanien und in der Soviet Union für den Kommunismus kämpften, haben für ihre Ideen mit dem Leben bezahlt - zwei Onkel und eine Tante in den stalinistischen Vernichtugslagern.

In so einer Familie hat mir die stalinistische Pest zu früh die Augen gestochen und zu früh hab ich verstanden, daß der Kampf für den Kommunismus zuerst ein Kampf gegen den Stalinismus sein muß. Als Sohn einer angesehenen kommunistischen Familie hat das bulgarische KGB etwas gezögert mich zu verhaften und zuerst wurde ich nur verhört und gewarnt. Aber in 1966 saß ich schon in dem Sofioter Zentralgefängnis und dann wurde ich für Jahre in den Psychiatrien von Sofia von meinen "verrückten" Ideen und Handlungen zwangshaft "geheilt", bis ich in 1977 ausgewiesen wurde und meine bulgarische Bürgerschaft   . mir aberkannt wurde.

Wir erleben einen raschen Zusammenbruch des Stalinismus im Osten. Der Stalinismus aber hat sich zu tief mit dem Kommunismus verschmolzen. Es besteht die Gefahr, daß der Zusammenbruch des Stalinismus zu einem Zusammenbruch des Kommunismus führen würde. Als Kommunist seit meiner Kindheit möchte ich diesem Zusammnbruch wíderstehen.

Darum wende ich mich an den 27. Parteitag mit den folgenden Vorschlägen:

1. Die österreichische KP muß klar und deutlich sich von dem Stalinismus distanzieren.

2. Als ein unentbehrlicher Schritt auf diesem Weg schlage ich vor, daß sich der Parteitag mit einem Brief bei der Genossin Elisabeth Koplenig und bei allen mit ihr aus der Partei ausgeschloßenen Genossen sich entschuldigt und sie alle bittet,wieder in die Partei einzutreten.

3. Der Parteitag muß klar und deutlich der Perestroika in der SU seine Unterstützung geben und allen demokratischen Kommunisten wie Gorbatschew, Eltzin, Djilas, Dubcek seine Sympatien aussprechen.

4. Die stalinistischen Totengräber des Kommunismus, in erster Reihe der rumänische Tyran Ceausescu,muß verurteilt sein.

5. Als die christliche Kirche eine Staatskirche wurde, hat sie sehr schnell die Ideen des Evalngeliums verraten. Es waren viele Bewegungen (Bogomilen, Kataren), die das Christentum wieder zu der Lehre Jesu zurück bringen wollten. Die stärkste solche

Bewegung in dem XIX-en und XX-en Jahrhundert war der Kommunismus. Die Stalinisten haben
die wahre Wurzel des Kommunismus abgeschnitten und einen widerlichen Kampf gegen die
Christen und gegen das Christentum entfalten. Nach dem Zusammenbruch des Stalinismus im
Osten werden die Volksmassen wieder zu den korrupten katholischen, protestantischen und
orthodoxen Kirchen für seelische Rettung suchen. Um das zu vermeiden, müßen die kommuni-
stischen Parteien klar und deutlich sagen, und es mit Taten beweisen, daß sie die wahre
Nachfolger Christi sind. Einen wichtigen Schritt in diese Richtung kann die österrei-
chische KP erweisen, wenn sie an diesem Parteitag den Namen christlich-kommunistische
Partei nimmt. Damit können wir eine große Hilfe der KP Italiens leisten, die seit gerau-
mer Zeit nach einem neuen Namen vergebens sucht. Vergessen wir nicht die profetischen Worte
Dostojewskis zu der   -. Welt des goldenen Kalbes: "Nur wenn glaubenden Kommunisten
kommen, wird ihr Reich des Geldes zu Grunde gehen."

   6. Der "Kommunismus" im Osten hat zu einem Zerfall der Wirtschaft geführt. Als Ret-
tung von diesem Zerfall sucht man jetzt wieder den kapitalistischen Weg. Dieser Weg ist
kein Weg. Ich möchte dem Parteitag bekanntmachen, daß echt kommunistisch geführte Wirt-
schaft nicht zum Zerfall führt, sondern der einzige Weg von der ökologischen Krise ist,
in welcher die ganze Erde versinkt. Ich möchte von der christlichen Kommune METHERNITHA
in der Schweiz berichten, wo die Leute eine gerechtigte und gesunde Wirtschaft haben und
wo sie in Ruhe, sozialer Sicherheit und gemeinsamer Liebe leben. Jeder kann in die Kommune
eintreten, der Lahme und der Gescheiterte, der Jude und der Grieche. In dieser Kommune
wurde die Maschine TESTATIKA gebaut, die Energie aus nichts produziert und die also die
verhängnisvolle energetische Krise der Welt   löst (ich habe diese Maschine und ihre
physikalische Grundlage in meinem Buch THE THORNY WAY OF TRUTH, Band V, der Welt bekannt-
gegeben). Die Kommune aber glaubt, daß eine kapitalistische Welt   diese energetische
Quelle nicht zum Gunsten der Menschheit ausnützen würde, sondern um ihren   Niedergang
noch zu beschleunigen. Ich wende mich zu dem 27. Parteitag der KPÖ mit der Bitte, mir
die Möglichkeit zu geben die Botschaft der Kommune Methernitha, deren Mitglied ich bin,
der Welt bekannt zu machen: Kehrt zu den Prinzipien des christlichen Kommunismus, bevor
es zu spät wäre. Die technologischen Probleme für eine gesunde Zukunft der Menschheit
sind gelöst. Aber ohne die geistlichen Probleme zu lösen, wird uns die Technologie allein
nicht retten.

PHYSICS LETTERS A

Editorial note. With this letter Prof. Vigier answers Marinov's letter of the 9.IV.90.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

3 May 1990

Dr. E. Panarella
PHYSICS ESSAYS
Nat. Res. Council
Room 100, Bldg. M10
Ottawa
Ontario K1A OR6
Canada

Dear Dr. Panarella,

Thank you very much for your letter of the 22 April 1990 concerning my paper PE2263/k1a.

I have answered the referees comments in a polite form in the addendum to my paper.

In this letter I should like to tell you, that NOBODY has calculated the most simple interactions of loops by the help of Ampere's formula, as the calculations are terribly difficult from a mathematical point of view. Pappas always asserts that he makes the calculations by the help of computors. Attached is p. 183 of my book THE THORNY WAY OF TRUTH, Part VII, where I offer to Pappas to make certain SIMPLE calculations by the help of Ampere's formula and if he will be successful, I shall pay him for any of the calculations good amounts of money. Until the present day I have not received certain of these problems solved. You can give this p. 183 to my anonymous referee. If he will be able to solve the posed problems, he will receive the indicated sums.

Ampere's formula is a very CLEVER formula, as it ALWAYS (for closed loops) gives the results which can be obtained also by the Grassmann's formula. But this is only a theoretical conclusion. When one tries to calculate something by the help of Ampere's formula, one encounters TERRIBLE mathematical difficulties.

However, the decisive answer that Grassmann's formula is the right one and that Ampere's formula is wrong give my Bul-Cub Machine without stator and my Rotating Ampere Bridge with Displacement current. If you will be interested the reports on these experiments, I shall gladly send them.

The PACS numbers of the paper are: 03.30, 03.50, 41.10.

I beg you to send the invoice for the page charge to the following address:

> Dipartamento di matematica (to the attention of Prof. Bartocci)
> Universita di Perugia
> via Vanvitelli 1
> I-06100 Perugia, Italy.

The photograph of the RAF-machine is enclosed only to the first copy (original) of my paper. After using the photograph, I BEG YOU VERY MUCH to send it back to me.

Hoping to receive an acknowledgement for the reception of this letter and then in due time also your final decision,

Sincerely yours, Marinov

ADDENDUM

The anonymous referee of the above paper who is supporter of Ampere's formula for th
interaction of two current elements and who considers Grassmann's formula (1) as <u>wrong</u>,
presented the following objection:

> The Ampere law easily explains the various motoring actions shown in this paper.
> The propulsion forces are reaction forces between parts of the circuit which can
> move relative to each other. Consider figure 1. When ABC and FGH are stationary
> and sliding contacts are provided at C and F, the bridge will move in the f_3-direc
> tion, but the f_3-forces·are not acting on DE. They are longitudinal forces in CD
> and FE produced by repulsion across the corners from BC and CF.
>
> The same longitudinal forces also explain the rotation of the motor when BC and
> GF are metal disks with sliding contacts to CD and FE.
>
> The Ampere force predicts that Marinov's motor will not rotate if C and F are
> welded conductor junctions and AB and GH are made very long compared to DE. The
> current from a battery or other source would then have to be introduced with sli-
> ding contacts at A and H. Also the battery leads would have to be kept well away
> from the motor. If the motor still rotates with these modifications, then I accept
> Marinov has proved his point of selfpropulsion.

I agree that Ampere's formula predicts for my rotating Ampere bridge a torque in the
same direction as predicted by Grassmann's formula.

However the referee is not right when asserting that if the wire BCDEFG in fig. 1 is
solid with sliding contacts at the points B and G and with a rotational degree of free-
dom about the axis ABGH, then according to Grassmann's formula a rotational moment
(torque) should exist (according to Ampere's formula, of course, the torque in such a
case must be null).

Now I shall show that also according to Grassmann's formula the rotational moment
must be null.

Indeed, if assuming BC = CD = DF = EF = FG = a and if transferring the origin of the
reference frame from point D to point B, the moment of the forces $\mathbf{f_3}$, which will drive
the bridge clockwise about the axis ABGH, will be given by the following formula (see
formula (6))

$$\mathbf{M_{f_3}} = 2R(-\hat{\mathbf{z}}) \times \int_{a_0}^{a} df_3\hat{\mathbf{y}} = (\mu_0 I^2/2\pi)R \int_{a_0}^{a} (dx/x)\hat{\mathbf{x}}, \qquad (18)$$

while the moment of the forces f_2 and f_1, which will drive the bridge counter-clockwise about the axis ABGH, will be given by the following formula (see formula (6), introducing respective changes in the notations and taking into account the action of the forces around the point C as well as around the point F)

$$\mathbf{M}_{f_2} + \mathbf{M}_{f_1} = 2\int_{a_0}^{a} y\hat{y} \times df_2(-\hat{z}) + 2\int_{a_0}^{a} (R-z)(-\hat{z}) \times df_1(-\hat{y}) =$$

$$= (\mu_0 I^2/2\pi)\int_{a_0}^{a} dy(-\hat{x}) + (\mu_0 I^2/2\pi)R\int_{a_0}^{a}(dz/z)(-\hat{x}) + (\mu_0 I^2/2\pi)\int_{a_0}^{a} dz(\hat{x}) =$$

$$= (\mu_0 I^2/2\pi)R\int_{a_0}^{a}(dz/z)(-\hat{x}). \tag{19}$$

Thus we obtain

$$\mathbf{M}_{f_3} = -(\mathbf{M}_{f_2} + \mathbf{M}_{f_1}), \tag{20}$$

so that the net torque will be null.

It is common place that the forces of interaction of two current loops or of the two parts of one current loop (which always can be presented as two loops, by connecting the border points by two infinitely near parallel wires along which the same current as in the loop flows in opposite directions) calculated according to the Ampere and Grassmann formulas are equal. These forces are different only when the one (or both) of the interacting loops contains condensers (with dielectric or without dielectric). As the so-called "displacement current" which "flows" between the plates of a condenser neither acts on other currents with potential magnetic forces nor can be set in motion by the potential action of other currents, such loops containing condensers present interrupted loops. First Graham and Lahoz[8] and then the present author[3,4] with the help of his Bul-Cub machine without stator and his Rotating Ampere Bridge with Displacement Current have shown that Grassmann's fromula is the right one and Ampere's formula is wrong.

Here I should like to note the following simple but important conclusion: The force acting on the Ampere bridge does not depend on the form of the bridge; consequently a Π-form and a U-form bridges are pushed by the same forces. If this will be not true a rectangular loop,   on the one side with U-form end, will be set in motion by internal forces. In Ref. 9 I showed that the pushing force acting on a U-form bridge does not depend on the radius of curvature.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

7 May 1990

Prof. J. P. Vigier
PHYSICS LETTERS A
11 Rue P. et M. Curie
F-75231 Paris Cedex 05

Dear Prof. Vigier,

Thank you for your letter of the 2 May with which you answered my letter of the
9 April and for the final rejection of my papers V1280a and V1281a.

In my letter of the 9 April I wrote:

Concerning my second paper V 1281a you write in your letter:

It has not proved possible to elicit a report on V 1281a. I have looked throu
the paper and I cannot find material deserving rapid publication.

In my paper I give the report on an experiment where according to conventional
physics there MUST BE an effect. Meanwhile I established EXPERIMENTALLY that
THERE IS NO EFFECT. And you assert that the report on SUCH an experiment does
not deserve rapid publication. I AM WONDERING! To show to you how important is
my experiment, I pose to you the following question:

Will, ACCORDING TO YOU, the ring in fig. 4 of my paper rotate when filled with
dielectric or not?

If you will not answer this question by "yes" or by "no", I remain with the opi-
nion that you have NOT read my paper. Thus I shall accept the rejection of my
paper, only if you will answer the above question by "yes" or by "no" (or by
"I do not know").

As I expected you did NOT answered my question. Thus you have rejected my paper
WITHOUT HAVING GIVEN IT TO A REFEREE AND WITHOUT HAVING READ IT. This is a VERY DANGE-
ROUS TREND in the publication policy of ALL physical journals of the world (see confir
mation of this assertion in my collection of documents THE THORNY WAY OF TRUTH). The
physics journals of the establishment, seeing that many of the basic concepts of con-
ventional physics are WRONG, search   to preserve the status quo by rejecting to pub-
lish the experimental evidence showing that these basic concepts are wrong.

I shall submit my papers to another journal but I expect also in the next journal
exactly the same answer as yours.   <u>Editorial note</u>. Paper V 1281a entitled "Repetitior
is published in TWT-VII, p. 26.

Now I submit another paper (in a single copy, as I see that you even do not send my pape
to a referee). If the paper will be rejected, I again ask you to give an answer by "yes"
"no" or "I do not know" to the following questions:

1. Will, according to you, the inverse rotational Rowland experiment give a positiv
effect?

2. Will, according to you, the inverse rotational Rowland experiment give a null eff

3. Will, according to you, the inverse inertial Rowland experiment give a positive
effect?

4. Will, according to you, the inverse inertial Rowland experiment give a null effe
The title of the paper is (PACS etc. as for all my papers)

CHILDISHLY SIMPLE EXPERIMENT VIOLATING THE PRINCIPLE OF RELATIVITY.

I hope that this time either you will accept the paper, or will give answers to the
above questions after rejecting it. If you will do neither the first not the second,
I can only say "O tempora, o mores!"   Yours: Stefan Marinov
<u>Editorial note</u>. Answered on the 14.V.90.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

9 May 1990

Prof. Remo Ruffini
IL NUOVO CIMENTO B

P.le Aldo Moro 2
I-00185 Roma

Dear Prof. Ruffini,

Thank you for your letters of the 20 April with which you rejected my papers REPETI-
TION OF WHITEHEAD'S... (Nr. 1175) and MAXWELL'S ILLUSION... (Nr. 1176).

You suggest that I submit these papers to FOUND. PHYS., SPEC. SC. TECHN. and INT. J.
THEOR. PHYS. My relations with the editors of these journals are the following:

1. After visiting me in Sofia in 1976, Prof. Yourgrau published about 10 of my papers
in FOUND. PHYS. After his death, Prof. van der Merwe, his successor, continued with the
publication of my papers but in 1984 he wrote me that being under the pressure of the
relativistic lobby he must stop with the publication of my papers, otherwise the finances
of his journal will be ruinated, remaining, nevertheless, a good friend of me and writing
me every now and then a letter.

2. I publish papers in SPEC. SC. TECHN. Recently (**12**, 187, 1989) I published there a
paper on my repetition of Silvertooth's experiment, but I cannot publish all my papers
in one journal.

3. The INT. J. THEOR. PHYS. published few papers of me when its editor was Prof. Yates.
However when David Finkelstein took the power there, the doors of this journal (as well
as the doors of all journals which are in the hands of the relativistic lobby) remained
closed for me.

My papers in the seventies and early eighties were from the domain of space-time phy-
sics and I showed with many experiments which can easily be repeated that space and time
are the most simple and clear Newtonian categories and the theory of relativity is a con-
glomerate of nonsenses.

But after 1984 my papers are dedicated predominantly to the violation of the laws of
conservation, as I and some other investigators have <u>observed</u> violations of these laws.
This is an enormously important TOPICAL PHYSICAL ISSUE. The reports of my experiments
must be published in the most AUTHORITATIVE physical journals and the scientific commu-
nity must begin to discuss them.

My paper VERY EASY DEMONSTRATION... (Nr. 9718) is since almost TWO YEARS in your of-
fice and you still do not send me your final decision, although I have asked for this
final decision with many letters. I submitted my above rejected papers (Nr. 1175 and 1176)
PERSONALLY in the Bologna office of IL NUOVO CIMENTO and in the accompanying letter ad-
dressed to the Director of N.C. I begged him to investigate the problem with paper Nr.
9718 and to send me the final decision of the Editorial Board as soon as possible. Now
you reject the papers submitted on the 23 March 1990 but you still do not give me the
final decision on the paper 9718 submitted on the 11 July 1988.

In paper 9718 I give the report on an experiment where I demonstrate violation of the
law of angular momentum conservation. And this paper is blocked for two years by you. I
am asking WHY?

The two papers rejected now show that the displacement current is a fiction deprived
of ANY PHYSICAL REALITY. This aspect of the displacement current gives the explanation
why my Bul-Cub machine without stator (reported in paper 9718) rotates under the action
of INTERNAL FORCES, violating thus the angular momentum conservation law. The so-called
by you "well-settled" physical theories are full of ELEMENTARY LIES and experiments which
can be carried out by CHILDREN can reveal these lies. It turns out that the law of conser-
vation of energy is not valid. This has tremendous importance for the future of mankind. I
beg you to pay a due attention to my papers and to give them to authoritative referees.
We have already a perpetuum mobile which produces energy from nothing. I informed the
italian public about this machine in two journals: SEAGREEN, Bologna, autunno 1988 e
FRIGIDAIRE, Roma, **108**, novembre 1989. The topics of my papers are of such scientific im-
portance that they must be discussed in N.C. if your journal wishes to be a leading phy-
sical journal. Thus I beg you to give my papers 1175 and 1176 to a referee and to send
me your final decision on paper 9718 AS SOON AS POSSIBLE.

Sincerely yours, *Ⅎ. Marinov*   Stefan Marinov

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
12 May 1990

Prof. J. P. Wesley
Weiherdammstr. 24
D-7712 Blumberg

Copy: Prof. P. T. Pappas

Dear Paul,

Thank you very much for your letter of the 9 May. As it contains certain details wh
will be of no interest for the readers of TWT, I shall cite in my present answer only
this part of your letter which will be of interest for those readers.

In your letter of the 12 April you blamed me that "I am rejecting experimental resul
which do not happen to agree with my ideas". In my letter of the 30 April I wrote: "I s
be very thankful to you if in your answer you will notice such experiments." In your
letter of the 9 May you wrote:

> You ignore the force on Ampere's bridge! Since the Biot-Savart law
> gives mathematically any answer what-so-ever; your decision to choose
> the Ampere result does not mean the Biot-Savart law predicts the force
> on Ampere's bridge; it does not and cannot. If you really were not to
> ignore the Ampere bridge experiment, then you would be forced to accept
> the empirical law of Ampere. I know of no other law that gives the
> right answer. You ignore the forces on Pappas' Z-shaped antenna! Either
> you are ignoring the experiments or you are incapable of handling the
> math involved. Perhaps both?

Here are my comments.

Until now the force on the Π-form Ampere bridge was calculated NEITHER proceeding f
Grassmann's formula, NOR proceeding from Ampere's formula, as both formulas lead to si
larities which can be not went around. Instead of a Π-form bridge I analysed a U-form
bridge (a topsy-turvy U), hoping that there will be no singularities and taking into
account that the forces on a Π-form and U-form bridges must be equal (indeed, if they
would be not equal, then, making the one end of a rectangular loop to have a U-form, o
will be able to set this loop in motion by internal forces). As I showed (see TWT-VII,
p. 165), also for a U-form bridge, Grassmann's formula leads to singularities. This wa
a very strange result. Now Prof. Bartocci of the Perugia Institute of Mathematics has
calculated the improper integral (9) on p. 167 of TWT-VII and showed that it converges
to infinity. The calculation will be presented in TWT-VIII and we have to conclude tha
there is no way to calculate the pushing force on a U-form Ampere bridge made of an in
nitely thin wire.

It is, indeed, very strange fact that the pushing force on a U-form bridge comes out
to be infinitely big according to Grassmann's formula, as there are NO GEOMETRICAL sin
larities. Thus, it remains a strange conclusion that the force between two infinitely
near current elements along a CIRCULAR wire becomes infinitely big when the angle betwee
these two infinitely near current elements tends to zero, as according to Grassmann's
formula the force between collinear elements is zero. On the other hand I showed (see
the mentioned paper) that the force on a U-form bridge does NOT depend on the radius o
curvature. Thus the U-form bridge can be made with a very big radius where the angles
between infinitely near current elements will be definitely zero. This problem is torme
ting me a lot in the last time.

On the other hand, however, if the force on a U-form bridge will be finite, then we
shall come to a very unpleasant MATHEMATICAL contradiction: As the force on a Π-form
bridge is definitely converging to infinity and as the forces on a Π-form and U-form
bridges must be equal, then the force on a U-form bridge CANNOT be finite. You see, how
MATHEMATICAL PHYSICS with a SUPERB SUBTERFUGE falls always on its feet!

Thus, until the present day NOBODY has calculated the force pushing the Ampere bridg
(I repeat, NEITHER the proponents of Grassmann's formula, as me, NOR the proponents of
Ampere's formula, as you and Pappas). Consequently, the effects measured on the Ampere
bridge give no indication which formula is the right one and you have NO right to blame
me that I reject the Ampere bridge experiment, moreover taking into account that I was
the first man after Ampere who has CONSTRUCTED the rotating Ampere bridge and PROVED by
measurements ALL my theoretical predictions on that bridge (see TWT-IV, p. 136).

One must especially EMPHASIZE that the pushing force on the Ampere bridge does not depend on its form, on its size and on the thickness and the chemical substance of the wire, so that all authors (as Pappas etc.) who search for finding experimentally such dependencies are simply losing their time. I should like to note that here one comes to a highly strange PARADOX: It is obvious that the force on a Π-form bridge depends on the length of the shoulder (for a longer shoulder the force must be bigger). On the other hand the force on two U-form bridges with radius of curvature respectively equal to the half shoulders of two Π-form bridges is the same. Hence if taking two rectangular loops ending on the one side with U-forms, then for the case that the net force on the one bridge is zero, on the other will be different from zero. Which is the solution of this paradox? - The fact that ideal Π-form bridges can be not realized. Thus if you will take one U-form bridge and if you divide it at the top point incerting there some straight wire of an arbitrary length, making thus the bridge much wider, then I am sure that the pushing forces on these two bridges (which can be easily calculated in the form of improper integrals) will be THE SAME. I have not done these calculations, as I need my time for more important things and AS I AM SURE that the forces will be equal. If you can show by calculation that the pushing forces on these two bridges will be DIFFERENT, you will receive from me 100 DM.

Let us now come to the other experiment: Pappas' Z-shaped antenna. You cannot blame me that I ignore that experiment, as I was the editor who published Pappas' report (in TWT-IV, p. 158) and you, surely, have learned abouth this experiment from my publication.

It SEEMS that the null result of Pappas' experiment with the Z-shaped antenna rejects Grassmann's formula which predicts an effect. However, has Pappas taken into account ALL forces which act on his antenna? - No. He has IGNORED the CURRENT JET forces. As far as I know, I am the only author in the world who has introduced the current jet forces into physics. Until now I do not know about an EXPERIMENTAL confirmation of these forces (when the current streams in metal wires). I have proposed (see TWT-II, p. 98) a pretty simple experiment for separating these forces from the "Grassmann's" forces (i.e., from the magnetic forces between current elements) and for measuring the pushing force of the current jet forces, but until now NOBODY has carried out this experiment. This experiment (if done with precision) will give us a reliable number for the energy velocity of the current conducting electrons, which I FIRMLY expect to be of the order of c. Thus at the present moment the current jet forces of the current conducting electrons streaming in a wire are HYPOTHETICAL, however when the electrons leave the wire and continue to stream in vacuum, they manifest their current jet forces, as this has been many times experimentally established.

In the Z-shape antenna of Pappas (see the first figure 5 on p. 167 of TWT-IV), only the Grassmann forces are indicated which, as a matter of fact, MUST set the antenna in rotation. However, the current jet forces are not indicated. A rotating moment about the axis of rotation, besides of the current jet forces at the angles, will be generated by the current jet forces of the electrons streaming to the end points of the antenna and from the end points of the antenna. The reaction forces of these electrons when they are stopped and then accelerated in the opposite direction generate a rotational moment EXACTLY OPPOSITE to the "Grassmann" moment. The law of angular momentum conservation will require that the moments of rotation of these two kinds of forces must be equal and oppositely directed. You might object that, according to me, this law can be violated and that I have experimentally demonstrated such violations. Yes, but in the majority of cases the law is preserved. Pappas experiment shows that here the law is preserved.

Now I shall reveal to you a certain detail of Pappas' experiment which Pappas has not communicated to the reader but only to me. When the experiment was done first, Pappas HAS observed a clear and definite rotation in the same direction as predicted by Grassmann's formula. He noticed, however, that there was a stream of electrons out of the antenna's ends (there was a slight noise). Pappas covered the ends by scotch, the noise disappeared and the rotating moment, too. This aspect of Pappas experiment (which is WELL KNOWN) shows that the current jet forces are enough big. We need further detailed and precise experiments for separating the current jet forces from the magnetic forces and only then we can make a verdict on Pappas Z-shaped antenna. I will be happy to hear your comments and to print them (along with another your interesting paper) in TWT-VIII which will appear somewhere in July.

Yours: *Stefan*

Editorial note. Answered on the 15 May 1990.

PHYSICS LETTERS A

14/5/90

Dear Dr Marinov,

I regret that your work is not suitable for publication in PLA and I return your ms herewith.

Yours sincerely,

JPV

TRANSCRIPTION:

Dear Dr. Marinov,

I regret that your work is not suitable for publication in PLA and I return your ms herewith.

Yours sincerely, JPV

Editorial note. With the above letter Prof. Vigier answers Marinov's letter of the 7 May 1990. Marinov's answer to the above letter is given in Marinov' letter of the 1 June 1990.

The rejected paper entitled "CHILDISHLY SIMPLE EXPERIMENT VIOLATING THE PRINCIPLE OF RELATIVITY" is published in TWT-VII, p. 325.

J. P. WESLEY, *Ph.D. Physicist*

Weiherdammstr. 24, *7712 Blumberg, West Germany,* Tel: 07702-658

15 May 1990

Dear Stefan,

Thanks for your letter of 12 May 1990.

I see that your problem is not <u>ignoring the experiments</u> but is an incapablility of handling the math involved.

You claim the Ampere law and Grassmann law "lead to singularities that cannot be avoided". This is NOT TRUE. As every physics student is taught in the second year of university physics, if one <u>assumes</u> singular sources, i.e., sources that can go to infinity, then one predicts infinite forces. Since infinite forces are never found in the laboratory, obviously singular sources, i.e., infinite sources cannot, in fact, exist. One <u>never ever</u> in any experiment actually has a "point" charge; thus, no one <u>ever</u> reports an infinite force due to Coulomb's law. Coulomb's law is expressed in terms of <u>point</u> charges; but this does not mean that one can find <u>point</u> charges in nature! The usual expression for Coulomb's law is merely a mathematical artifact. For any actual experiment one must sum over a continuous distribution of charges. Even for experiments regarding the electron itself, its charge is always assumed to be some continuous distribution over space. The Ampere law and Grassmann law, as usually expressed in terms of line current elements, involve singular sources, or infinite sources. No one assumes these abbreviated expressions can be used as is in the laboratory (anymore than Coulomb's law can be used as is in the laboratory). With their singular infinite sources they predict, of course, infinite forces (at least in Ampere's law). Since no infinite forces are ever observed; one must either interpret the laws (as clearly intended) or must explicitly present them as having <u>no inifinite nor singular sources.</u> Since you seem to need the <u>actual</u> Ampere and Biot-Savart laws which can be checked in the laboratory (NO singularities or infinite forces are implied no matter what the abbreviated notation might be!); here they are:

Ampere:
$$d^6F/d^3r\,d^3r' = (R/R^3)\left[-2J{\cdot}J' + 3(J{\cdot}R)(J'{\cdot}R)/R^2 \right],$$

CORRECT!

Biot-Savart:
$$d^6F/d^3r\,d^3r' = J \times (J' \times R)/R^3$$

These are the full expressions for the usual abbreviated forms which are not physically tenable as is, namely:

Ampere:
$$d^2F = II'\left[- 2ds{\cdot}ds' + 3(ds{\cdot}R)(ds'{\cdot}R)/R^2\right]R/R^3$$

Biot-Savart:
$$d^2F = II'ds \times (ds' \times R)/R^3$$

WRONG!

where $R = r - r'$ is the distance between an element at r and a primed element at r'. Similarly the Coulomb's law that is empirically correct and which can be checked in the laboratory is

$$d^6\mathbf{F}/d^3r\,d^3r' = \rho\rho'\mathbf{R}/R^3,$$ **CORRECT!**

not

$$\mathbf{F} = qq'\mathbf{R}/R^3.$$ **WRONG!**

    Contrary to your claim, I calculated the force on Ampere's bridge using the Ampere formula and the Biot-Savart formula for wires of finite rectangular cross section. You can find the explicit expressions in PROGRESS IN SPACE TIME PHYSICS 1987. There I show the Biot-Savart law to be completely ambiguous (having nothing to do with your artificial singularity due to using incorrect linear current elements).

    You are also wrong in claiming Pappas fails to mention the corona discharge effect on his antennas; he does mention it. Moreover, you are wrong in claiming the intimate details of the contact of the ends of the bridge are irrelevant. Everyone .. reports the difficulties! You are ignoring experimental facts again.

    But enough! You force me to discuss things on/too elementary and primitive level. I thought I left it all behind me when I no longer had to explain things to my graduate students who I felt were too uneducated. Now you demand that I reduce myself to the undergraduate level! Sigh!

    I hope to see you in Milano.

Tschüß

Paul

Copy to Pappas

P.S. Enclosed is a reprint of a model for an electron that is rather cute. Such things I do not take too seriously; but at least it is infinitely better than the quantum electrodynamics nonsense for the anomalous magnetic moment of the electron!

P.P.S. I will keep TWT in mind if I ever have anything appropropriate to contribute.

Zentralkomitee der
Kommunistischen Partei Österreichs

Gen.

Stefan Marinov

Morellenfeldgasse 16

8o1o   Graz

Wien - 199o - o5 - 21

Lieber Genosse Marinov !

        Wir danken Dir für Deinen Brief und müssen Dir
leider mitteilen, dass auf Grund der Länge Dein Bei-
trag in der "Volksstimme" nicht veröffentlicht werden
kann (Leserbriefe sollen nicht länger als eine halbe
Maschinschreibseite sein).

        Wir ersuchen Dich daher, Deine Gedanken kurz zu-
sammenzufassen und der Redaktion der "Volksstimme"
als Leserbrief zu schicken. Weiters möchten wir Dir
mitteilen, dass wir Deine Überlegungen gelesen haben
und einige Gedanken sehr interessant finden. Wir ver-
sichern Dir, dass Deine Anregungen in unseren Diskus-
sionen ihren Niederschlag finden werden.

        Mit freundlichen Grüssen !

                    Org.-Referat

                    Veronika Stöckl-Holzknecht

Editorial note.

This is the answer to Marinov's letter of the 1 May 1990.
See the letter of the journal DER STANDARD of the 26 June 1990
and the pages following this letter.

Budapest, 24th May 1990.

Dr Stefan Marinov
Morellenfeldgasse 16
A-8010 Graz, Austria

Re: Your paper: "New measurement of the Earth's absolute
velocity with the help of the "Coupled Shutters"
experiment"

Dear Dr Marinov,

According to our Referees' opinion, we regret
not to be able to accept your paper for publication
in Acta Physica.

Yours sincerely

Editorial note. This letter is answered
by Marinov with his
letter of the 6.VI.90.

Prof. I. Kovács
Editor

Referee's Report on the paper
"New measurement of the Earth's absolute velocity..."

The paper is not suitable for publication because the result
reported is not original. The person who originally detected
the absolute motion of the Earth is German Karginov. He
carried out his experiment with a slightly different apparatus,
and using the sunlight. However, the outcome of both experiments
is remarkably similar, and Karginov was unable to publish his
results.
If the author wishes to learn about the discovery, I suggest
that he may contact Karginov, who used to live in Budapest. I
do not know his present whereabouts, but it may be of help to
mention that he is the author of the book "Rodchenko" which
has been translated also to Hungarian and was published in 1975
by Corvina.
I should like to add that the present form of the manuscript
is unsuitable for publication, independently of the validity
of the results, because it contains a large number of side-
remarks which are unnecessary for communicating the results.
However, the paper is written in a quite enjoyable style,
and it could be published either as a personal edition or
perhaps in a literary magazine.

Ref: LMR/CW

31 May 1990

Dr S Marinov
Morelienfeldgasse 16
A-8010 Graz
AUSTRIA

Dear Dr Marinov

Your papers were discussed at a meeting of the Editorial Board where it was agreed that they were unsuitable for publication in Classical and Quantum Gravity.

I enclose the typescripts together with the Board Report.

Yours sincerely

Editorial note. This letter was answered by Marinov with his letter of the 7.VI.90.

Linda M Richardson
Managing Editor
Classical and Quantum Gravity

'Propulsive and rotating ampere bridges...' and 'The action of constant electric current...' by Stefan Marinov.

These articles are concerned with experimental work in the field of elementary electromagnetic theory. As such, they are not appropriate for the Journal of Classical and Quantum Gravity, which deals with neither Special Relativity/electromagnetism nor with practical experimental work. (The journal does cover experimental tests of General Relativity but only from the point of view of theoretical analysis.)

Clearly, if the ideas proposed proved to be correct, the ramifications for General Relativity would be profound, and these ramifications might reasonably be discussed in this journal. However, both Special Relativity and General Relativity have been experimentally tested to extreme accuracy with numerous independent tests and it is therefore up to the author either to show that his theory also satisfies these tests or to show why they are wrong.

I therefore advise that the articles should be rejected.

BOARD REPORT

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

1 June 1990

Prof. J. P. Vigier
PHYSICS LETTERS A
Institut H. Poincare
11 Rue P. et M. Curie
F-75231 Paris Cedex 05

Dear Prof. Vigier,

Thank you for your letter of the 14 May with which you rejected my paper V 1465a entitled CHILDISHLY SIMPLE EXPERIMENT VIOLATING THE PRINCIPLE OF RELATIVITY.

I am amazed, however, to see that you rejected my paper without giving MOTIVATIONS fo the rejection. In the last years you rejected my papers by sending me comments of your referees where there were coarse MATHEMATICAL errors and I begged you many times to sear for the advices of people who had good notes in mathematics when being students. Now, as I see, you could neither find some mathematical invalid to help you in writing a negative comments.

Meanwhile my paper rejected without presenting motivations is of an extreme scientifi importance, as it gives the theory and the report on a very easy experiment which demonstrates drastic violation of the principle of relativity.

As you were unable to present negative comments justifying at least FORMALLY the rejection of my paper, I pose you the following two questions which I insist that you answer by "yes", "no" or "I do not know":

Yes No I do not k

1. Will the rotational inverse Rowland experiment give an effect
   opposite to the effect given by the rotational direct Rowland
   experiment?

2. Will the inertial inverse Rowland experiment give an effect
   opposite to the effect given by the inertial direct Rowland
   experiment?

If you will not answer these two questions, you will demonstrate to the world on whic THEORETICAL scientific level was my paper rejected.

If you will answer the first question by "yes", you will demonstrate that you have re jected the report on an experiment which contradicts your scientific prediction. Such an act can be categorized only as obscurantism.

Now my paper ABSOLUTE AND RELATIVE NEWTON-LORENTZ EQUATIONS (V 791a) which was rejected on the ground of the IDIOTIC comments of your referee sent to me with your letter of the 12 September 1989, where there were blunt mathematical errors (see my comments sent to you with my letter of the 9 October 1989) will appear in PHYSICS ESSAYS. I send you the proofs of the paper and I invite your referee or you to appear with a criticism on this paper in the press. If in six months from now such a paper will appear in PHYSICS LETTERS A, I shall pay you $ 1000. The money can be prepaid. If the paper will not appear, you have only to send the money after six months back to me.

I know, dear Prof. Vigier, that you will NOT dare to appear in the press with a criticism on my paper ABSOLUTE AND RELATIVE... And I am afraid that you will not dare to answer the two questions posed in my present letter. Et je m'adresse à l'ombre de celui, qui a chanté gloire à la vaillance: "Sont-ils morts au jour d'hui les braves valets de Gascogne... et de la France?" .

I am submitting again my rejected paper V 1465a and I am looking with curiosity for your prompt answer.

With sincerity: *S. Marinov*

Stefan Marinov

Editorial note. With a letter of the 13 August 1990, Prof. Vigier rejected paper V 1465a entitled "Childishly simple experiment... (published in TWT-VII, p.325) by using the following NINE words:

I CONFIRM THAT PAPER V 1465a IS DEFINITELY REJECTED.

STEFAN MARINOV
Morellenfeldgasse 16   5 June 1990
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

Prof. B. Pippard
EUROPEAN JOURNAL OF PHYSICS
Cavendish Laboratory
Cambridge CB3 OHE
England

Dear Prof. Pippard,

Thank you very much for your letter of the 11 May 1990 which I answer after a 20-days rip to Switzerland and Italy.

If some of my correspondents notes that his letter to me is not to be published, I o not do this. I have many such letters which throw abundant light on some delicate spects of the scientific background. Although being very interesting, these letters ill never reach the public. Your letter is not of this kind, as you treat in it ome purely scientific problems, but, as you begged me not to publish it, of course it ill remain only in my archives.

I shall shortly answer your remarks concerning the   paper submitted by me.

THE DEMONSTRATIONAL MOLLER-MARINOV MACHINE. You write:

I must point out that your remark, at the bottom of page 3, concerning the apparent violation of Newton's third law, is wide of mark. I am not aware that anyone tries to conceal this from students, but it is not significant violation since two _complete_ (your italics - S.M.) circuits influence one another with equal action and reaction. Thus there are no observable consequences of this odd phenomenon, and Newton cannot be held responsible for omitting to comment on an unobservable effect.

According to my statistics, of 77 only 1 textbook on electromagnetism for universities nd colleges published after WWII mentions this "odd" violation. Meanwhile any textbook ublished before WWI, where Grassmann's formula is introduced, mentions it. I qualify his "odd" fact only as concealment. In the last five years some scientists, as my friends appas, Graneau, Wesley, Assis, etc., who are honest with themselves, seeing that Grass-ann's formula (i.e., the fundamental electromagnetic LORENTZ EQUATION) is contradicting ewton's third law, try to save this "sacred" law rejecting Grassmann's formula and em-racing the older Ampere's formula which preserves Newton's third law. All other scien-ists make as if Grassmann's formula does not exist, although it is an immediate conse-uence of the Lorentz equation which can be not left without attention, as we do ALL our alculations in electromagnetism proceeding from this equation (as I showed - see my CLAS-ICAL PHYSICS - Maxwell equations are result of the Lorentz equation).

The motivation to not take into account the violation of Newton's third law by the orentz equation, which all scientists sustain, is presented in your letter: "As at the nterection of two complete circuits action and reaction are equal, why to bother about he violation bewteen NON-COMPLETE circuits. In nature all circuits are complete!" The ast assertion which was not given in your letter (but which you CERTAINLY also sustain) s, however, NOT TRUE. Any circuit with a condenser is not complete. You will certainly bject: "It is made complete through the displacement current 'flowing' between the con-enser's plates." Sorry, Prof. Pippard, but this is another LIE which all scientists re-eat more than a century (although Maxwell introduced it as an EXPERIMENTALLY NON CON-IRMED HYPOTHESIS). So many experiments (see TWT-VII) have shown that displacement cur-ent (in vacuum and in dielectrics) is NOT current, as it neither acts with potential agnetic forces on other currents nor reacts with kinetic forces to the potential action f other currents. And by realizing interaction between UNCOMPLETE circuits, I violated ewton's third law in the MOST DIRECT WAY with my "Bul-Cub machine without stator" and Rotating Ampere Bridge with Displacement Current" (see TWT-III and TWT-IV).

All my efforts to publish the reports on my experiments violating Newton's third law n electromagnetism failed. I consider this attitude of the editors of the scientific ournals as CONCEALMENT OF THE TRUTH.

Concerning your remark that Newton is not to be blamed for the violation of his third aw, I can say only the following: At the time of Newton there was no electromagnetism nd he, indeed, cannot be accused of having overseen this violation. But the scientists f the XIXth and XXth century ARE TO BE BLAMED for sustaining that this law has a GENE-RAL VALIDITY.

Concerning the Müller-Marinov machine, you write:

> Turning now to your machine, I do not understand why you think it adds anything to any argument concerning relativity... May I stress that I do not discuss your ideas - I simply cannot tell from your presentation where the conflict lies between your views and those of others.

I assert (and I <u>demonstrate</u> this assertion with my machine) that when rotating the metal sticks keeping the magnetic belt at rest, an electric intensity is induced in the sticks according to the formula

$$E_{mot} = (v/c) \times rot\mathbf{A}, \qquad (1)$$

where $\mathbf{v}$ is the velocity of the sticks and $\mathbf{A}$ is the magnetic potential originated by the magnetic belt at the space domain where the sticks are located.

However, when the sticks are at rest and the belt rotates, there is NO induced tension as in this case the induced electric intensity is to be calculated according to the formula

$$E_{mot-tr} = (1/c)(v.grad)\mathbf{A}, \qquad (2)$$

where $\mathbf{v}$ is the velocity of the magnetic belt, and, as formula (7) in my papee shows, for my machine $E_{mot-tr} = 0$.

These two completely different effects and the competely different formulas (1) and (2) <u>contradict the principle of relativity</u>. For the second case relativity writes the WRONG formula

$$E = - (v/c) \times rot\mathbf{A}. \qquad (3)$$

I beg you that in your answer you state which, according to you, is the right formula for the second case: my formula (2) or the relativistic formula (3)? If you will not give a clear answer by writing "formula (2)" or "formula (3)", you will give a demonstration of a concealment of the scientific truth.

And I beg you not to try to juggle with the subterfuge of inertial and rotational motion. You can take the magnetic belt in the form of RECTILINEAR "conveyor" belt where the motion of sticks or belt will be PERFECTLY INERTIAL. And another remark: The effect of presence of induced tension along the sticks will be detected by attaching golden leaflets at the ends of the sticks (as one does in the electrometers). I have used this method in my inductive INERTIAL KENNARD EXPERIMENT (see TWT-IV) with whose help for a FOURTH time in the last 15 years I have measured the Earth's absolute velocity.

I submit again my rejected paper, hoping that YOU will publish it.

Enclosed are the covers of TWT-III, TWT-IV and TWT-VII which you can receive by sending the indicated prices.

Hoping to receive your answer soon,

<div align="right">
Sincerely yours,
<i>S. Marinov</i>
Stefan Marinov
</div>

<u>Editorial note</u>. This letter remained without answer.

<u>Marinov's note</u>. In his letter of the 11 May 1990 Prof. Pippard begged me especially to NOT publish it.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

6 June 1990

Prof. I. Kovacs
ACTA PHYSICA HUNGARICA
Department of Atomic Physics
Budafoki ut 8
H-1521 Budapest
Your letter of 24 May 1990.

Dear Prof. Kovacs,

Thank you very much for the quick decision taken on my paper "New measurement of the Earth's absolute...", although, I must confess, its rejection, of course, was not pleasant for me. There are already six years that I cannot find a scientific vehicle to publish the report on my "coupled shutters" experiment which has shown, CONTRARY TO THE PREDICTIONS OF TODAY'S PHYSICS, that the velocity of light in the laboratory of the moving Earth is direction dependent and so I succeeded   to measure the Earth's absolute velocity.

I was however unpleasantly shocked when reading the motivations with which the referee has rejected publication of my paper:

> The paper is not suitable for publication because the result reported is not original. The person who originally detected the absolute motion of the Earth is German Karginov. He carried out his experiment with a slightly different apparatus, and using the sunlight. However, the outcome of both experiments is remarkably similar, and Karginov was unable to publish his results.

First, if the report on an experiment has not been published, such an experiment DOES NOT EXIST. Second, if the experiment of Karginov and my experiment have given "REMARKABLY SIMILAR OUTCOMES", the referee and you, as Editor of ACTA PHYSICA HUNGARICA, are OBLIGED, if you both   consider yourselves as adepts in the temple of science, to publish the reports of Karginov and me, and to save finally the world from the idiotic dogma proposed by Einstein that velocity of light in a moving laboratory is NOT direction dependent. Two men, Karginov and Marinov, do independently one of another two similar experiments and obtain for the Earth's absolute velocity (which according to official science can NEVER be measured) "REMARKABLY SIMILAR RESULTS", and you reject to publish the reports of these two men. Is this not SUPPRESSION of unpleasant scientific experimental evidence, because this evidence contradicts well-established scientific THEORIES? Where we are: in the XXth century or in the time of Galileo?

I beg you to answer this question and I resubmit my paper, hoping that you will publish it and that you will do YOUR BEST to find and publish also Karginov's report.

On the other hand, I am extremely thankful to the referee that he informed me about German Karginov and about his experiment, as I have never heard about Karginov, neither Janossy told me about him (or may be he has done his experiment after the death of Janossy?). --- I looked today in the telephone directory of Budapest but the name of Karginov is not there, and I think that my letter to the Editorial House "Corvina" (whose address I do not know) will remain without answer (the year 1975 is 15 years behind).

Thus I am addressing the referee with the following plea: If he will find for me the address of Karginov, or if he will send me the report on his experiment, I shall send him $ 50. If this money is not enough, I shall send $ 100, or even $ 200.

You surely know that I am one of the best world experts in "perpetuum mobile" problems. You cannot imagine how many perpetua mobilia have existed and EXIST now in the world (I send you the cover of the sequence TWT, volume V, dedicated to the Swiss perpetuum mobile TESTATIKA). But because detailed descriptions of these machines have not been published (for different reasons, not only for the stubbornness of the editors), many of the machines have "died" and nobody will be able to find their secrets and resurrect them. I will do my best to not leave TESTATIKA to "die".

Thus I am anxious about Karginov and I do not wish that his experiment also "dies". Help me to save the experiment of Karginov for future generations!

I submit also another paper where the description of a CHILDISHLY SIMPLE experiment for measuring the Earth's absolute velocity is described. Its title is:

ACTION OF CONSTANT ELECTRIC CURRENT ON ELECTRONS AT REST DUE TO THE ABSOLUTE VELOCITY...

Enclosed is my article "Absolute and Relative Newton-Lorentz Equations" where a more detailed account on the theory of this experiment is presented (the paper will appear in PHYSICS ESSAYS). If you appreciate the reports on my experiments, publish them, if not, reject them, but do not give me the advice to bring them to a literary magazine.

t. note. Answered on the 4.X.90.   Sincerely yours, *0. Marinov* S. Marinov

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

6 June 1990

J. P. Wesley

Copy: P.T. Pappas
U. Bartocci

Dear Paul,

After reading once more your letter of the 15 May and your paper on p. 170 of SPACE-TIME PHYSICS 1987, I decided not to write a letter intended for publication in TWT-VIII as I wish to clear some problems and I hope that you will help me (in the shortest possible time).

I agree with you that the calculation for the case when one searches for the forces with which current elements of the SAME circuit act on each other is to be done taking the current densities and not the current elements as infinitely thin filaments. But for the case of my U-form Ampere bridge where there are NO geometrical singularities and where ACCORDING TO GRASSMANN FORMULA, the forces with which current elements lined up in row are ZERO, I EXPECTED that the calculation with infinitely thin current elements will lead to a final integral. Unfortunately this was not the case. Now Prof. Bartocci made DETAILED calculations of the integral (9) on p. 167 of TWT-VII and he showed that it is converging to infinity. I shall present Bartocci's calculations in TWT-VIII.

At this stage of my research on the U-form Ampere bridge FROM THE VIEW-POINT OF GRASSMANN FORMULA, I am addressing you for help. Can you calculate FOR ME the pushing force on the U-form Ampere bridge, BY USING GRASSMANN FORMULA and YOUR method with the current densities. You write in your letter that you have done such calculations for the Π-form Ampere bridge not only according to Ampere formula but ALSO according to Grassmann formula (see the first line in your letter, p. 2), but in SPACE-TIME PHYSICS 1987 I found only the calculations with the Ampere formula. Can you supply me with the calculations with the GRASSMANN FORMULA? And can you do the calculations with the U-form bridge, taking into account the preliminary calculations which I did in TWT-VII, thus, practically transforming the result in the integral (9) on p. 167 of TWT-VII to volume integrations. I shall be very thankful to you for this help and especially if you will help me in shortest time, as I wish to prepare the paper for TWT-VIII.

Indeed, I claimed that nobody has calculated the force on the Ampere bridge. The reason is that I did not take seriously your calculations in S.-T. PH. 87. And the reason for this was that when reading the paper after receiving the book, I found blunt (according to me) mathematical errors. Here are my problems with your calculations:

The term in the brackets in formula (4) on p. 174 is written by you in the form

$$- 2Y/r^3 + 3Y^3/r^5,$$

meanwhile, as J_1 and J_2 are parallel to r, I obtain this term in the form

$$- 2Y/r^3 + 3Yr^2/r^5 = Y/r^3.$$

Please, note where am I wrong.

Then in formula (12) on p. 175 I obtain the third term on the right side not $- (2/3)$ but $+ (2/3)\ln2$. Please, note whether you or I did the right calculation of the sum of F' and F". Then I beg you to write me in more detail the calculation of the integral (9) for obtaining the integral (10).

Then I have some objections of principle: I affirm that the force on the Ampere bridge cannot depend on the FORM of the bridge, on the THICKNESS of the wire, shortly it must be THE SAME for ALL bridges (small or big) and must depend ONLY ON THE CURRENT. Thus the force for 1 A must be a PRECISE NUMBER FOR ALL Ampere bridges. Indeed, if this is not true, then making a circuit with two Ampere bridges at their ends, one will be able to set the circuit in motion in this direction in which the force on the respective bridge is bigger. (I beg you now, FOR A WHILE, to suppose that the right formula is the Grassmann formula!!!) Thus your formula (15) where I see dependence on the thickness must be wrong in principle. (You may object that you calculate according to Ampere and for this reason you obtain such a dependence, etc.). The fact that Pappas has observed a dependence on d is not an argument, as I know how imprecise are the measurements on the Ampere bridge.

My FIRM intuitive opinion is that the force on ANY Ampere bridge (with infinitely lo

legs) must be $\mu_0 = 4\pi\times10^{-7}$ N/A^2 or some multiple (2, or 4) of μ_0. Thus a PRECISE calculation MUST lead to such a UNIVERSAL CONSTANT. This will be a very interesting PHYSICAL result, as μ_0 will obtain a firm and solid physical ground (as, for example, that c is $(\varepsilon_0\mu_0)^{-1/2}$. I do not leave my efforts to calculate PRECISELY the pushing force on the U-form Ampere bridge and I beg you to help me with your method with the current densities.

And, please, answer, whether, according to you, the pushing force on the Ampere bridge, CALCULATED WITH THE HELP OF AMPERE FORMULA, will lead to a dependence on the FORM, SIZE and WIRE THICKNESS of the bridge. I am FIRMLY PERSUADED that for Grassmann formula such dependences CANNOT EXIST.

Then I beg you for the following: Take in your fig. 1 on p. 173 or SP.-T. PHYS. 87 the lengths N and M equal to infinity. And calculate the force on the Ampere bridge, taking into account only the forces with which wire 2 acts on wire 5 and wire 10 acts on wire 7, ACCORDING TO AMPERE. You have done such calculations obtaining formulas (8) and (10), but there you have such expressions as, ln2N which from a mathematical point of view are nonsensical, as N has dimensions, and if I shall put N = ∞, I shall obtain an infinity. The force on this INFINITELY LONG Ampere bridge MUST BE a simple figure. If you cannot obtain such a simple figure (calculating according to Ampere formula), this signifies that, surely, your calculations were wrong, or that Ampere formula is wrong. Thus I beg you once more, send me the calculated by you value, if you can make for such a case a PRECISE calculation according to your method.

And I have another question: If in your integral (9) on p. 174 I shall put R = 0, then the integral becomes improper and, surely, will be equal to infinity. Explain to me WHY you save your integral from infinity and I cannot save it for the case of my integral (9) on p. 167 of TWT-VII. I speak here, meaning only the MATHEMATICAL aspects of the problem. You have an integral with infinitely big integrand and I have an integral with infinitely big integrand, but you obtain a finite number for the integral, and I cannot. WHY?

Now to some other topics in your letter. I said in my letter of the 12 May that Pappas has NOT reported in his paper published in TWT-IV that when the end points of his Z-shape antenna were NOT covered with scotch, then there WAS rotation, as prescribed by Grassmann formula. You wrote in your letter of the 15 May:

> You are also wrong in claiming Pappas fails to mention the corona discharge effect on his antennas; he does mention it.

Please, note the page and the line where Pappas mentions the corona discharge!

Then I cannot UNDERSTAND what you mean with the following phrase:

> ... you are wrong in claiming the intimate details of the contact of the ends of the bridge are irrelevent. Everyone reports the difficulties!

As I see from the end of your letter, you are a little bit boring to discuss with me things on a too elementary and primitive level. Unfortunately, I can discuss things ONLY on a primitive and elementary level. I lose myself in a complicated discussion and CANNOT rationalize. And my WHOLE theory and ALL MY EXPERIMENTS are done on an UNDERGRADUATE LEVEL. As I consider THIS my statement AS VERY IMPORTANT, I decided to publish the above letter in TWT-VIII. Thus, also your valuable answer will be published. Hoping to receive this answer soon,   Yours, *Stefan*

Marinov's note. In chapter 6 of his future book which Prof. Wesley kindly sent to me on the 30.X.90, the force F acting on the Ampere bridge, calculated by Prof. Wesley with volume integrals, is given in the form (see fig. 1 on p. 273 of Wesley's SPACE-TIME PHYSICS 1987), for w small,

$$c^2F/2I^2 = -1 + (1 + L^2/M^2)^{1/2} - \ln\{1 + (1 + L^2/M^2)^{1/2}\} + \ln\{1 + (1 + L^2/(M-N)^2)^{1/2}\}.$$

For $L/M \to 0$, $L/(M-N) \to 0$, one obtains the ABSURD result F = 0. In my letter of the 7.IX.90 to Prof. Wesley I noted that such an ABSURD result (force on the Ampere bridge equal to zero when its legs will be much longer than its shoulder) is a SUFFICIENT INDICATION that Wesley's calculations MUST BE WRONG.

Prof. Wesley answers the above letter with his letter of the 14 June 1990.

FYZIKÁLNY ÚSTAV

Centra elektro-fyzikálneho výskumu Slovenskej akadémie vied

Dúbravská cesta 9, 842 28 Bratislava

⌐ Dr.S. M a r i n o v   ⌐
Morellenfeldgasse 16
A – 8010 GRAZ
Austria   ⌐

Naša značka   Bratislava 6 June 1990
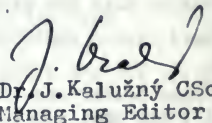
Dear Dr. Marinov,

                                        16th
       Thank you very much for your letter of March 12th, 1990.
As you wished we sent your paper "Physical essence of the
Maxwell-Lorentz equations" to the arbitrator. Enclosed please
find his point of view to your paper. Editorial Board of
the journal Acta Physica Slovaca confirms its first decision.
The paper "Physical essence of the Maxwell-Lorentz equations"
will not be published in the journal Acta Physica Slovaca.

       Thank you very much for sending your manuscript to
our office.

       With best wishes

                                    RNDr.J.Kalužný CSc.,
                                    Managing Editor

       The second referee report of the paper:

       Physical essence of the Maxwell- Lorentz equations by
       S.Marinov

       I have found the first referee report of that paper
completely correct. The paper by Marinov contains several
errors or approximations.
Eq.(4) represents electromagnetic potentials in the quasistationary
approximations. These potentials do not satisfy the relations (10),
for example.

       The paper by Marinov is incorrect and by this fact Icannot
recommend it for the publication in Acta Physica Slovaca.

STEFAN MARINOV
Morellenfeldgasse 16   7 June 1990
A-8010 GRAZ — AUSTRIA

Dr. Jan Kaluzny
ACTA PHYSICA SLOVACA
Dubravska cesta 9
CSFR-842 28 Bratislava

Dear Dr. Kaluzny,

Thank you very much for your letter of the 6 June, although I was unpleasantly shocked to read such a superficial opinion of the arbitrator on my paper PHYSICAL ESSENCE OF THE MAXWELL-LORENTZ EQUATIONS. I close the problem with the submission of this paper but I wish to give expression by the present letter of my disaccord   with the opinion of the arbitrator. The potentials (4) in my paper SATISFY the equations (10). I show THIS on p. 45 of my paper MAXWELL'S ILLUSION: THE DISPLACEMENT CURRENT which is now on examination in your journal. I have the feeling that you have not handed to the referee also the other three papers which are now under examination in your journal. It is a pity because in these papers I describe in detail my experiments with which I show that Maxwell's displacement current is NO current, that circuits with condensers are NON-CLOSED circuits, and with such circuits I demonstrated violation of the angular momentum conservation law. The problems (better to say the SOLUTIONS) which I bring to the attention of the scientific community are of a too great importance and it is highly inappropriate to reject my papers with such a kind of motivation as that presented in the report of your arbitrator.

To show to your arbitrator the extent of my research, I send him (and I grant to him) the fifth volume of my encyclopaedic work CLASSICAL PHYSICS. I give to him the advice to read Chapter V and to see that the potentials with which one must calculate ALL electromagnetic effects are the potentials AT THE MOMENT OF OBSERVATION and not, as it is commonly WRONGLY accepted, the potentials at the RETARDED moment (which, as a matter of fact must be called ADVANCED moment, as I do in my theory). The big error of conventional physics is that it takes the magnetic potential in the form $A = qv'/r'(c - n'.v')$, where all primed quantities are taken at the moment $t' = t - r'/c$, called by me advanced, as it is previous to the moment of observation t. Meanwhile the right formula is $A = qv/cr = q(v' + r'u'/c)/r'(c - n'.v')$. Only working with MY form one obtains the Lorentz frictional forces (the radiation reaction "field") by AUTOMATICAL calculation and one has not to make artificial calculation leading to the so-called "self-acceleration", as one does in the conventional theories.

Hoping to receive your decision on my other three papers submitted on the 16 March in a due time,

Sincerely yours, *J. Marinov* Stefan Marinov

Editorial note. Dr. Kaluzny answers this letter with his letter of the 29 June 1990.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

7 June 1990

Ms. Linda Richardson
JOURNAL OF PHYSICS A
IOP Publishing Ltd
Redcliffe Way
Bristol BS1 6NX

Dear Ms. Richardson,

Thank you for your letter of the 31 May which finally, after so many letters and phone calls from myside, reached me.

I am, however, AMAZED that the papers which I have submitted to the JOURNAL OF PHYSIC A have landed in the scholastic and Talmudic JOURNAL OF CLASSICAL AND QUANTUM GRAVITY (who, my God, has observed quantum gravitational effects!). I have submitted my papers to the J O U R N A L O F P H Y S I CS A! Enclosed are the letters of submission.

I asked you SO MANY TIMES to acknowledge the reception of my papers, but you did not acknowledge their reception. And now I receive referee's opinion from the JOURNAL OF CLASSICAL AND QUANTUM GRAVITY. Why is the whole this play with me? If the JOURNAL OF PHYSICS does not wish to have contacts with me, it has to state this clearly in a letter, as already PHYSICAL REVIEW, EUROPHYSICS LETTERS, some of the editors of the JOURNAL OF MODERN PHYSICS and some of the editors of PHYSICS LETTERS have done. But if such a letter will not reached me, I beg you to handle my correspondence in the usual way and papers submitted to the J. of PHYS. to not catapult to other journals.

Thus I send you AGAIN both papers rejected by Classical and Quantum Gravity and I beg you to send them to a referee of the JOURNAL OF PHYSICS. These two papers are sent now in single copies:

1. PRPOPULSIVE AND ROTATING AMPERE BRIDGES VIOLATE THE PRINCIPLE OF RELATIVITY.

2. ACTION OF CONSTANT ELECTRIC CURRENT ON ELECTRONS AT REST DUE TO THE ABSOLUTE VELOCITY OF THE EARTH.

The PACS numbers are 03.30, 03.50, 41.10.

Herewith I transfer the copyright for these papers to the JOURNAL OF PHYSICS.

All eventual charges will be paid by myself.

Since the date of submission of my second paper many things have CHANGED in electro magnetism. Prof. W. Rindler in AM. J. PHYS., 57, 993 (1989) has recognized that for the case of moving magnet NOT the relativistic formula

$$E = - v \times rotA,$$

but MY formula

$$E_{mot-tr} = (v.grad)A$$

is the right one. And this aspect of electromagnetism allowed me to measure the Earth's absolute velocity with my inertial Kennard experiment reported in the second paper. I enclose also my paper "Absolute and Relative Newton-Lorentz Equations" which gives the detailed theoretical ground of my second paper and I beg you to hand this paper to the referee, as it will appear after a couple of months in PHYSICS ESSAYS.

With my letter of the 30 March 1989 I presented my objections to my paper "Violatior the referee's comments on of the laws..." (Q/13004/P) but until now I do not know whether these objections are considered and whether there will be a new decision on this paper. Please, inform me which is the fate of THIS paper. Copies of my objections are enclosed.

Thus I beg you to acknowledge the reception of the above two papers and to write me whether my objections to the third paper are under examination. And I beg you to do this IMMEDIATELY after the reception of this letter. Do not let me to send you again a dozen of letters and to phone you a dozen of times asking whether my papers have reached you. And I repeat, if the JOURNAL OF PHYSICS does not wish to maintain scientific contacts with me, state this clearly in a letter, which is to be signed by the EDITOR of the journal and not by you. Physics is COMPLETELY different from that present in the papers published in your journal. The scientific community must become aware of this.

Sincerely yours, Stefan Marinov

Edit. note. This letter remained unanswered.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

11 June 1990

Prof. Robert Romer
AMERICAN JOURNAL OF PHYSICS
Merrill Sc. Bldg., Room 222
Box 2262
Amherst
MA 01002

Dear Dr. Römer,

To my letter to you of the 26 February 1990 there is still no answer and I am anxious whether this letter has reached you or not. Neither Prof. Rindler has answered my letter to him of the 27 February and this COINCIDENCE tells me that the cause is not in the post.

The problems, however, are TOO IMPORTANT to be "settled" by silencing them. I wish to have your answer whether you have RECEIVED my letter of the 26 February.

The question which I wish to clear is the following:

Prof. Rindler asserted in the AM. J. PHYS., **57**, 993 (1989) that, for the case of a magnet moving with a velocity **v** and generating the magnetic potential **A**, the electric intensity induced in a piece of a wire at rest is not

$$\mathbf{E} = - \mathbf{v} \times \text{rot}\mathbf{A}, \tag{1}$$

as it is commonly accepted by the relativistic physics, but

$$\mathbf{E} = (\mathbf{v}.\text{grad})\mathbf{A}, \tag{2}$$

as it is already deduced in many of my publications.

On the 20 December 1989 I submitted a Letter to the Editor of the AJP to note that formula (2) is already deduced by me (and that ANY child has to deduce this formula).

You rejected my Letter to the Editor with the following motivation in your letter to me of the 9 January:

... we are not interested in publishing papers that criticize or attack well-established theories such as relativity and Maxwell's electromagnetism.

Meanwhile with my Letter to the Editor of the AJP I wished only to defend MY priority in introducing the extremely important formula (2).

Now my paper ABSOLUTE AND RELATIVE NEWTON-LORENTZ EQUATIONS (rejected by the AJP on the 1 August 1989) will appear in PHYSICS ESSAYS. A copy of this paper is enclosed.

In the light of this publication I pose you my INSISTENT question and I should like VERY MUCH to have your WRITTEN answer:

WILL YOU PUBLISH MY LETTER TO THE EDITOR OF THE AJP AND RECOGNIZE MY PRIORITY IN INTRODUCING FORMULA (2), OR YOU WILL PREFER TO FURTHER SILENCE THE PROBLEM?

I am looking for your prompt answer.

Sincerely yours,

∩. Marinov

Stefan Marinov

Editorial note. This letter remained unanswered.

Marinov's letter to Prof. Romer of the 26 February 1990 was published in TWT-VII, p. 323.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

11 June 1990

Prof. D. Finkelstein
INT. J. THEOR. PHYS.
Georgia Inst. of Techn.
Atlanta
GE 303320-0430

Dear David,

I use the occasion to thank you for your letter of the 10 November 1989 (post stamp date) although the rejection of my VERY IMPORTANT paper THE MYTHS IN PHYSICS was, of course, not pleasant for me, moreover taking into account that you have not presented motivations for this rejection.

Now I submit to your journal other two EXTREMELY IMPORTANT papers:

1. REPETITION OF WHITEHEAD'S EXPERIMENT FOR DEMONSTRATING THAT DISPLACEMENT CURRENT IS A PURE MATHEMATICAL FICTION.

2. MAXWELL'S ILLUSION: THE DISPLACEMENT CURRENT.

The PACS numbers are 03.50, 41.10.

All charges will be paid by myself.

Herewith I transfer the copyright for this paper to your journal.

In the paper THE MYTHS IN PHYSICS which you have rejected (surely without having read it attentively) I reported on my Bul-Cub machine without stator with which I demonstrated a violation of the angular momentum conservation law. In the present two papers I report on[a] historical and on my original experiments which show that displacement current neither acts with potential magnetic forces on other currents nor react with kinetic forces to the potential magnetic forces of other currents, i.e., that the displacement current is not current. These experiments show that circuits with condensers are OPEN circuits and, as according to the Lorentz equation (i.e., according to Grassmann's formula) the action of current elements VIOLATES Newton's third law, my present experiments SHOW why could I violate this law MACROSCOPICALLY with my Bul-Cub machine without stator.

In the case that you will reject my paper (and I am almost sure that you will reject it) I beg you to answer the following question:

WILL, ACCORDING TO YOUR CONCEPTS, THE DIELECTRIC RING IN FIG. 4 OF THE FIRST PAPER ROTATE WHEN ALTERNATING CURRENT WILL BE SENT IN THE APPARATUS or NOT? I shall not accept the rejection of the paper if this question will be not answered by Y

A photograph to the first paper and better executed drawings will be sent if the paper will be accepted for publication. I must however emphasize that the reproduction of the figures from Whitehead's historical paper can be not very good. I can, of cours redraw these figures but I think that it is better to publish the ORIGINAL figures.

Hoping to receive your acknowledgement for the reception of these two papers and th in due time also your final decision,

Sincerely yours,

J. Marinov

Stefan Marinov

PHYSICS ESSAYS

11 June 1990
PE2500/KLA

Dr. Stefan Marinov
Institute for Fundamental Studies
Morellenfeldgasse 16
A-8010 GRAZ,
AUSTRIA

Re: Manuscript: PROPULSIVE AND ROTATING AMPERE BRIDGES AND THE PRINCIPLE OF
RELATIVITY, by Stefan Marinov, submitted for publication in <u>Physics
Essays</u> (received 19 July 1989).

Dear Dr. Marinov:

I sent your manuscript and comments back to the original reviewer, who responded with
the enclosed comments.

I am still worried about your paper especially in light of the first comment by the
reviewer.
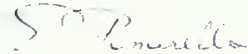
I would like you to consider very carefully and objectively this review and revise the
paper in the text, and not as an Addendum. In rewriting your paper, please use a very detached
tone, almost an impersonal one. In this way your paper might have some impact on the physics
community, which otherwise would be lost. Also, the title should be mitigated into the one
indicated above. These are all steps that encourage a reader to study your article, rather than
avoiding it.

Since I would like to keep a tight schedule on this process of revision, I would therefore
like to have your revised manuscript, <u>in triplicate, back to me by 28 August 1990, at latest</u>.
Moreover, in retyping the manuscript, please follow the Instructions to Authors here enclosed.

I thank you for having submitted your paper to us.

Sincerely yours,

Editorial note. Marinov answers this letter
            with his letter of the
            25 June 1990.

E. Panarella

Enc.

PHYSICS ESSAYS

Comments on:

PROPULSIVE AND ROTATING AMPERE BRIDGES VIOLATE THE PRINCIPLE OF RELATIVITY with Addendum

by Stefan Marinov

1. Marinov now accepts that Ampere's force law does explain the torque on his Ampere bridge motor. Since this law complies with Newton's third law, it cannot produce self-propulsion nor perpetual motion. So what is the point of the paper?

2. I did not assert anything about the circuit portion BCDEFG of fig.1, and therefore the mathematics of equs.(18) to (20) does not apply to my closed loop ABCDEFGH plus return conductors.

3. I do not agree that two parts of one current loop can always be represented by two loops. Infinitely thin, coincident filaments cannot carry finite currents. Two real wires of finite cross-section would greatly change the calculated forces.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

12 June 1990

Dr. Peter Newmark
NATURE
4 Little Essex Street
London WC2R 3LF

Dear Dr. Newmark,

You know very well which are my relations with Dr. Maddox. During 5 years he conti-
nuously promises to publish papers of me and on me (written by him) and he does not
this. During these five years I came five times to London with the only scope to acce-
lerate the publication of these materials and during these five years I spoke with
Dr. Maddox more than 1000 times on the phone.

I think that Dr. Maddox is an ill person, he is a kind of a CLINICAL LIER. Dr. Mad-
dox has an excellent character, he is well-educated person and he acts on me with an
IRRESISTIBLE CHARM. I said this many times to Dr. Maddox and I can confirm this sta-
tement also to you: I love Dr. Maddox, he is for me an EXTREMELY SYMPATHETIC PERSON.
But after five years of hearing his lies (in which, can you imagine, EVERY TIME I BE-
LIEVED because of his personal charm acting on me!), I see that simply we have an ill
person. I beg you very much to tell me, how you (and your colleages) appreciate Dr.
Maddox. If you also are the opinion that he is an ill person (you know him much better
than me), then, please, take "my problem" in your hands and give me the decision of
NATURE on the papers submitted by me and accepted by Dr. Maddox for publication.

You know very well that the paper "Experimental violations..." was composed BY ME
in the Nature's office. Dr. Maddox confirmed to me writtenly that it will appear on
the 18 August 1988, then (with his letter of the 29 July 1988) postponed the publica-
tion date for the 13 October 1988. When I visited the Editorial office of Nature in
September 1988 YOU PERSONALLY confirmed to me that the paper will appear. Meanwhile
the paper until now has not appeared.

I send you my last two letters to Dr. Maddox (of the 27 February 1990 and of the
21 March 1990). I beg you very much to answer all questions posed in these two letters.
Perhaps you will say that you have to await for the return of Dr. Maddox (he will be
in the office on the 25 June). Leave the answers of these questions which you cannot
answer without him and answer these questions which you CAN answer, first of all, tell
me whether you consider Dr. Maddox as an ill person.

I beg you also to write me your DIRECT phone number. I phone from Austria and when
I go through the operator, I lose too much time (and money). After receiving your
letter I should like to have a conversation with you.

Maybe, Dr. Maddox is not an ill person, but he postpones the publication of my ma-
terials because he wishes to be sure who is right: Einstein or Marinov and jump at
the just train. I gave him too much time for taking a decision; following his invita-
tion, I came to London once more to explain to him the errors of today's physics. I
can no more await. NATURE has to decide: either to publish my papers or to reject them!

Dr. Maddox ordered on the phone my book TWT-VII which I sent him on the 21 March.
In every phone conversation he promised that "next day" he will send me the money for
the book (Ł 20). If NATURE does not wish to purchase the book, be so kind to send it
back to me, putting Ł 5 for my post expanses.

I beg you to write me as soon as possible. The experiments which I have done are
from an extreme importance for mankind.

Sincerely yours,

Stefan Marinov

Editorial note. This letter remained unanswered.

J. P. W E S L E Y, *Ph.D. Physicist*

Weiherdammstr. 24, *7712 Blumberg, West Germany,* Tel: 07702-658

14 June 1990

Dear Stefan,

Although you might find the word "tension" used to mean voltage in some English text of over a hundred years ago; it is now limited to a mechanical force tending to tear something apart. Its original meaning belongs on the psychoanalyzer's couch, having no physcial meaning at all. You can't change the vagaries of a language. You cannot expect to invent your own Italian language and expect to be understood in Italy. The language of physics in English is arbitrary, and as you note, not always sensible; but its the only language available; so if you write a physics paper in English then you must use the English language for physics!

No, I will not bother with the Biot-Savart law for your particular geometry for a wire of finite cross section. Enclosed is a paper in which *it is done* for a rectangular bridge.

Re: page 174, Eq. (4) <u>Prog. Space-Time Phys.</u>:

$$r^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2.$$

Since J_1 and J_2 are parallel to the x, y, or z directions; how could they be parallel to $\mathbf{r}$?

You are right; I copied a sign wrong! It should be $+(2/3)\ln2$ in Eq.(12) on page 173, and not $-(2/3)\ln2$. The error is contained in further work in the paper and in Fig. 3. The error is <u>not</u> in the enclosed paper.

I do not understand you. The force on Ampere's bridge for a finite cross-sectional area, as given by Eq.(12) (with corrected sign) in <u>Prog. Space-Time Phys.</u> page 175, is an extremely strong function of the thickness of the wire (for a given current I); as given by the $\ln(L/w)$ term. As expected, it goes to infinity if you let w go to zero! Where do you get the idea that it is not a function of the thickness of the wire? You confuse me!

The constant μ_o has no physical meaning what-so-ever; it is arbitrarily and nonsensically chosen as $4\pi \times 10^{-7}$. It is one of the sad aspects of the engineering system (SI-system) of units. They are ridiculous. Only gaussain units are scientific. When gaussian units are used only a c occurs and that <u>only</u> in combination with the time t.

For the case of M and N going to infinity as you wish to consider the answer for w small and a square wire is given by Eq.(12). The force is

$$F/2I^2 = 13/12 - \pi/3 + (2/3)\ln2 + 1 - 0 + \ln(L/w).$$

What is your problem? Naturally a ln2N term cannot stand alone; after all terms are combined, one gets Eq.(12) with no such ln2N term all by itself.

Again one usually gets in their undergraduate courses proofs for the convergence of integrals involving the inverse square force over volume densities. It may be readily shown that integrating Eq.(2) on page 171

over a small volume where J_i and $\mathbf{J_2}$ occupy the same volume yields a finite answer. I have done it (even though I already knew the answer). I have no idea where my notes are. I don't want to do it again here. I don't want to spend the time.

A corona effect is easily distinguished from a possible "Grassmann effect" by 1) turning off the lights and seeing the blue glow, 2) by putting ones finger near the end and feeling the prickling sensation, 3) by listenning carefully and hearing a very slight crackelling sound. And, if in doubt, one simply puts a bit of tape over the ends! The tape will not change the Grassmann effect; but it will change the corona effect.

If you do the Ampere bridge experiment, you <u>too</u> will find that the mercury cups give problems. Everyone reports the difficulties; and they are to be expected.

I see no merit in publihing my letters (or not publishing my letters) in TWT; as I find that you are too far off from any reasonable understanding of the electromagnetic problems involved.

Paul

copy to Pappas

Editorial note. See Marinov's answer on the next page.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

20 June 1990

Prof. J. P. Wesley
Weiherdammstr. 24
D-7712 Blumberg

Copy: P. T. Pappas

Dear Paul,

Thank you very much for your letter of the 14 June 1990.

I shall publish your letters to me in TWT, only if they are written in a clear and DIDACTIC way, so that the reader can easily "enter" into the problem and profit of the exchange of our thoughts. I beg you to take seriously the publications in TWT as soon (because of the perpetual motion machines described there) TWT will be sold in thousands of copies.

Unfortunately many aspects of your letters are not clear even·for me, although I read your writings with great attention.

Now let me answer your letter of the 14 June.

1. I sustain firmly the opinion that the word "electric tension" is to be introduced again into the English scientific language and the word "voltage" is to be abandoned. Also the word "electromotive force" is to be abandoned and replaced by the word "electromotive tension". The symbol $\mathcal{E}$ for electromotive force used at the present time (which, moreover, cannot by found in the typing machines nor distinguished by handwriting from the usual letter E) is to be replaced by the symbol U, with which I denote the quantity "electric tension". There are different kinds of tension: electromotive tensions (chemical, inductive, piezoelectric etc.), ohmic tensions (when current flows along a wire), Coulomb tensions (between the plates of a condenser) etc. All of them are to be distinguished one from another, when appearing in the same context, by respective indices. I shall follow this line in my writings hoping that after 100 years humanity will accept i

Many scientists are unhappy with the word "electromotive FORCE", as "force" has completely different sense and dimensions. Some recent authors use the term "electromotance" which I consider also as bad.

I beg you to take into account that these changes which I introduce in the terminolog are not linguistic but scientific.

2. It is a pity that you did not wish to help me to calculate the pushing force on the Ampere bridge by the help of Grassmann's formula when using your method with the current densities.

3. You object my assertion that the current density **J** along the portions 2 & 5 (and al 7 & 10) of your Ampere bridge on p. 173 of Sp.-T. Phys. 87 is not parallel to the distance **r** between the current elements of these portions. I repeat, when the cross-section of the wire is small I do not see how **J** can be not parallel to **r**.

I do not understand your mathematics in the WHOLE Sect. II on p. 172 of Sp.-T. Phys. If you wish to help me, write and explain all mathematical calculations as to an undergraduate. I am your very SYMPATHETIC reader and I spent much time trying to understand and check your mathematics but I could not. Help me.

4. The force on the Ampere bridge (with long enough legs) depends only on the current It depends neither on the thickness of the wire, nor on the shape (form), nor on the size The most easy proof can be given for the shape: If the pushing forces on a Π-form and U-form bridges will be different, then if making a very long rectangular loop ending on the one side by a U-form, the loop will be set into motion by internal forces. Similarly can be given the proof that the force cannot depend on the thickness of the wire. In TWT-VII, p. 165 I showed that the pushing force on a U-form bridge does not depend on the radius of curvature, i.e., on the size of the bridge. Many authors also have noticed that the pushing force on the Ampere bridge depends only on the current and on NOTHING ELSE. For this reason I have dedicated too much time for trying to calculate in a mathematically exact way the pushing force on a U-form bridge, but I obtained only the result that the force must by stronger than 0.3443 μN/A^2. I do not abandon the efforts to obtain an EXACT number for this force and I beg you to help me.

5. If the force on the Ampere bridge will be $f = \mu_0$ N/A^2 in the system SI, it will be

$f = 4\pi/c^2 = 4\pi/9 \times 10^{20}$ dyne/abampere (cm^2/sec^2) in the CGS-system, what is a universal constant.

6. I see that you save your formulas where you have such terms as ln(2N), where N is length with dimensions "cm", but formulas must ALWAYS be written well. If some of your undergraduate students will write lnN, you, surely, will give him a bad note. Meanwhile in your final result (formula (15) on p. 178 of S.-T. Phys. 87) you have again a NASTY term lnd, where d is length measured in millimeters.

Nevertheless I have the feeling (not the conviction!) that your calculations are good. They are only difficult for understanding by people with undergraduate mentality and mathematical capabilities as me.

7. The problem with the corona effect in Pappas Z-shape antenna experiment is not of how a corona effect can be detected (if you have read my paper on p. 8 of TWT-V and my paper on p. 235 of TWT-VII, you could establish that in the last time I investigate the corona effects experimentally very intensively). The problem is WHY when there is a corona effect the Z-shaped antenna of Pappas does rotate and why when there is no corona effect, it does does not rotate. You will say: At the availability of corona effect, there will be a back push on the antenna caused by the electrons leaving the antenna, as is the case when charging the antenna by static electricity. Meanwhile I shall say: When there is a corona effect the electrons in the alternating currents streaming to the end points of the antenna are NOT stopped there and the unbalanced Grassmann's forces which remain to act alone set the antenna into rotation.

8. Now I should like to give a general comments on the Weber-Wesley electrodynamics. I find your approach very interesting and the fact that you obtain so many important results is remarkable. I think, however, that the W.-W. electrodynamics is WRONG.

I shall present a speculation on an undergraduate level as FUNDAMENTAL controversies can be settled only on an undergraduate level. I beg you VERY MUCH to answer my criticism also on an undergraduate level.

I shall refer to your paper EVIDENCE FOR WEBER-WESLEY ELECTRODYNAMICS AND LIMITATIONS OF MAXWELL THEORY which you kindly sent me with your last letter.

You affirm (formula (10)) that according to your theory the magnetic potential originated by a current element I'dr' at a space point distant r from it is

$$A = I'dr'/cr. \qquad (A)$$

In the text after your formula (7) you assert that the magnetic intensity is to be calculated from the magnetic potential according to the formula

$$B = rotA. \qquad (B)$$

Thus the magnetic intensity originated by the above current element will be

$$B = rotA = (I/c)rot(dr'/r) = (I/c)\{(1/r)rot(dr') - dr\times grad(1/r)\} = I'dr'\times r/cr^3, \quad (C)$$

as $rot(dr') = 0$.

You assert (formula (73)) that the force acting on a charge q moving with a velocity **v** in a magnetic field **B** is

$$f = (q/c)v\times B = (q/c)v\times(I'dr'\times r/cr^3). \qquad (D)$$

Writing qv = Idr, we obtain

$$f = (II'/c^2r^3)dr\times dr'\times r. \qquad (E)$$

Thus for the interaction of two current elements you obtain GRASSMANN FORMULA. Meanwhile you assert that the right formula must be Ampere's formula and that Grassmann's formula is WRONG.

On p. 183 of TWT-VII I wrote: "If Pappas will write the magnetic potential **A** generated by a current element Idr at a distance r along an arbitrary direction, so that by multiplying in a certain manner this magnetic potential **A** by another current element I'dr', he will obtain Ampere's formula, I shall pay him $ 1000." --- Although Pappas is always short with money, he could not win this sappy sum.   Yours, *Stefan*

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

20 June 1990

Dr. K. K. Datta
INDIAN JOURNAL OF PHYSICS
Jadavpur
Calcutta 700 032

Dear Dr. Datta,

Thank you very much for your letters of the 7 and 2 March 1990, although the rejecti of my papers ON THE ABSOLUTE ASPECTS OF ELECTROMAGNETIC INTERACTIONS and VIOLATIONS OF T LAWS OF CONSERVATION OF ANGULAR MOMENTUM AND ENERGY was, of course, not pleasant for me.

I do not accept the referee's criticisms on these papers. The fact that my papers are published with difficulties in the scientific journals is not an argument against my theory and EXPERIMENTS. Experiments either must be accepted or one has to show that their results are wrong. If the referee cannot show that the experiments are wrong, he MUST accept their results. Then the referee either has to try to explain these results in the frame of the conventional theory, or (in the case that he cannot do this), he mus ACCEPT the new proposed theory. Only this way is a scientific way.

I have done a lot of experiments which show that the answer of NATURE is not such a one as one should expect proceeding from the conventional theory on space-time physics and on electromagnetism. The results of my experiments must be presented to the judgement of the scientific community. Of course one can be mistrustful and one can reject my papers, but this can last only a limited time, as experiments cannot be silenced.

I know how difficult is for the referees and the editors of the physical journals to publish my papers. But with DIFFICULTIES I already published about 50 papers in the refe reed journals. My published experiments are not discussed in the literature because no-body dares to take the responsibility to say "yes" or "no", but soon THEY WILL BE DISCUS SED. (A paper of me has appeared in the IND. J. PHYS., **55B**, 403 (1981)).

But I have many CRUCIAL experiments which VIOLATE laws of conservation. Such are the BUL-CUB MACHINE WITHOUT STATOR and the ROTATING AMPERE BRIDGE WITH DISPLACEMENT CUR RENT (to distinguish it from the ROTATING AMPERE BRIDGE WITH SLIDING CONTACTS consrúcte recently by me, a report on which will be published in PHYSICS ESSAYS). The presentatio of these two machines are given in the paper

THE MYTHS IN PHYSICS

which I submit now to IND. J. PHYSICS.

These two experiments VIOLATE the law of angular momentum conservation. Yes, <u>they VIOLATE a fundamental physical law</u>. If the referee should suggest rejection, he has:

1) Either to show that my experiments are WRONGLY done, i.e., that they cannot demon strate the reported effects.

3) Or, if he will accept their effects, he has to show that these experiemts do NOT violate the law of angular momentum conservation. I think, however, that even if he wil be successful in showing that these apparatus do not violate the law of angular momentu conservation, the paper is to be published, as the experiments are INTERESTING and one must allow to the executor of an interesting experiment to present his explanation of the effect observed.

I send only one copy of the paper, as my hopes that the paper will be accepted are feeble. The photographs will be sent, if the paper will be accepted. I can change the style of the paper, but I think that when one destroys a FUNDAMENTLA PHYSICAL LAW, one must be allowed to say this loudly enough, without the eternal Saxon "it seems", "one is afraid", etc.

You, Dr. Datta, have a difficult task. I should like that you find enough scientific courage to publish this paper.

Hoping to receive your acknowledgement for the reception of this paper and then also your final decision in due time,

Sincerely yours,
S. Marinov
Stefan Marinov

Editorial note. The answer to the above letter is given by Dr. Datta with his letter
of the 3 July 1990.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

25 June 1990

Dr. E. Panarella
PHYSICS ESSAYS
Nat. Res. Council
Room 100, Bldg. M10
Otawa
Ontario K1A OR6

Dear Dr. Panarella.

Thank you very much for your letter of the 11 June 1990 concerning my paper PE2500/KLA.

I have rewritten the paper following ALL your suggestions and convincing me once more in the validity of the formula

$$\text{Impact} = (\text{Quality})^n,$$

i.e., that the impact which a paper can have on the reader is equal to the quality of of the paper at a power n, where n is the number of rewritings of the paper. Thank you of having suggested to me to rewrite the paper! Now it is almost perfect.

Here I shall answer the three remarks of the referee in his second comments.

1. I do NOT accept Ampere's law when writing (in the addendum of the last variation of my paper): " I agree that Ampere's formula predicts for my rotating Ampere bridge a torque in the same direction as predicted by Grassmann's formula". I only noticed that Ampere's formula predicts the SAME direction of rotation as Grassmann's formula (and as Nature does). From this conclusion the referee cannot make the conclusion that I accept Ampere's formula. - I also agree that Goebbels' statement "Every lie if 1000 times re-peated becomes a truth" is true, but this does not signify that I accept Dr. Goebbels.

Thus I continue to assert that the Ampere's bridge is propulsing by the action of in-ternal forces and NOT by the action of the currents in the stator (as is the assertion of Ampere's formula). The point of the paper is exactly AT THIS POINT.

2. In his first comments the referee wrote:

The Ampere force law predicts that Marinov's motor will not rotate if C and F are welded conductor junctions and AB and GH are made very long compared to DE. The current from a battery or other source would then have to be introduced with slip-ping contacts at A and H. Also the battery leads would have to be kept well away from the motor. If the motor still rotates with these modifications, then I accept Marinov has proved his point of selfpropulsion.

I showed in the addendum (this text is now included in the paper - p. 8) that if the sliding contacts will be put at the points B, G or at the points A, H, far enough from the bridge, then ALSO ACCORDING TO GRASSMANN'S FORMULA, the bridge will NOT rotate. According to Grassmann, a CLOSED current loop cannot rotate about an axis and violate the angular momentum conservation law. But an UNCLOSED current loop CAN ROTATE about an axis violating thus the angular momentum conservation law. Such are the loops in my Bul-Cub machine without stator and in my Rotating Ampere Bridge with DISPLACEMENT current.
    UNCLOSED loops(i.e., circuits with condensers) rotate under the action of INTERNAL FORCES and such a rotation can be explained only by Grassmann's formula but NOT by Ampere's formula as the WHOLE stator wire comes from infinity and goes to infinity ALONG the AXIS.*

3. I gave in the new version of the paper a more DIDACTIC explanation (p. 5) why two parts of one and the same current loop act one on another with equal and oppsoitely di-rected forces (according to Grassmann). If this "topological" explanation is not suffi-cient for the referee and he will ask me "How thin must be the two parallel wires", or "One cannot send finite current along very thin wires because they will be burnt", etc., I shall prefer to cut the discussion because even to the statement "The day has 24 hours" one can raise objections of being imperfect.

Hoping to receive soon   your final decision concerning my paper

    PROPULSIVE AND ROTATING AMPERE BRIDGES AND THE PRINCIPLE OF RELATIVITY,

I wish to point out that the photograph of the RAF-machine was sent with the previous version of the paper.

Sincerely yours,

Stefan Marinov

Stefan Marinov

*One can put the source of alternating tension along the axis of the Bul-Cub machine without stator BETWEEN THE POINTS OF SUSPENSION and then the ROTATING system will be COMPLETELY ISOLATED.

DER STANDARD

Herrn
Stefan Marinov

Morellenfeldgasse 16
8010 Graz

Wien, 26. Juni 1990

Sehr geehrter Herr Marinov,

wir bestätigen dankend den Erhalt Ihres Schreibens vom 20. Juni d. J.
und der Artikel "Meine Vorschläge zu dem 27. Parteitag der KPÖ",
und "Dem christlichen Kommunismus entgegen".
Wir haben den Inhalt mit Interesse zur Kenntnis genommen, müssen Ihnen
aber leider mitteilen, daß wir beide Artikel aus Platzmangel nicht ver-
öffentlichen können. Wir haben in der Ausgabe vom 22. Juni d. J. einen
Artikel über den bevorstehenden Parteitag der KPÖ auf Seite 9
veröffentlicht und haben auch über den Bundesparteitag berichtet.
Mehr können wir leider nicht darüber schreiben, da uns wie gesagt,
nicht genug Platz zur Verfügung steht.

Mit freundlichen Grüßen

DER STANDARD
Redaktion

Editorial note. See Marinov's letter to the journal DIE VOLKSSTIMME of the 1 May
1990.
See the following page.

ЭПИЛОГ МАРИНОВА К ЕГО УСИЛИЮ СПАСТИ МИРОВОЙ КОММУНИЗМ ОТ КРУШЕНИЯ

Итак газета DIE VOLKSSTIMME отказала опубликовать мою статью DEM CHRISTLICHEN KOMMU-NISMUS ENTGEGEN. Я поставил этот вопрос перед партийной организацией в Граце. Геноссе Э. Кальтенеггер ездил в Вену и обсуждал этот вопрос в газете и в ЦеКа, но ответ был тверд: Партия и газета не согласны опубликовать мою статью. В результате этого отказа я выполнил поставленный мой ультиматум и вышел из рядов КП Австрии.

Мое письмо геноссу Гарберу и обе статейки были посланы австрийским газетам DER STAN-DARD, DIE PRESSE и немецкому журналу DER SPIEGEL.

DER STANDARD ответил мне своим идиотским /неподписанным/ письмом от 26-го июня, подобным идиотскому письму газеты DIE VOLKSSTIMME от 21-го мая, так что я только сказал себе: "А чего же еще можно ожидать от этих убогих венских провинциалов, которые, наверное, никогда в жизни не слышали, что такое ПЕРПЕТУУМ МОБИЛЕ и каково его значение для чело-вечества, особенно при настоящей трагичной   энерго-экологической ситуации."

Газета DIE PRESSE и журнал DER SPIEGEL сочли мое утверждение, что палачи Гиммлера в лагерях Аушвице и Бельзене-Бельзене являются жалкими дилетантами по сравнению с их кол-лагами в лагерях Ловече и Белене-Белене как обиду на немецкую нацию и вообще мне не от-ветили.

Таким образом ценный опыт привнесенный болгарами в гигантской борьбе с инакомыслием, инакочувствием и инаковерием /не газом душить и в землю закапывать, а дубиной /сопой/ приканчивать и свиней кормить/ останется "опытом в пустыне".

И когда будущие Гиммлеры приступят снова к делу /а в разных странах мира они и теперь не ленятся/, то они опять ухватятся за неэкономичный и неэффективный, но широко-извест-ный немецкий способ массового уничтожения.

Я прилагаю статью из ШПИГЕЛЯ, где болгарскому способу кормления свиней уделено пара мелких строчек, а известно, что существует информационный порог, который мелкая инфор-мация не может переступить и фактически такая информация умирает к вечеру того самого дня, в котором ее напечатали. Ведь директор БиБиСи так напутствовал своих служащих: "Чтобы не допустить какую-то информацию пойти посвету, ее нужно выпустить только раз в двух словах."

▬▬▬ Bulgarien ▬▬▬

Schmutzige Knochen

Horror-Lager nach Hitlers und Stalins Vorbild: Bulgarien hatte, wie erst jetzt herauskommt, seinen Gulag.

Das erste Opfer am Tag meiner An-kunft war ein Mann aus Plowdiw. Er mußte aus der Reihe treten, sich mit dem Gesicht nach unten auf die Er-de legen und wurde vor unser aller Au-gen mit Knüppeln erschlagen.

„Zuerst hat er geschrien und um Gna-de gebettelt, doch die Bewacher haben nur laut gelacht. Dann schoß Blut aus dem Nacken des Mannes. Die beiden Offiziere, Lagerchef Oberstleutnant Ni-kola Gasdoff und sein sadistischer Vize

Zwatko Goranoff, schlugen noch, als der Mann schon längst tot war."

Bojidar Witanoff, 47, der die Horror-Szene erstmals schildert, war Mithäftling und Augenzeuge. Als 18jährigen Schüler hatte man ihn in das berüchtigte „Arbeits-Erziehungslager" Lowetsch, 130 Kilometer nordöstlich von Sofia, eingeliefert. Morgens um fünf klingelten mehrere mit Pistolen bewaffnete Männer an Witanoffs Wohnungstür: „Sie kamen in mein Zimmer gestürzt und schrien: Sie hören imperialistische Musik! Woher haben Sie die? Wo ist das Tonband? Gestehen Sie alles! Später, auf der Miliz-Wache am Boulevard Dondukoff, hat sich keiner mehr dafür interessiert."

Jazz-Fan Witanoff war zuvor schon siebenmal von der Miliz festgenommen

In den Jahren zwischen 1945 und 1962 unterhielt das Regime mindestens 20 solcher Umerziehungslager, die neue politische Opposition führt sogar 60 Lager auf. Alle waren nach nazideutschem Vorbild eingerichtet und betrieben worden. Häftlingsbaracken, Zwangsarbeit, Hungerrationen, Kapo-Bewacher, Zählappell, Tritte, Schläge, Quälereien und Mord beherrschten auch in Bulgarien den KZ-Alltag. Toten wurden sogar, wie bei den Deutschen, die Goldzähne herausgebrochen.

Berichte von Überlebenden mit präzisen Angaben über Opfer und Täter zwangen die noch immer regierenden Kommunisten, die sich Anfang April in „Bulgarische Sozialistische Partei" (BSP) umbenannten, zur Flucht nach vorn.

schrecklichen Wahrheit sehr nah kommt. Die Lager waren für Kriminell und politisch gefährliche Personen e richtet worden, Übergriffe und Recht verletzungen seien auf die mangelnd Aufsicht durch die Behörden zurückz führen. Als Hauptgrund für die Tode fälle werden „grobe Perversionen de Regimes, ständige Prügel und Verle zung der menschlichen Würde" angeg ben.

Mörder der Häftlinge waren lau Kommission in der Mehrheit „Krimine le, ein Kreis grausamer Sadisten". Zw sind fast alle namentlich bekannt, abe kein einziger der sadistischen Täter wu de bisher festgenommen.

Die Bulgaren wühlen nun selber in ih rer Vergangenheit – und das im Wor sinn. Nach Hinweisen aus der Bevölke

Staatspräsident Mladenoff, geöffnetes Massengrab: „Endlich einen Schlußstrich ziehen"

worden, wegen zu enger Jeanshosen, zu langer Haare oder weil er sich mit jungen West-Touristen im Musik-Café des Hotels „Bulgarija" getroffen hatte. „Wahrscheinlich hat mich einer der Nachbarn verpfiffen."

14 Monate lang mußte er in den Steinbrüchen des Todeslagers Lowetsch schuften. „Wie durch ein Wunder" hat er überlebt. Von den 1235 Gefangenen allein im Lager Lowetsch wurden nach offiziellen Untersuchungen 147 Häftlinge ermordet, nach Meinung der Überlebenden ist die wirkliche Zahl der Toten „mindestens dreimal so hoch".

Ein halbes Jahr nach dem unblutigen Ende der Diktatur des starrsinnigen KP-Chefs Todor Schiwkoff beherrscht Vergangenheitsbewältigung die politische Debatte. Immer deutlicher enthüllen Horrormeldungen das Ausmaß des bulgarischen Gulag, der die Herrschaft der Partei absichern sollte.

Staatschef Petar Mladenoff, der am 10. November vorigen Jahres den erfolgreichen Putsch gegen Schiwkoff angeführt hatte, ließ – ganz im alten Stil – ohne Gerichtsbeschluß drei der für die Todeslager Hauptverantwortlichen verhaften.

Doch ihr Chef, der ehemalige Innenminister Georgi Tsankoff, blieb unbehelligt. Die Erklärung der Justiz: Der Mann sei zu alt, und zudem habe man dem Minister, „wie in sozialistischen Regimen üblich", nie erzählt, was in den Lagern wirklich geschah.

Mladenoff setzte eine Untersuchungskommission aus Parlamentariern und Beamten des Innenministeriums ein, die freilich alle schon dem alten Regime gedient hatten. Kommentar der größten Oppositionszeitung *Demokrazija*: „Die Mörder richten über sich selbst."

Im April legte die Kommission einen Zwischenbericht vor, welcher der

rung haben Oppositionsmitglieder übe all im Land geheime Gräber geöffn und die Skelette von Opfern ausgegr ben, die 1944 nach dem Sieg der Rote Armee hingerichtet und verscharrt wo den waren: Ärzte, Lehrer, Kaufleute un Künstler, aber auch angebliche Fasch sten und Kollaborateure.

Fast täglich werden Gräber gefunde in Wäldern, an Bahndämmen, in Mo ren oder in verfallenen Häusern. D neue sozialistische Staatsmacht muß 20 000 willkürlich Hingerichtete eing stehen, die Opposition schätzt die Zal auf mindestens 200 000.

Aus Angst vor neuen Enthüllunge hat Innenminister Atanas Semerdsche unkontrollierte Ausgrabungen inzw schen verboten, angeblich um die Gr besruhe der Toten zu schützen. Aber d Bulgaren lassen sich von dem Verb nicht abschrecken. In der vorvergang nen Woche wurde in der Nähe des Do

Tran, nur wenige Kilometer vor der jugoslawischen Grenze, wieder ein Grab mit mindestens 30 Toten in einem vergiften Brunnen entdeckt.

Die stalinistischen Verbrechen der bulgarischen Kommunisten wurden das wichtigste Thema des Wahlkampfs. Am kommenden Sonntag können die Bulgaren über das erste freie Parlament seit 44 Jahren bestimmen. Die „Union der Demokratischen Kräfte" (SDS), ein Wahlbündnis von 25 neuen Parteien und Bürgerbewegungen, wirft der ehemaligen Staatspartei BSP vor, sie weigere sich, die Verantwortung für die Blutschuld ihrer Vergangenheit zu übernehmen. Nach jüngsten Umfragen liegen die Sozialisten in der Wählergunst mit 44,5 Prozent weit vorn, gefolgt von der SDS mit knapp 26 Prozent der Stimmen.

Um ihre kleinbürgerliche Klientel nicht zu verunsichern, haben die Sozialisten ihre Vergangenheitsbewältigung unterdessen gebremst. Die Beschäftigung mit dem alten Regime, so ihr Parteiblatt *Duma*, nehme den Bulgaren den Elan, sich mit den komplizierten Problemen von Gegenwart und Zukunft zu befassen.

Eine Leserbriefkampagne unterstützte in alten Zeiten die neue Linie der Partei. „Es muß endlich Schluß gemacht werden mit der Nekromanie", empörte sich ein treuer Genosse, „wichtiger als die alten Knochen ist Bulgariens Wirtschaftsreform." Ein anderer warnte: „Denken die jungen Leute, die jetzt mit den Gerippen Stimmung machen wollen, auch daran, daß unter diesen Skeletten mancher schmutzige Knochen ist, der das Vaterland verraten hat?"

Die drei für die Todeslager verantwortlichen Politiker wurden inzwischen aus der Untersuchungshaft entlassen, bleiben aber weiterhin unter Hausarrest. Geheimdienst-General Petko Kiproff, Mitglied der Untersuchungskommission und Chef der Archive im Innenministerium, behauptet: „Was in den Oppositionszeitungen steht, ist gelogen und schließlich Kriminelle und freche Hooligans gesessen."

Ob Lagerhaft, Zwangsarbeit oder der Tod an Häftlingen eine angemessene Strafe für angebliche Rowdys seien, beantwortet der General mit einem Achselzucken: „Das ist das Gesetz des Lebens." Gewalt habe es schließlich immer gegeben: „Die Guillotine arbeitete während der Französischen Revolution wie eine Maschine, und Straflager kannte Sibirien schon vor Stalin."

Auch Staatspräsident Mladenoff will nicht weiter in der Vergangenheit wühlen. Zur ließ er an einem Felsen im Steinbruch des Todeslagers Lowetsch eine eiserne Tafel anbringen mit der Aufschrift: „Nie wieder Gewalt, nie wieder Rechtsbruch. Wir verneigen uns vor den Opfern des Totalitarismus". Aber bei der

Überlebende im Gulag Lowetsch*
„Das Verbrechen darf nicht verjähren"

Gedenkfeier seiner Partei, Mitte April, forderte er, „endlich einen Schlußstrich zu ziehen".

Oppositionsanhänger lassen sich darauf nicht ein. Sie hatten in der Nacht vor der Parteifeier an einer unzugänglichen Steilwand ein Transparent angebracht mit der Aufschrift: „Die Mörder kehren an den Tatort zurück".

Doch das tun auch die Opfer: Zu den angeblichen „Kriminellen" oder „Hooligans", die in Lowetsch eingesperrt waren, gehört auch Nikola Dafinoff, 48, damals ein Schüler von 17 Jahren. Sein Verbrechen bestand darin, daß er privat sieben Sprachen, darunter auch „kapitalistische" wie Englisch, Französisch und Deutsch, lernte. Dafinoff: „Im Felsen arbeiteten Männer und Frauen von morgens fünf Uhr bis zur Dunkelheit, selbst am Sonntag mußten wir bis mittags schuften. Mit Loren wurden die Felsbrocken zur Steinmühle transportiert, was besonders mühsam war, wenn die verrosteten Gleise bei Regen und Schnee unter Wasser standen."

„Männer und Frauen, Alte und Junge, jeder mußte 17 Kubikmeter Steine täglich abliefern. Doch die meisten Sträflinge waren Intellektuelle, denen schwere körperliche Arbeit fremd war. Kaum einer konnte die Norm erfüllen." –

Wer weniger ablieferte, bezog beim abendlichen Zählappell Prügel. Ohne Mühe kann der Ex-Häftling ein halbes Dutzend Namen der Mehrfach-Mörder

nennen: „Schacho der Zigeuner, Blago der Esel, Gantscho der Barbier" und, die Schlimmste im Lager, „Julia die Schöne".

„Einen Schuß habe ich während meiner gesamten Lagerzeit nie gehört", erzählt Dafinoff. „Die täglichen Toten, zwischen fünf und acht, sind alle an den Folgen der Schläge gestorben."

Auf den Totenscheinen der Lagerverwaltung, heute im Archiv des Innenministeriums, sind die Todesursachen angegeben: Herzstillstand, Lungenentzündung, Gehirnerkrankungen, siebenmal sogar Hitzschlag – mitten im Winter. Die Leichen wurden in Säcke gesteckt und neben den Toiletten gestapelt, bis ein Lastwagen einmal in der Woche die Opfer abholte. „Vor allem im heißen Sommer stank es bestialisch", erinnern sich Dafinoff und seine Leidensgenossin Nadja Ilkowa, 61.

Keiner der Häftlinge ahnte damals, was mit den Leichen geschah. Erst jetzt konnten andere Zeugen die Wahrheit bekanntmachen: Um keine Spuren zu hinterlassen, ließen die Gulag-Herren die Toten auf der Gefängnisinsel Belene in einem „Objekt 6" genannten Bau den Schweinen zum Fraß vorwerfen.

Militärstaatsanwalt Boris Jotsoff, der im Lowetsch-Prozeß die Anklage vertreten soll, auf die besorgte Frage von Bojidar Witanoff, ob er für seinen Freimut nun mit Unannehmlichkeiten rechnen müsse: „Von mir nicht. Aber Sie wissen ja, es gibt noch viele Stalinisten mit großem Einfluß in unserem Land."

Deren Wut bekommt auch Petar Kornajeff zu spüren, der Vorsitzende des neugegründeten bulgarischen Advokatenverbandes. Fast täglich gehen bei ihm Drohbriefe mit der Forderung ein, sich aus den Untersuchungen der kommunistischen Verbrechen herauszuhalten.

Kornajeff tut das genaue Gegenteil. Er klagt den Präsidenten Mladenoff öffentlich des Rechtsbruchs an, die Regierung und das Parlament wegen Verletzung der Verfassung. „Wir, die Bulgaren, haben in der Nachkriegszeit dreimal, zuletzt in der KSZE-Urkunde, feierlich unterschrieben, daß Verbrechen gegen die Menschlichkeit nicht verjähren können. Der Prozeß gegen die Mörder von Lowetsch muß geführt werden."

Der Anwalt hofft auf den Sieg der Opposition bei den Parlamentswahlen: „Wir wollen mit Anstand und Würde auf das neue Europa zugehen." ◀

* Bojidar Witanoff, Nadja Ilkowa, Nikola Dafinoff vor der offiziellen Gedenktafel.

FYZIKÁLNY ÚSTAV

Centra elektro-fyzikálneho výskumu Slovenskej akadémie vied

Dúbravská cesta 9, 842 28 Bratislava

> Dr. S. M a r i n o v
> Morellenfeldgasse 16
> A - 8010 G R A Z
> Austria

Vaša značka   Naša značka   Bratislava June 29th, 1990

Vec

Dear Dr. Marinov,

Thank you very much for your letter of June 7th, 1990 and the enclosed volume of your encyclopaedic work Classical Physics. I sent it to the arbitrator of your paper "Physical essence of the Maxwell-Lorentz equations".

Please, allow me to repeat: Editorial Board of the journal Acta Physica Slovaca with respect of the point of view of the referee /the arbitrator agreed with it/ decided that the mentioned paper would not be published in our journal.

As regards the papers "Violations of the laws of conversation of angular momentum and energy", "Repetition of Whitehead's experiment for demonstrating that displacement current is a pure mathematical fiction" and "Maxwell's illusion: The displacement current" which you sent to our office in the letter of March 16th, 1990 I am very sorry but I must put it you that you did not keep the instructions to the authors. I enclose it to this letter. Therefore, they are not considered for publishing in the journal Acta Physica-Slovaca.

Sincerely yours

Edit. note. This letter is answered
    on 9 July 1990.

        RNDr. J. Kalužný CSc.,
        Managing Editor

MS 900612

Address for proofs:

Dr. Stefan Marinov
Institute for Fundamental Physics
Morellenfeldgasse 16
A-8010 Graz, Austria

Author(s):
Stefan Marinov

Title:
Maxwell's Illusion
11 pages +4 figures

Status:
Declined 90.07.01

Author misses the physical content of Maxwell's Equations

Date:
90.07.01

Title:
Repetition of Whitehead's Experiment for Demonstrating
that Displacement Current is a Pure Mathematical Fiction
11 pages +4 figures

Status:
Declined 90.07.01

Date:
90.07.01

editorial note. Marinov answers this letter with his own of the
3 August 1990.

INDIAN ASSOCIATION FOR THE CULTIVATION OF SCIENCE

From **Dr.(Mrs.) K.K. Datta,**
~~ximxmxxxxxxm~~ **Asst. Secretary &**
~~xxxxxxxxx~~ **Scientific Editor,**
INDIAN JOURNAL OF PHYSICS

JADAVPUR
CALCUTTA-700 032
INDIA

In reply please quote No.IJP-1/501/90.

3 - 7 - 90.

To

Dr. Stefan Marinov,
Morellenfeldgasse 16,
A - 8010 GRAZ - AUSTRIA

Dear Dr. Marinov,

Thank you for your paper 'The myths in Physics' and
the ~~xxxxxxxxx~~ accompanied letter dated June 20, 1990.
The Board of Editors, however, has decided not to
accept this kind of controversial papers for publication
in Indian Journal of Physics. Hence, you are requested
not to send this kind of paper to our journal any more.
Your present paper is being returned herewith.

Sorry for the inconvenience !

Your sincerely,

(K. K. Datta)

Editorial note. The paper THE MYTHS IN PHYSICS is published in TWT-III, p. 59.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

FAX: Austria 0316/827560

3 July 1990

To FAX: 0044/71/8369934 .
_____

To the Managing Editor
(in her absence to the person
 who   has taken the responsibility)
NATURE
4 Little Essex Street
London

To hand copy to Dr. Maddox

Dear Lady,

On the 12 June I sent a letter to Mr. P. Newmark to which until the present day I have not received an answer. Later I saw in the editorial information in NATURE that Mr. Newmark is no more in its editorial staff. But my letter was to be answered by this person who has taken his place.

I beg you that you find this letter to Mr. Newmark and that you answer it by FAX today before 17.00 your time.

I beg you also to take from Dr. Maddox my letter to him of the 21 March 1990 and to give answers to all 6 questions posed there (by discussing the matter with Dr. Maddox).

I beg you to confirm in your fax that Dr. Maddox' article (which he faxed to me on the 13 March 1990 - or an improved variation) will appear on the 12 July, together with my Letter to the Editor, as Dr. Maddox promised to me five minutes ago on the phone.

And I beg you to confirm whether Dr. Maddox will send to me the ₤ 20 which he owes me for the book TWT-VII which he has ordered   some months ago.

I was so good to Dr. Maddox. I believed 5 years in his lies. He has not appreciated my sincerity and he has trampled with his feet the belief of a man in his fellow man. Every time I believed in Dr. Maddox' promises. But he has trespassed all limits of human decency.

I hope that Dr. Maddox will finally understand that his abhorrent behaviour is unpardonable and I shall pray to the Lord to help him in his difficult hours.

Looking forward for your FAX-ANSWER today before 17.00 London time,

Sincerely yours,

J. Marinov

Stefan Marinov

STEFAN MARINOV
Morellenfeldgasse 16   4 July 1990
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

Fax: Austria 0316/827560

To FAX: 0044/71/8369934

To the Managing Editor
(in her absence to the person
 who restitutes her)
NATURE
4 Little Essex Street
London WC2R 3LF

To hand a copy to Dr. Maddox

Dear Lady,

I excuse myself that I have not noticed your name, but I have no time to go to the library to look at.

To my fax of the 3 July there was no answer, although I begged you to send it to me yesterday before 17.00 London time by fax. I BEG FOR YOUR ATTENTION.

My contacts with NATURE have an enormous importance for the progress of science and for the future of mankind. I have done numerous experiments which have demonstrated the invalidity of the principles of relativity and equivalence, of the laws of conservation of angular momentum and energy, and   the fallacy of a great part of today's concepts on electromagnetism. With some scarce exeptions of some random journals, no physical journal in the world publishes the results of my experimental and theoretical research, as otherwise the whole body of today's space-time physics will crumble to pieces in a couple of months.

Dr. Maddox was the only editor of an authoritative scientific vehicle who since five years promises to publish papers of me. However, during all these five years his promis turned to be only systematic flagrant lies. In these five years I came five times to London (once Dr. Maddox payed me the trip), I spoke with Dr. Maddox more than 1000 time on the phone and we exchanged hundreds of letters, telegrams and faxes. This was an enormous psychic, financial and time devouring expense from my side (I earn my bread as a groom in a stable). I did all these sacrifices because I am the only man in the world who is able to inform humanity about the first perpetuum mobile constructed on our pla- net - the machine TESTATIKA in the religious community METHERNITHA in Switzerland of which I am a member. The problem with this machine is VERY COMPLICATED, as its construc tor, Mr. Paul Baumann, thinks that humanity is not ripe for this illimited source of energy. I think that humanity will be never ripe, but I hope that this pure and clean source of energy will save our world from the ecological and ethical catastrophy to which it is precipitating. Although I published a whole book dedicated to the machine TESTATIKA (TWT-V), the world will listen to me only if I shall have a high enough tri- bune. I can mount on this tribune only when the world will hear about my own important discoveries. My own publications (the series TWT) reach   only a limited number of peop

I have observed in many of my experiments violations of the energy conservation law, but I could not succeed until now to construct a machine with a closed energetic circle (perpetuum mobile), as the machine TESTATIKA is (first of all because of the lack of money). However I constructed two machines (The Bul-Cub machine without stator and the Rotating Ampere Bridge with Displacement Current) which demonstrate a patent violation of the angular momentum conservation law. Such experiments cannot impress the journa- lists, as they are too stupid, but they can impress the scientific community. If the reports on these experiments will be published in a big scientific journal, in a couple of months the whole world will listen to every of my words. My Bul-Cub machine without stator is presented in the big paper which Dr. Maddox personally in 1987 and then I in 1988 finally have composed in the editorial office of NATURE. This paper had to appear on the 18 July 1988 (see the letter of Dr. Maddox of the 29 July 1988 published in TWT-III, p. 330). But Dr. Maddox still has not published this paper. When I came to Lon don in December 1988 for a lengthier discussion, I agreed to expect first for the publi cation of Dr. Maddox' own "Christmas puzzle" (since '84 Dr. Maddox is puzzling about things clear to 15-years old children!). However during a year and a half this notoriou "puzzle" does not appear, although Dr. Maddox promises to do this EVERY WEEK. I believe to Dr. Maddox all these years. I do not believe more. He has broken the jug.

Thus I address you and Dr. Maddox with the following ultimatum. If today before 17.0 London time my last two faxes, the letter to Dr. Newmark and my last letter to Dr. Madd will be not answered by a fax signed by you both, tomorrow at 11.00 I shall commit myse to the flames on the steps of the English consulate in Graz. Yours, *S. Marinov* S. Mari

GALILEAN ELECTRODYNAMICS
Box 251
Boulder, CO 80306
tel. 303-444-0841

6 July 1990

Dear Dr Marinov:

I am sorry I did not answer your letter of 7 April -- I
thought I had. I have no help and have to earn a living by pub-
lishing another journal, this one is a labor of love, so things
get very confused at times.

In any case, I don't think your paper is suitable for GE,
as it is really a letter to another journal on their article and
contains too many references to be easily understandable. There
are references always, of course, but here all the meat is in
your previous papers and the entire paper seems highly specula-
tive. Please look at the papers published in GE so far (issue 4
went out one month ago) for a model. All of them attempt to
present a closed idea with (easily available) references used
only to buttress peripheral points. That is not the case with
your paper, I am sorry.

Of course, if you argue with somebody in a previous paper,
your procedure is acceptable. But your letter argues with an
article published somewhere else.

As for your paper on electromagnetic potentials, it seems
incredible that a test charge could reverse direction depending
on the shape of the solenoid's cross-section under otherwise
equal conditions, and in any case, I cannot see how this would
explain Muller's findings (which already have a very simple
explanation: the emf is proportional to v cross B).

Yours sincerely,

Petr Beckmann

Petr Beckmann
Editor

Editorial note. Marinov answers this letter with his own of the 2 August 1990.

P. T. PAPPAS

Prof. of Mathematics, Doctor of Physics
Marcopulioti 26, Athens 117 44, Greece Tel. (0301) 86 23 278

Stefanon Marinou,   7 July 1990.
Paul Wesley,

Dear Stefane and Paul,

          This letter will answer the letter of 20 June 1990
of Stefano Marinou to Paul Wesley with a copy to myself, as well
as will answer page 183 of TWTVII. I hope Marinou will publish
it in the next TWT(?) VIII, without excuses. Marinou excuses for
declining papers, when* Marinou does not like, are not new.
Marinou when in power, becomes one of the most severe censors.
The establishment control and censorship looks an amateur tactic
compared to Marinou persistence either rational or irrational.
It seems that sometimes the heroes of the earth mankind surpass
their tyrants at the end when in power themselves.

When I attempted to talk to Marinou some ten years ago about the
matters discussed in the letter of June 20, 1990, Marinou did not
want to hear even the issue. Marinou was too busy constructing
his 1001 energy violating magnetic machine. Briefly, I should
repeat that in the experiment the forces of pi-frames do depend
on the wire's cross dipping in the mercury troughs. Data may be
found in J. Appl. Phys. 59(1),19,1986. This is in agreement and
predicted by the Cardinal law of Ampere. Similar predictions are
expected by the Lorentz law, ignoring the reaction problem,
momentum and energy conservation in the field and after making
certain assumptions for the convergence of the involved
integrals. The complete answer is analyzed in Physics Essays,
March 1990. The non rotating S-Antenna with insulating tape
covered ends (International Conference on the Interpretations
of RT, London 1988), is the most striking experiment that favors
the cardinal law and disproves the lorentz law.**

In the letter Marinou again brings the issue for about a 10001
time of certain terms such as "electromotive force" suggesting
the word "electromotive tension" instead. I would like to suggest
myself a term to use. This term is "Marinou paranoia". The
meaning of this term will be very useful. For example, it will
easily express the answer to the Marinou suggestion to write the
magnetic potential of a current in terms of the Ampere formula.
The answer is: It is a "marinou paranoia" to ask someone to write
the magnetic potential of a current, when one pretends to have
proved that magnetic energy is not preserved 10001 times. It is
even a more "marinou paranoia" to start an axiomatic foundation
of physics and electromagnetism based on the concept of potential
and its implied energy conservation to end up with energy
violation. I suggest to Marinou to check what is meant with
potential energy, and what are the three mathematical
requirements for a force field to be characterized by a potential
function. I hope Marinou should read the first condition for a
field characterized by a potential is that the closed work path
integral of the field is zero. The work integral integrated
between two points is independent of the path and always equal
to the difference of the two potential values at the two
considered points. This is considered as the condition for
conservation of energy (which Marinou pretends but not believes,
to have violated 10001 times, ignoring his forbidding potential
with so much "marinou paranoia").

* which ** Lorentz law

P. T. PAPPAS

Prof. of Mathematics, Doctor of Physics
Marcopulioti 26, Athens 117 44, Greece Tel. (0301) 86 23 278

As far as I am concerned I do believe that energy is not conserved in 1 part to 10^{10} on the average and in medium planetary scale. I am aware that the Ampere cardinal law correctly violates energy and that it will be a "marinou paranoia" to write a potential function for the cardinal law of Ampere or even for actual electrodynamics. There is no consistent potential function. There is no animal in the universe which people imagine with the name magnetic potential, period. These ideas are also expressed in the paper I am enclosing for publication in the TW Truth(?)VIII. A version of this paper was invited by Mr Ogden for publication in Electronics and Wireless World. Perhaps, it will also appear there, or it may be the second paper to be declined after being accepted as it was the case with the paper "Forces on S-Antennas". I hope Stefano and his readers will enjoy the reading.

Sincerely,

Dr. P.T.Pappas,
Professor of Mathematics,
Professor of Physics.

Editorial note. This letter is answered by Marinov's letter of the 26 July 1990.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

9 July 1990

Dr. J. Kaluzny
ACTA PHYSICA SLOVACA
Dubravska cesta 9
CSFR - 842 28 Bratislava

Your letter: 29 June 1990
received: 9 July 1990

Dear Dr. Kaluzny,

Thank you for your letter, although I remained unleasantly shocked, seeing that after FOUR months following the submission of my papers

1. Violations of the laws...

2. Repetition of Whitehead's experiment...

3. Maxwell's illusion...

you rejected them with the motivation: "You did not keep the instructions to the autho

In the instructions you have underlined the following two requirements:

a) The papers should deal with original research, not published so far. - MY PAPERS DEAL WITH ORIGINAL RESEARCH AND ARE NOT PUBLISHED SO FAR.

b) The authors are requested to attach a letter with a clear demand to publish their contribution in Acta Physica Slovaca. - IN MY LETTER OF SUBMISSION OF THE 16 MAR 1990 THIS DEMAND WAS CLEARLY EXPRESSED.

As far as the other instructions to the authors are concerned, I saw only two point of discrepancies:

1. A formal point: You require that the pages should be numbered in the upper right hand corner. Meanwhile my pages are numbered on the upper side in the middle. Then, you request that the references should be given as follows: 1. Gillmann, L: Phys. Rev. 104 (1965) 435. Meanwhile I give them in the form: 1. L. Gillman, Phys. Rev. **104**, 435 1965).

2. Important point. You request that the international system of units (SI) should b used. I did this in the second of the submitted papers which is PURELY EXPERIMENTAL paper. However in the first and third papers, which are predominantly THEORETICAL, I used the Gauss system. I consider as a CRIME to write THEORETICAL PAPERS DEDICATED TO THE FUNDAMENTALS OF PHYSICS (as my papers are) in the idiotic, HELAS!, system SI.

Thus I beg you VERY MUCH to note WHICH DEMAND of your instructions to the author was not fulfilled by me.

If you do not wish to publish my papers because they contradict FUNDAMENTAL PHYSICA "LAWS" accepted by today's physics, as the principles of relativity and equivalence and the laws of conservation, you had to write this clearly. I know very well that as the referees of the physical journals (and I am since 20 years in an every-day-contact with ALL physical journals of the world) cannot present objections to my papers (and if such objections are presented, then IMMEDIATELY I show their inconsistency) and break the contacts in a "polite" form, they present some formal reasons. I have the feeling that this is also the case with you.

Thus I beg you very much to write me clearly whether you should like to receive papers from me (in such a case you have to examine them, and after the reception of my objections, you have to give them to arbitrators), or you do not wish. In the second case you will spare your and my time and money.

Hoping to receive your answer soon (and in the case of breaking of the contacts, also my submitted papers),

Sincerely yours,

Editorial note. This letter was answered by Dr.
Kaluzny with his letter of
the 27 July 1990.

Λ. Marinov

Stefan Marinov

Stefan Marinov's seasonal puzzle

With yet another holiday season brewing up in the Northern Hemispehere, readers may wish to brood on a conundrum devised by an anti-relativist. No prizes are offered for a solution.

STEFAN Marinov, the exiled Bulgarian physicist now living in Graz in Austria, is himself a puzzle. He is indefatigable in the persecution of what seems to be his only cause, which is to prove that Einstein's theory of relativity is a pack of lies.

Marinov claims, among other things, to have shown by direct measurement that the velocity of light differs according to the direction of its travel along a fixed path. He has also partly developed what he says is a perpetual motion machine, a kind of Wimshurst machine driven by an electric motor which, when last inspected in his journal's London office, was said to require a car battery for its operation.

But most of Marinov's work is theoretical. Over the years, he has bombarded this and other journals with a series of papers with titles such as "The myths of physics" and "Violations of the laws of conservation of angular momentum and energy". Details of his correspondence with several editors may be found in his series of volumes called The Thorny Way of Truth, of which seven volumes have now been published by International Publishers (East–West) at Graz, of which Marinov appears to be the sole owner.

From time to time, Marinov threatens to immolate himself in front of a British embassy or consulate if he cannot get an answer to a question about the publication of one of his papers. Some years ago, when embarked on such an enterprise outside the British consulate at Genoa, Marinov ran away when the police approached. Afterwards he explained that he had done so because he was in Italy illegally, without a visa. Mercifully, he has threatened to immolate himself on Nature's account for many months.)

Part of everybody's difficulty is that so little is said about the details of the experiments whose results are said to contradict common expectation for even sympathetic critics to be able to provide constructive statements of how they and Marinov part company. Another is that Marinov's single-minded zeal for his own conviction considerably outstrips the zeal of those who may disagree. But from time to time, in Marinov's copious writings, there are relatively simple arguments that appear accessible even to those still at high school. Here is one series of gedanken experiments presented as if it were a Christmas puzzle (the original intention), with some helpful (or misleading) hints for a solution.

The figure shows a pair of circular conductors arranged as two concentric circles. Equal electrical currents are circulated in each, but in opposite directions. The simplest way of creating this arrangement is to cut through the concentric pair at some point and to join the loose ends in pairs by short lengths of straight conductor. An electromotive force applied anywhere along the conductor will engender a current which must be everywhere uniform. At the bridged gap, there will be equal currents flowing in opposite directions, so their influence on the magnetic fields in the concentric gap will be zero.

The device is thus a means of arranging that there is a uniform magnetic field in the space between the concentric circles in a direction perpendicular to their plane (downwards into the plane of the paper when the current in the circuit flows in the direction indicated). The sensor in the experiment is a conductor long enough just to bridge the gap between the concentric circles and mounted on thin insulating supports in such a way that it can be made to slide around the circle. The objective is to measure the voltage across the sliding conductor, either by a standard voltmeter or by a condenser whose accumulated charge will be a measure of the voltage in a steady state.
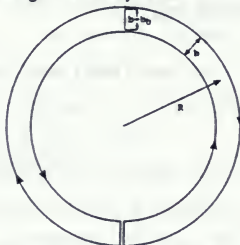
The simplest case is when the sliding conductor is at rest. Then there is no voltage. Right? Next comes the case in which the sliding conductor moves at uniform speed around the concentric gap, always pointing along a radius of the concentric circles. As the slider moves, it will cut through magnetic lines of force at a constant rate, so there will be a constant voltage across the ends. The polarity of the slider will depend only on the direction of the current in the concentric circuit, and not on whether the slider moves clockwise or anticlockwise. Right again?

Now comes the tricky part, at least so far as Marinov is concerned. What happens if the sliding conductor is fixed in space, but the underlying concentric circuit is rotated about its centre? Relativity theory naturally predicts that the voltage across the sliding conductor would be the same as in the first experiment, and with the same polarity. On the other hand, questions may be raised about the degree to which the pattern of magnetic forces generated by the current is dragged around the ring by its rotation. Maybe there is a smaller voltage, but with the same polarity. What, asks Marinov, is the answer?

The second conundrum is superficially simpler: simply rotate the apparatus in its own plane, about the centre of the concentric circles. (There will be a small voltage due to the Earth's magnetic field, but this may safely be neglected.) Is there now a voltage, and with what polarity? If the answer to the first question is "Yes" the answer to the second must be "No", and vice versa. Readers are invited to make up their minds before reading on.

Marinov's own answers are unambiguous. Vice versa wins the day. When the underlying concentric circuit is rotated and the slider is kept fixed, there is no voltage across the movable conductor. But when the whole apparatus is rotated about its centre, the voltage across the now-moving sliding conductor is identical with that obtained when the slider is moving relative to the concentric circuit.

The implications are evidently important. The null answer to Marinov's first question implies that relativity has vanished through the window, the affirmative answer to the second implies that an isolated apparatus carrying a circulating current will generate a voltage when rotated, which raises forbidden questions about absolute space. Indeed, Marinov has devised a gedanken measurement of the Earth's velocity through space by stretching his concentric circuit into a linear version of it. He also claims that these violations of simple expectation are the basis on which his perpetual motion machine is built.

What is the truth? Nobody is quite sure, for nobody has done the experiments — not even Marinov. Indeed, one's confidence in the whole enterprise is somewhat undermined by Marinov's flat statement that he does not bother to repeat experiments whose outcome must be obvious. For one whose confidence in his own heterodoxy appears to be sustained only by his confidence that experiments will prove him right, Marinov seems curiously passive in the business. **John Maddox**

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA   23 July 1990

Tel. 0316/377093

Dr. John Maddox
NATURE
4 Little Essex Street
London WC2R 3LF

Dear Dr. Maddox,

Once more, now in a written form, I wish to express my admiration for your scientific courage of having published your "conundrum" in NATURE. This short article of you will be the COUP DE GRACE to relativity. The relativists will NEVER promise you this pace. I do not know whether they will poison or stone you, but be cautious. I do not wish to lose you, as I LIKE you, and you know this.

I wished to await for the publication of my LETTER TO THE EDITOR (published on p. 221 of TWT-VII) and for the answer to my last letters, and then to write my comments to your article. But after our today's conversation I have the feeling that you will again postpone and postpone the publication and the writing of the letter. On the other hand, you said me that you will await for a reaction of the readers to your conundrum. Look, Dr. Maddox - NONE of the relativists will send you a comment. NONE, NONE, NONE. You think that the relativists are so stupid to begin a discussion which they surely will lose?! - You shall receive some comments only by non-relativists, some of them on a low scientific level (unfortunately the scientific level of the anti-relativists is not high).

Thus, I wrote my comments to your article and I submit it for your CORRESPONDENCE column. I beg you very much in a very short time (2 - 3 days) to give me your decision whether you will publish my comments (and WHEN) or whether you will reject them. But I wish to have a WRITTEN answer, as in the case of rejection I shall submit this comment to NEW SCIENTISTS, enclosing your rejection letter.

Although the relativists will not comment your conundrum, I give to relativity, after the publication of your article, no more than 6 months of life. The CHILDREN will make whistles, and these children's whistles will bury relativity.

We surely will speak on the phone in the next days and you will give me your decision on the publication (rejection) of my present LETTER TO THE EDITOR, but I REPEAT, I wish to have a WRITTEN answer.

Once more admiring you and thanking you,

with my friendly greatings,

J. Marinov

Stefan Marinov

HOW DR. MADDOX BLABBED OUT THE SECRET ABOUT THE GOAT'S EARS
OF KING ALBERT

Dr. Maddox' article in Nature (**346**, 103, 1990) poses a couple of questions on a conundrum which is tormenting him since many years. In his article Dr. Maddox evades to give his own predictions, although, as he states, the matter "appears accessible even to those still at high school".

I wish, however, to inform the readers of Nature that Dr. Maddox has already given his own predictions in a letter to me of the 8 March 1985 and these predictions can be seen on p. 296 (see also pp. 301 and 304) of the second volume of my series of documents The Thorny Way of Truth (TWT-II) which is on sale in all scientific bookshops of London.

I shall not cite here Dr. Maddox' answers, and the curious reader can search for them in my book.

I wish only to note that the simple picture which Dr. Maddox has copied in his article from my scientific advertisement (New Scientist, **112**, 48, 1986) is not a "conundrum". This is the diagram of an experiment carried out by E. H. Kennard (Philosoph. Mag., **33**, 179, 1917) published in the year when Lenin took the power in Russia. The results of Kennard's experiment (which I call the rotational Kennard experiment) are the same as predicted by my absolute space-time theory and contradicting the predictions of King Albert. When the loop consisting of the two concentric circles will be transformed into a prolongated rectangle, I call this the inertial Kennard experiment.

Variations of the inertial Kennard experiment (when moving the wire with respect to the loop and when moving the loop with respect to the wire) have been carried out by my friend Francisco Müller (Miami) and published in TWT-VII (p. 319), noting that Müller used a flat permanent magnet instead of a loop. Müller's report has now appeared also in Galilean Electrodynamics (1, 27, 1990).

I carried out the inertial Kennard experiment for the case when wire and loop move together and succeeded in measuring the Earth's absolute velocity with an electromagnetic experiment (my three previous measurements of this velocity were optical - Czech. J. Phys., **B24**, 965, 1974; Gen. Rel. Grav., **12**, 57, 1980; TWT-II, p. 68). I submitted a report on this experiment to Nature handing it to Dr. Maddox during my fifth visit of his editorial office in 1988. The report was entitled "Action of constant electric current on electrons at rest due to the absolute velocity of the Earth" and can be read in TWT-IV, p. 110. Until the present day there is no decision from the part of Dr. Maddox about rejection or acceptance. The experiment is so simple that even high-school students can carry it out. For its execution one needs some hundreds meters of wire, two leaflets of an electrometer and a car battery - nothing else.

Thus the problem is not to give answers to a "conundrum", as the answers are given by Nature itself (I do not mean the journal Nature), but to recognize the absolute character of the electromagnetic phenomena and the failure of the principle of relativity.

Although Dr. Maddox thinks that with his article he has digged out only a hole in th woodland, and after shouting there "King Albert has goat's ears", has covered it again, but over the hole a willow will grow up, the children will then make whistles of its branches, and soon the sacred secret will come to the ears of the whole world.

Concerning the perpetual motion à la Wimshurst machine mentioned at the beginning of Dr. Maddox' article, the truth is the following: The name of this machine is TESTA-TIKA, its inventor is Paul Baumann, the spirital head of the Christian community METHER NITHA in the village of Linden, 30 km south of Bern.   I never have brought this ma-chine to London (the only machine which I have demonstrated to Dr. Maddox was my ball-bearing motor during my first visit of his office in 1985). As a member of this religiou community, I invited Dr. Maddox to inspect this first perpetuum mobile on the planet Earth. He promised to come, but never found time for the trip. At this situation I pro-posed to Dr. Maddox the following: If he will come to Switzerland and if then he will publish in Nature a photograph of TESTATIKA and of him in front of it with the declara-tion that the machine is not a perpetuum mobile, I shall pay him ₤ 10,000 (the money should be prepaid before the trip). Neither under these conditions   has Dr. Maddox come to Switzerland. Then I submitted to the advertisement office of Nature a two-pages advertisement with a photograph of TESTATIKA and short its description, prepaying the ₤ 1,000 of the   page charge. After three months and dozens of phone conver-sations, under the intervention of Dr. Maddox, the advertisement was rejected and the page charge was returned to me. All letters, telefaxes and payment orders are reproduced in TWT-VI and TWT-VII.   TWT-V is dedicated quite the whole to TESTATIKA.

Certain of my machines violating the laws of angular momentum and energy conservatior (until the present day I could not succeed to close the energetic circle and to run some of my machines eternally) are presented in the article "Experimental violations of the principles of relativity, equivalence, and conservation of energy and angular momen† (the short title is cited by Dr. Maddox). This paper was composed by me in the editorial office of Nature during my third visit in June 1988, as in the two years after its accep tance I have not received the proofs from Dr. Maddox, although during my second visit in March 1987 Dr. Maddox composed a part of the paper himself and I stayed in his room to correct the proofs. The paper had to appear on the 18 August 1988, then on the 13 Octobe 1988 (see the letter of Dr. Maddox to me of the 29 July 1988 on p. 330 of TWT-III) but until the present day it is not published and it can be seen only on p. 146 of TWT-III.

Note (second column of Dr. Maddox' article, down). At clockwise and counter-clockwise motion of the slider, the polarity of the induced tension is not the same, as Dr. Maddox asserts, but opposite.

Stefan Marinov
Institute for Fundamental Physics
A-8010 Graz, Austria

STEFAN MARINOV
Morellenfeldgasse 16   26 July 1990
A-8010 GRAZ — AUSTRIA

Prof. P. T. Pappas
Marcopulioti 26
GR-117 44 Athens

Dear P.,

Thank you very much for your letter of the 7 July and for the paper

AMPERE'S CARDINAL LAW IN EXPLAINING VIOLENT WATER ARC EXPLOSIONS
VERSUS MAGNETIC POTENTIAL

which you submitted for TWT-VIII. The paper is accepted and the book will go to print at the beginning of September.

If in the meantime you will have another paper for publication, be so kind to submit it. If the paper will come before the end of August, it might be included.

Now I shall answer the different items which you raise in your letter.

1. You blame me that I decline papers submitted to TWT, if I do not like them. Let us speak about your papers, as you do not know whether I have declined papers of other contributors (I did!).
   I have never declined a paper submitted by YOU. If I have declined such paper, please mention its title and my letter with which I have declined it.
   I started to publish papers of other contributors in TWT beginning with the IVth volume. You were presented: in TWT-IV with three papers, in TWT-V with one paper, entitled MARINOV AND HOLYWOOD PHYSICS which, as very colloquious was included in the correspondence section, in TWT-VI with two papers, in TWT-VII with one paper, and in TWT-VIII with one paper. This summer I phoned you twice to urge you to submit a paper for TWT-VIII. Finally you submitted such a one on the 7 July.
   I have rejected publication only of some letters of you, as you have written them in haste where bad physics was mixed with low style vituperations which could be of no help for the readers of TWT and which would be only damaging to your scientific reputation which I cherish. However, I wrote you that for the page charge of ß 40 I shall print any typed sheet which you will submit. When my papers were not accepted by the journals of the establishment, I paid ß 1000 for a page in NATURE and ß 3000 for a page in NEW SCIENTIST. You were not willing to sacrifice ß 40 for one page of you.
   Your present letter is also of this "vituperative class". This letter will only be damaging to your scientific reputation. But its physics is more or less reasonable. Thus I shall publish it free of charge, so that you stop to call me a TYRANT.

2. I always hear VERY ATTENTIVELY what you say to me. Ten years ago (July 1982) you came to the ICSTA-Conference in Genoa and told me about your experiment with the floating Ampere bridge. I heard you attentively, although I did not realize at that time the GRANDIOSITY of the problem. I think that neither you have realized AT THAT TIME the grandiosity of the problem. However, in the following years YOU turned the attention of the world to this problem and now it became one of the most discussed scientific controversies in which many distinguished scientists take part and the rail guns have been classified in the American military institutes becoming an important item in the SDI program.
   I published the report on your Ampere-bridge experiment in the PROCEEDINGS OF ICSTA (1982) and in the recent years I carried out many experiments with this bridge, culminating with two masterpieces: the RAF-machine (Rotating Ampere Bridge coupled with a cemented Faraday disk) which was the FIRST rotating Ampere bridge constructed by human hands, and the RABDC-machine (Rotating Ampere Bridge with Displacement Current) which violated the angular momentum conservation law. In TWT-VIII you will read about an experiment constructed by me which is based on the Ampere bridge.

3. There is no reliable experimental evidence whether the force on the Ampere bridge depends on the wire's cross-section. Your measurements (Table 1 in J. Appl. Phys., **59**, 19, 1986) say NOTHING to the topic, as the differences do not show any tendancy and are to be accepted only as measuring errors.
   My latest EXACT mathematical calculations of the force pushing the U-form Ampere bridge (note that the U-form Ampere bridge is introduced by ME) show that for pretty thick wire the force is lower because the currents filaments are farther one of another. However when the thickness of the wire tends to zero, the force converges to a PRECISELY DEFINED VALUE. For a U-form bridge with infinitely long legs this value is 0.5 $\mu N/A^2$.

My present calculation (see also TWT-VII, p. 165) is EXACT and you will see it in TWT-VII. Thus the conlcusion of Wesley (Progress in Space-Time Physics 1987, p. 170) that at thick ness of the wire tending to zero the force pushing the Ampere bridge tends to infinity is WRONG.

4. In my letters to Wesley, copies of which were sent to you and which will be publi shed in TWT-VIII, I gave my treatment of your S-shape-antenna experiment. I do not con sider this experiment as a proof of Ampere's formula.

5. I firmly have decided to call the "electromotive force" "electromotive tension" and I shall do my best to impel the world to accept this. You, of course, can call me "Mari nou paranoia". Perhaps you are right.
I wish only to note that YOU use MY terminology and not that of conventional phy sics. Let me cite from your paper published in TWT-IV, p. 169. On p. 171 in the second half you write: "The two symmetric discs in this way neutralized the TENSIONS induced by external fields..." If you do not wish to be "paranoic", you had to write instead of "induced tensions" "induced electromotive forces". Then further on the same page 171 you write: "Under these conditions excessive heating resulted changing the involved resistors and thermocouple TENSIONS." If you do not wish to be "paranoic", you had to write instead of "thermocouple tensions" "thermocouple electromotive forces", etc.

6. I, however, do not find any relation between this <u>terminological</u> problem and the problem whether the Ampere formula allows the introduction of a magnetic potential. And I consider your categorization of my question whether one can write a magnetic potential satisfying Ampere's formula as "paranoia" only as a LAPSUS of your hasty writing.

7. It is true that in the last years I have carried out many electromagnetic experi ments and constructed different machines in the hope to build a perpetuum mobile. I do not see any reason for ridiculizing me for my endeavours. Exactly on the opposite, one has to admire my persistence, taking into account that I finance my whole experimental activity from my own pocket. And the results are not bad, as I could reveal the weaknesse of conventional electromagnetism, although I was able to observe violation of the energy conservation law only in three experiments: 1) the ball-bearing motor, 2) the cemented (or uncemented) Faraday disk, 3) the machine MAMIN COLIU. But my research brought me to the village Linden in Switzerland. And if the only result of my efforts would be the fact that on the 3rd March 1989 I held in my hands the first perpetuum mobile on the planet Earth (the machine TETSTAIKA), I should be thankful to God.

8. Thus you agree that you cannot write a magnetic (i.e., vector) potential, so that Ampere's formula can be obtained as a mathemtaical result of this potential. Well. This is also MY opinion. But if one cannot introduce the notion magnetic potential **A**, one is unable to introduce the notion magnetic intensity **B** = rot**A**. I do not see then how you will be able to calculate the magnetic fields of wires and coils and how you will make the calculations enabling one to construct the electromagnetic machines on which a good deal of today's industry is based.
On the other hand, without introducing the electric and magnetic potentials of an electric charge q moving with a velocity **v**, one is unable to calculate the radiation field of this charge, thus one will be unable to calculate the whole technical park of radio-engineering. By asserting that "there is no animal in the universe which people imagine with the name magnetic potential, period" you BURN the whole body of human know ledge in electromagnetism created during two centuries by its most brilliant heads. Well. If you will be able to CACLULATE all electromagnetic machines which mankind builds with out using the notion "magnetic potential", this will be an enormous ACHIEVEMENT. But first you must show that you can calculate the machines. As a first homework, please, be so kind to give the answers to the problems 2, 3 and 4 enumerated on p. 183 of TWT-VI and win the total sum of 2100 dollars.

9. You assert that if a force field has a potential (you mean a SCALAR, not a VECTOR potential), then this force field cannot violate the energy conservation law. Then you assert that the Ampere cardinal law may violate the energy conservation law. Well. But Wesley (Progress in Space-Time Physics 1987, p. 193) and Spencer (see this volume) show that Ampere's formula can be obtained from the SCALAR Weber potential. Consequently Ampere formula cannot violate the energy conservation law. Please, comment!

Yours: *Stefan*

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

26 July 1990

Peter Graneau (Donald S. Thompson)
Center for Electromagnetic Research
Northeastern University
Boston MA 02115, USA

Neal Graneau
Department of Engineering Science
Oxford University
Parks Road
Oxford OX1 3PJ
England

Dear P. Graneau and dear N. Graneau,

I invite ANY of you to submit a paper for the VIIIth volume of my series THE THORNY WAY OF TRUTH. This volume will be dedicated quite the whole to the problem about the potential energy between two current elements (two moving charges) and to the force of their interaction (Ampere-Grassmann controversy).

In this volume, besides my papers on this topic, there are already submitted papers by P. Pappas, D. E. Spencer and H. Aspden. Prof. Wesley has promised to submit a contribution.

If you would like to submit papers, I beg you to do this AS SOON AS POSSIBLE. I know that now is summer time, but my intention is to print the volume at the beginning of September. Your paper(s) can be included even if they will arrive 2 - 3 days before giving the volume to print. Of course, I should like to have them earlier, before having numbered the pages, so that I can include the article at the "papers section" and not at the end of the volume (at the end there is always "correspondence section").

I send you for information TWT-VII, so that you can see which will be the character of TWT-VIII and how you have to prepare the manuscript which will be photocopied (send it on pages A_4 which then will be reduced at the print). As you can see, TWT-VII was dedicated quite the whole to the problem whether the displacement current is a current or not. TWT-VIII will be dedicated to the "cardinal law of electromagnetism", with historical papers and with reports on original until now not published experiments. Thus I shall appreciate VERY MUCH experimental contribution, but also purely theoretical contribution will be welcome.

If you would like to retain TWT-VII, be so kind to send me $ 25 in an envelope IN CASH (I lose from a cheque more than 1/3 part). If you would like to order also TWT-VIII, enclose further $ 25.

I shall be very glad if you will acknowledge the reception of this letter, so that I can know whether you will submit (and when) a paper.

I beg Dr. P. Graneau to transmit my greetings to his wife. I will be happy to meet them both at the new conference which Prof. Bartocci organizes at the spring of 1991.

Sincerely yours,

J. Marinov

Stefan Marinov

PS. Enclosed is an article on the "Kennerd's puzzle" from J. Maddox. Comments on this article are also welcome. This article will be reproduced in TWT-VIII together with the comments which Dr. Maddox intends to publish in the next issues of NATURE.

<u>Marinov's comments.</u> This letters was answered neither by P. Graneau nor by his son. The volumes TWT-VII have been neither returned nor paid. Let me note that I suggested to Prof. Bartocci to invite Prof. Graneau at the conference in Perugia where we established friendly and kind relations. Prof. Graneau knows very well that I earn my bread as a groom in a stable and that ANY of my books is extremely precious for me. Poor Peter!

PHYSICAL SOCIETY OF JAPAN

Kikai-Shinko Building, 3-5-8 Shiba-Koen, Minato-ku
Tokyo 105, Japan

July 26, 1990

Dr. Stefan Marinov
Niederschocklstr. 62,
A-8044, Graz,
Austria

Dear Dr. Marinov:

This letter is concerned with your article #1031
entitled "Absolute and Relative Newton-Lorentz Equations"
which you submitted for considering to our journal.
We have sent the paper to our referee who is eminent
in the field of particle physics and relativity.
According to his comments, your manuscript should not appear
in our journal.

Considering the referee's report, the editorial
committee of our journal discussed your paper. Our
conclusion is that your paper is not appropriate for
publication in our journal. Consequently we regret
that we cannot publish your article in the Journal of
the Physical Society of Japan. We are returning herewith
your manuscript to you.

Sincerely yours,

Editorial note. This letter and the next one
are answered by Marinov with
his letter of the 16 August 1990.

Taizo Masumi

この "著者に対する注意" にこの
まま複写して著者に送らせていた
だきますので御了承下さい。尚、
複写を希望されない場合はその旨
お申し出下さい。

Journal

著 者 に 対 す る 注 意

I examine the paper again, but sorry to say, I could not

change my attitude in rejecting it.

I believe it of no use to explain the author which

assumption lead him to the wrong conclusion, because

he does not accept the principle of relativity and the

whole consequence of it.

No reliable experiment has suggested a breakdown of the

relativistic conception.

PHYSICAL SOCIETY OF JAPAN

Kikai-Shinko Building, 3-5-8 Shiba-Koen, Minato-ku
Tokyo 105, Japan

July 26, 1990

Dr. Stefan Marinov
Niederschocklstr. 62,
A-8044, Graz,
Austria

Dear Dr. Marinov:

Thank you for submitting your article entitled "Action of Constant Electric Current on Electrons at Rest Due to the Absolute Velocity of the Earth" to the Journal of the Physical Society of Japan.

It has been examined by our referee and our editorial board. Regretfully, we have concluded that your manuscript is not sufficient enough to be published in our journal.

We are returning your manuscript to you.

Sincerely yours,

I cannot admit the acceptance of the paper for publication in the journal by the following reasons.

1) Theoretical part of the paper is not acceptable.

In the case of a moving loop, the author considered only a contribution from the vector potential A, but not one from the scaler potential Φ which is non-zero in this case.

2) Descriptions of the experiment is not enough and not clear to be judged correctly. It is not quantitative.

I can not believe the experimental result, together with the wrong theoretical treatment.

FYZIKÁLNY ÚSTAV

Centra elektro-fyzikálneho výskumu Slovenskej akadémie vied

Dúbravská cesta 9, 842 28 Bratislava

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┌  Dr. S. M a r i n o v  ┐
   Morellenfeldgasse 16
   A - 8010 G R A Z
   Austria              ┘
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Vaša značka   Naša značka   Bratislava July 27th, 1990

Vec

Dear Dr. Marinov,

Thank you very much for your letter of the 9th July 1990. On the behalf of the Editorial Board of the journal Acta Physica Slovaca, allow me to give you the message concerning yours papers : 1. Violations of the laws...
2. Repetition of Whitehead's experiment...
3. Maxwell illusion ...

We are very sorry but we have find the clear demand neither in your letter of March 16th, 1990 nor in the letter of July 9th, 1990. That is the reason why we have not started the referee procedure so far.

Sincerely Yours

RNDr. J. Kalužný CSc.,
Managing Editor

STEFAN MARINOV
Morellenfeldgasse 16   2 August 1990
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

Dr. Petr Beckmann
GALILEAN ELECTRODYNAMICS
Box 251
Boulder
CO 80306

Dear Dr. Beckmann,

Thank you very much for your letter of the 6 July 1990. In the mean time I received also Nr. 4 of your journal and I thank you VERY MUCH. Write me, if I have to pay the four issues which you have sent me. I find the journal interesting and I shall gladly pay the charge. Of course, if you will grant them to me, I will remain deeply touched.

I should like to make some remarks to your comments on my two rejected papers. Perhaps these remarks will be of help for you.

1. COMMENTS ON W. A. SCOTT-MURRAY'S ARTICLE.

The name of Scott-Murray and the reference to its article can be simply CANCELLED. This is a self-contained paper and its aim is to show that the gravitational potentials Φ, Φ' at two points on sea level which lie at two different lattitudes ϕ, ϕ' ($\phi < \phi'$) are not equal. Moreover, the paper shows that the difference between the potentials is such a one that if the linear rotational velocities of these points are $v = R\Omega$, $v' = R'\Omega$, where R and R' are the distances from the Earth's axis and Ω is the Earth's angular rotational velocity, then

$$\Phi - \Phi' = (v' - v)^2/2. \tag{1}$$

For this reason two clocks placed at these two points will run with the same rate, as the connection between their readings T and T' is

$$T' = T\{1 + (v' - v)^2/2c^2 + (\Phi' - \Phi)/c^2\}, \tag{2}$$

and consequently

$$T' = T. \tag{3}$$

Mr. Scott-Murray enters into the article only with his wrong assertion that the rates must be different (although observations have shown that the rates are equal).

In this article only TWO formulas are taken without showing their veracity: The first one is the above formula (2) and the second one is the formula for the gravitational potential at a point x, y, z on a homogeneous, oblate rotational ellipsoid consisting of an incompressible fluid of mass density μ (γ is the gravitational constant)

$$\Phi = - (\gamma\mu/2)\{I_0 - I_a(x^2 + y^2) - I_b z^2\}, \tag{4}$$

where

$$I_0 = (4\pi a^2/3b^2)(4b^2 - a^2), \quad I_a = 4\pi(6b^2 - a^2)/15b^2, \quad I_b = 4\pi(3b^2 + 2a^2)/15b^2, \tag{5}$$

where a and b are the major and minor semi-axes of the ellipsoid.

I point out where one can find the deduction of formulas (2) and (4).

If you further will sustain the opinion that in my paper "the meat is in my previous papers and the entire paper seems highly speculative", then I cannot help you more.

2. THE ELECTROMAGNETIC EFFECTS ARE DETERMINED BY THE POTENTIALS AND NOT BY THE INTENSITIES.

I do not understand what do you mean when writing:

It seems incredible that a test charge could reverse direction depending on the shape of the solenoid's cross-section under otherwise equal conditions.

In my paper I assert: If there is a solenoid with a cross-section of a prolongated rectangle, then if moving the solenoid in parallel to its short side, there will be an induced electric intensity acting on the charge at rest put in the solenoid, however if moving the solenoid in parallel to its long side, there will be not an induced electric intensity acting on this charge.

Müller's findings are explained by the formula $\mathbf{E}_{mot} = v \times \text{rot} A$ for the case when a wire

moves with a velocity **v** with respect to a magnet generating the magnetic potential **A** at the domain of wire's location, but for the case when the magnet moves with a velocity **v** with respect to the wire the formula for calculating the induced electric intensity is $E_{mot-tr} = (\mathbf{v}.grad)\mathbf{A}$.

If you consider the predictions stated in my paper as INCREDIBLE (although Kennard, Müller and Marinov have experimentally confirmed them), then it is time to accept them as true and to change respectively your concepts as soon as possible. Enclosed see my paper ABSOLUTE AND RELATIVY NEWTON-LORENTZ EQUATIONS which had to appear in June in PHYSICS ESSAYS. This paper will give you the mathematical apparatus for understanding the "strange" phenomena at the motional-transformer induction.

I should like to add that not only you accept the results of our experiments as INCREDIBLE. Such a NEVEREC is also Dr. Maddox and he even has exposed his amazement in NATURE (his article is enclosed for your information).

The paper of Dr. Maddox is not written badly but as he is a CLINIC LIAR (if Dr. Maddox says five sentences, three of them are always LIES), he has put in his paper again numerous lies which can be revealed by anyone who will peruse the different volumes of THE THORNY WAY OF TRUTH (TWT).

I send you my letter to Dr. Maddox of 23 July 1990 and the enclosed to it LETTER TO THE EDITOR OF NATURE and my letter to the editor in duty of Nature during the absence of Dr. Maddox of the 1 August 1990.

And now I have the following question: If NATURE will reject my LETTER TO THE EDITOR OF NATURE, and then NEW SCIENTIST will also reject it, will you agree to publish it on the pages of the next issue of GALILEAN ELECTRODYNAMICS?

And I submit another paper to your journal, entitled

THE MYTHS IN PHYSICS.

I hope that this time the paper will be accepted. If you will reject it, I shall send you another paper.

Hoping to receive your answer soon,

Sincerely yours,

D. Marinov

Stefan Marinov

PS. The photographs for the paper will be sent if it will be accepted for publication.

Editorial note. This letter remained unanswered.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

3 August 1990

Ref. 62412, 62413

Dear Dr. Feshbach,

Thank you for your letter of the 6 July 1990, although the rejection of my papers

    1. Repetition of Whitehead's Experiment for Demonstrating...

    2. Maxwell's Illusion: the Displacement Current

was, of course, not pleasant for me.

In my letter of submission of the 11 June 1990, expecting an eventual rejection, I wrote:

> In the case of rejection, I beg you to answer the question:
> Will, according to your concepts, the dielectric ring in fig. 4 of the first paper rotate when alternating current will be sent in the apparatus or not? If this question will be not answered PERSONALLY by you, I shall remain with the feeling that you have not read my paper.

You have not answered this question. For this reason, I send you the first paper again and I beg you ONCE MORE to answer this question. If I do all this, the reason is that you SURELY will give an answer which is CONTRADICTING the experimental evidence. In such a case you must consent, that my paper MUST BE PUBLISHED and you (as well as whole conventional physics) have to change your electromagnetic concepts.

Enclosed is the paper which Dr. Maddox wrote recently in NATURE on another "puzzle" for conventional physics (although there is nothing puzzling, if one will throw over board the wrong theory of relativity and will embrace my absolute space-time theory). Enclosed is also my comments on Dr. Maddox' paper which is submitted to NATURE but which SURELY Dr. Maddox will reject. I should like to add that for the publication of this paper of Dr. Maddox I visited him five times in London, I spoke with him no less than 1000 (THOUSAND) times on the phone and exchanged some hundred letters, tele-faxes and telegrams. Please, have the understanding that I cannot expend the same efforts with every editor of physical journal.

I send you also my paper ABSOLUTE AND RELATIVE NEWTON-LORENTZ EQUATIONS which will appear in PHYSICS ESSAYS, so that you can solve Dr. Maddox' "puzzle" by the relevant mathematical apparatus.

I beg you VERY MUCH, do not give me the advice to send the papers rejected by you to PHYSICS ESSAYS. This journal cannot publish all my papers.

Hoping to receive your answer soon,

                        Sincerely yours,

                        S. Marinov

                        Stefan Marinov

Editorial note. This letter is answered by Dr. Feshbach with his letter of the
                23 August 1990.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

3 August 1990

Prof. David Finkelstein
INT. J. THEOR. PHYS.
Georgia Inst. of Technology
Atlanta
GE 30332-0430

Ref. MS 900612

Dear David,

Thank you for your letters of the 1 July 1990,although the rejection of my papers

1. Repetition of Whitehead's Experiment for Demonstrating...

2. Maxwell's Illusion: the Displacement Current.

was, of course, not pleasant for me.

In my letter of submission of the 11 June 1990, expecting an eventual rejection, I wrote:

In the case that you will reject my paper (and I am almost sure that you will reject it), I beg you to answer the following question:

WILL, ACCORDING TO YOUR CONCEPTS, THE DIELECTRIC RING IN FIG. 4 OF THE FIRST PAPER ROTATE WHEN ALTERNATING CURRENT WILL BE SENT IN THE APPARATUS OR NOT? I shall not accept the rejection of the paper if this question will be not answered by YOU.

You have not answered this question. I beg you ONCE MORE to answer it. I am so insisting, because you SURELY will give an answer which will be in contradiction with the experimental evidence. In such a case you must consent that my paper MUST BE PUBLISHED and you have (as well as conventional physics) to change your electromagnetic concepts.

Enclosed is the paper which Dr. Maddox wrote recently in NATURE on another "puzzle" for conventional physics (although there is nothing puzzling if one will throw over board the wrong theory of relativity and one will embrace my absolute space-time theory). Enclosed is also my comments to Dr. Maddox' paper which is submitted to NATURE but which SURELY will be rejected by Dr. Maddox. I should like to add that for the publication of this paper of Dr. Maddox I visited him five times in London, I spoke with him no less than 1000 (THOUSAND) times on the phone and I exchanged hundred letters, telefaxes and telegrams. Please, have the understanding that I cannot expend the same efforts with every editor of a physical journal.

I send you also my paper ABSOLUTE AND RELATIVE NEWTON-LORENTZ EQUATIONS which will appear in PHYSICS ESSAYS, so that you can solve Dr. Maddox' "puzzle" by the relevant mathematical apparatus.

I beg you VERY MUCH, do not give me the advice to send the papers rejected by you to PHYSICS ESSAYS. This journal cannot publish all my papers.

You rejected my two papers by the motivation: "Author misses the physical content of Maxwell's equations."

There is only ONE WAY to show who understands these equations better: the prediction of experimental effects. Only this scientist understands well the Maxwell equations who rightly predicts the issues of experiments. Thus, be so good and give the answer to my above mentioned question. Then, as a home-work, solve Dr. Maddox' "puzzle".

I know that now in Atlanta it is very hot and you would prefer to go to the swimming pool. But, dear David, the questions are SO SIMPLE, SO SIMPLE. Think about in the pool!

Hoping to receive your answer soon,

Sincerely yours,

S. Marinov

Stefan Marinov

THE NEGLECTION OF EXPERIMENTS HAS NEVER
SAVED A THEORY.
Ecclesiasticus

Editorial note. This letter remained unanswered.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

6 August 1990

Prof. J. P. Vigier
PHYSICS LETTERS A
Institut H. Poincare
11 Rue P. et M. Curie
F-75231 Paris Cedex 05

Dear Prof. Vigier,

I still have not an answer to my letter to you of the 1 June 1990. I beg you very much to answer this letter and to confirm whether you definitely reject my paper V 1465a entitled CHILDISHLY SIMPLE EXPERIMENT VIOLATING THE PRINCIPLE OF RELATIVITY. In the case that you definitely reject this paper, I should like to submit it as soon as possible to another journal. Thus I beg you to inform me about your decision, so that I know what to do.

Now I submit to PHYSICS LETTERS A my paper (in two copies)

ON THE ELECTRIC INTENSITIES INDUCED IN RAILGUNS.

The PACS numbers are 03.50, 41.10.

Herewith I transfer the copyright for this paper to PHYSICS LETTERS.

All eventual charges will be paid by myself.

I enclose the "conundrum" which Dr. Maddox published recently in NATURE. Enclosed are also my comments to this article which I submitted to NATURE but my hopes that Dr. Maddox will publish it are very feeble. I think also that no relativist with name will dare to comment Dr. Maddox' paper (the reasons are presented in my letter to Dr. Maddox of the 23 July 1990 which is enclosed too). Nevertheless the children's whistles will soon tell to the world the truth about the goat's ears of King Albert.

Hoping to receive your answer soon,

Sincerely yours,

S. Marinov

Stefan Marinov

PS. In your article in PHYS. LETT., **142** (1989) 447 you write:

... some experimental confirmations of Ampere's longitudinal forces have even led some physicists (Marinov,...) to contend the validity of Einstein's theory of relativity.

I contend Einstein's theory taking into account the effects of dozens of OTHER experiments. Until the present time NOBODY has observed the action of "Ampere's longitudinal forces", as such forces simply DO NOT EXIST.

Editorial note. With a letter of the 13 August 1990 Prof. Vigier confirmed the rejection of the paper "CHILDISHLY SIMPLE EXPERIMENT..." (see the letter to Prof. Vigier of the 1 June 1990 - the editorial note at the bottom) and acknowledged the reception of the paper "ON THE ELECTRIC INTENSITIES... (published in this volume) but then no letter from him arrived.

STEFAN MARINOV
Morellenfeldgasse 16   16 August 1990
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

Prof. Remo Ruffini
IL NUOVO CIMENTO B

P.le Aldo Moro 2
I-00185 Roma

Dear Prof. Ruffini,

I still have no answer to my letter of the 9 May 1990, a copy of which is enclosed. I beg you very much to answer this letter and to tell me your decision concerning my paper

Nr. 9718.   VERY EASY DEMONSTRATION OF THE VIOLATION OF THE ANGULAR MOMENTUM CONSERVATION LAW AND OF THE FAILURE OF CONVENTIONAL ELECTROMAGNETISM.

I beg you also to tell me whether my papers

Nr. 1175 REPETITION OF WHITEHEAD'S EXPERIMENT...

Nr. 1176 MAXWELL'S ILLUSION: THE DISPLACEMENT CURRENT

will be sent to a referee.

It is time to take my theory and my experiments SERIOUSLY and not to reject my papers without having realized their importance for physics.

To show you that the time for opening the discussion on the validity of the principles of relativity and equivalence and of the laws of conservation HAS COME, I send you the paper of Dr. Maddox STEFAN MARINOV'S SEASONAL PUZZLE.

I enclose my comments to this paper which, as it seems, will be not published in NATURE. After receiving the letter of rejection, I shall submit my comments to NEW SCIENTIST.

I enclose also my paper ABSOLUTE AND RELATIVE NEWTON-LORENTZ EQUATIONS which "solve" Dr. Maddox' puzzle, as in this CHILDISHLY SIMPLE EXPERIMENT there is NO PUZZLE at all. Puzzling is only the fact that relativity has survived for almost 100 years.

Hoping to receive your answer soon,

Sincerely yours,

J. Marinov

Stefan Marinov

Editorial note. This letter remained unanswered.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

16 August 1990

Dr. Taizo Masumi
J. PHYS. SOC. JAPAN
Kikai-Shinko Bldg.
3-5-8 Shiba-Koen
Minato-ku
Tokyo 105

Dear Dr. Masumi,

Thank you very much for your letters of the 26 July 1990 with which you rejected my papers:

      1. ABSOLUTE AND RELATIVE NEWTON-LORENTZ EQUATIONS.

      2. ACTION OF CONSTANT ELECTRIC CURRENT ON ELECTRONS AT REST DUE TO THE...

The referee of the first paper writes: "I believe it of no use to explain (to) the author which assumption led him to the wrong conclusion, because he does not accept the principle of relativity and the whole consequence of it. No reliable experiment has suggested a breakdown of the relativistic conceptions."

The principle of relativity is not true, as there are SO MANY experiments which have showed its inconsistency. One of them is the experiment presented in the second paper which can be carried out by any student. In the comments on the second paper the referee writes: "Description of the experiment is not enough and not clear to be judged correctly. It is not quantitative. I can not believe the experimental results, together with the wrong theoretical treatment."

In the paper I wrote that I could not calibrate the experiment and give the magnitude of the Earth's absolute velocity, as the experiment was done very primitively. Nevertheless, as the effect is HUGE, I was able to measure the right ascension of the apex of the absolute Earth's velocity. Instead to say: "I can not believe the experimental results", the referee had to say to some of his students to repeat the experiment and to see the effect. Then he will believe the experimental results and he will quickly change his BLIND belief in the principle of relativity. The issue can be solved only on an experimental level, as if I shall insist "there is an effect" and the referee "there can be no effect", we can never come to a mutual understanding.

However, my experiment is of such a kind, that even following the MOST SIMPLE LOGIC, one must come to the result predicted and OBSERVED by me. This was recognized by Dr. Maddox, the Editor of NATURE, in his article of the 12 July 1990 which is enclosed. Enclosed are also my comments to this article which are submitted to NATURE. Thus the referee has to give his predictions for the Kennard's experiment (which is rotational) which was carried out in 1917 and to my inertial Kennard experiment which was carried out in 1989. I beg him at least to give his predictions. But I am sure that he will not dare to give his predictions, as he has to state that I AM RIGHT and relativity WRONG. If now the referee will not withdraw his comments and if he will not suggest my paper for publication, then I can only say: In this way science cannot progress.

    The referee asserts the following:"In the case of a moving loop, the author considered only a contribution from the vector potential **A**, but not one from the scalar potential Φ which is non-zero in this case." This is another BIG LIE of relativity, namely that if a current loop moves, it will become electrically charged. Further BIG LIE of relativity is that if a magnet moves with respect to a charged body, it will experience a magnetic force. We have to put once and for ever an end to ALL THESE LIES of relativity. For this reason I submit now my paper

      CHILDISHLY SIMPLE EXPERIMENT VIOLATING THE PRINCIPLE OF RELATIVITY,

which serves to show the fallacy of the second of the above relativistic dogmas.

Herewith I transfer the copyright for this paper to your journal.

All eventual charges will be paid by myself.

Hoping to receive your acknowledgement for the reception of this letter and then in due time also your final decision,

<u>Editorial note</u>. This letter remained unanswered.

Sincerely yours,
Ð. Marinov
Stefan Marinov

Relativity values

SIR—Stefan Marinov's apparatus designed to test relativity (*Nature* 346. 103: 1990) is clearly similar in principle to the electromagnetic flow-meter, which works by measuring the voltage induced by fluid moving in a magnetic field.

Assuming that such an instrument is set up to give zero output for zero flow, a reversal of flow direction reverses the polarity of the output. I would therefore say that "the polarity of the slider depends on the direction of the current in the concentric circuit, *and also* on whether the slider moves clockwise or anticlockwise".

We use electromagnetic flow-meters both as current-meters. in which case the instrument is fixed and the water moves. and as ships' logs. where the water may be stationary and the instrument moves. However. I have not yet managed to get an output from such an instrument by putting it in a bucket of water and marching across the laboratory with it.

A. J. BUNTING
Institute of Oceanographic Sciences.
Deacon Laboratory,
Brook Road. Wormley,
Godalming,
Surrey GU8 5UB. UK

To be added to the LETTER TO THE EDITOR OF "NATURE" :

HOW DR. MADDOX BLABBED OUT THE SECRET ABOUT THE GOAT'S EARS OF KING ALBERT

In a recent letter A. J. Bunting (Nature, **346**, 694, 1990) corrected the error of Dr. Maddox commented in my above note. Dr. Maddox made in this case a blunt student error, and I think that Nature is not the right place for discussing such *lapsus mani*.

However, in the second part of his comments Dr. Bunting makes an error which, although being also on a high-school level, must be discussed on these pages, as the whole horde of King Albert's subjects makes the same error.

Dr. Bunting's error has its roots in the following wrong statement of Dr. Maddox (see the figure in Dr. Maddox' paper):

> The objective is to measure the voltage across the sliding conductor, either by a standard voltmeter or by a condenser whose accumulated charge will be a measure of the voltage in a steady state.

Kennard's experiment can be done (and was done!) only if the measurement of the voltage is carried out in the second way. If the voltage over the slider $b - b_0$ will be measured by a "standard voltmeter", the latter must be connected to the end points of the slider by respective wires. Consequently the following three cases mentioned by Dr. Bunting will take place (rotation or rest of the double circular current loop is immaterial):

1) If moving the slider by keeping the voltmeter at rest, a tension will be measured.

2) If keeping the slider at rest by moving the voltmeter and the wires which connect it to the slider, the opposite voltage will be measured (this is the case of Dr. Bunting's "ships' log").

3) If moving slider and voltmeter together, no voltage will be measured (this is the case when Dr. Bunting will march with his bucket of water).

Stefan Marinov

Institute for Fundamental Physics
A-8010 Graz, Austria

Editorial note. See Marinov's letter to
Dr. Maddox of the 5 October 1990.

ANNALS OF PHYSICS

August 23, 1990

Dr. Stefan Marinov
Morellenfeldgasse 16
A-8010 Graz, Austria

Dear Dr. Marinov:

It is not possible for me personally to confirm or to find
fault with a presentation. We simply receive too many papers
for that to be possible. I iterate we cannot publish all
papers which are correct because of the limited space available
to us. We must select and we have decided not to publish your
paper. You must be aware of the major journals such as Nuclear
Physics and The Physical Review to which you can submit your
papers.

Yours truly,

Herman Feshbach

Herman Feshbach
Editor

HF/eos

<u>Editorial note</u>. With this letter Dr. Feshbach answers
Marinov's letter of the 3 August 1990.

Ref.62412

WORLD SCIENTIFIC PUBLISHING CO. PTE LTD

14 Sept 1990

Prof Stefan Marinov
Institute for Fundamental Physics
Morellenfeldgasse 16
A-8010 Graz
AUSTRIA

Dear Prof Marinov

RE: MODERN PHYSICS LETTERS A (MPLA)

I am sorry to inform you that your article entitled

Childishly Simple Experiment...

has not been recommended for publication.

Please find enclosed a copy of the referee's report.   I wish to
thank you for submitting your paper to our journal and hope that
we will be able to publish some further work by you.

Yours sincerely,   Editorial note. With this letter Ms.
                                          Chionh answers Marinov's
                                          letter of the 25.IV.1990.

E H Chionh (Ms)
Editor (MPLA)

REFEREE'S REPORT

TITLE: Childishly Experiment Violating the Principle of Relativity

Author: S Marinov

Ref: EPF/202/90

The nature of this paper is <u>not</u> suitable for publication in
Modern Physics Letters A.

International scientific, technical and medical publishers

Ref: SDB/PAD/D

26 September 1990

Dr S Marinov
Morellenfeldgasse 16
A-8010 Graz
AUSTRIA

IOPP

Dear Dr Marinov

TITLE: Repetition of Whitehead's experiment for ...
AUTHOR: S Marinov

Thank you for your submission to Journal of Physics D: Applied
Physics. Your paper has now been considered, and I regret that it
was found to be unsuitable for inclusion in this journal. I am
therefore returning your typescripts herewith.

I apologise for the delay in our response.

Yours sincerely

S D Byford
Senior Editorial Assistant
Journal of Physics D: Applied Physics

Editorial note. The above paper is published in TWT-VII, p. 26.

Marinov answers this letter with his letter of the
5 October 1990.

Budapest, 4th October 1990.

Dr Stefan Marinov
Morellenfeldgasse 16
A-8010 Graz, Austria

Dear Dr Marinov,

Thank you for your letters of 6 June and 13
August. As I told you in my previous letter we are
unable to publish your paper "New measurement of
the Earth's absolute velocity..." on account of
our Referee's comments.

We also regret we cannot find for your the
address or the paper of Karginov.

Your other paper "Action of constant electric
current..." cannot be published because the capacity
of our journal is fully occupied for a longer period
to come.

The manuscripts of both papers are returned
herewith.

Yours sincerely

Prof. I. Kovács
Editor

Incl.: 2 manuscripts,
      11 + 7 pages,
      3 + 1 figures

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

5 October 1990

Dr. S. D. Byford
Journal of Physics D
IOP Publishing LTD
Redcliffe Way
Bristol BS1 6NX

Dear Dr. Byford,

Thank you for your letter of the 26 September 1990, although, I must confess, the rejection of my paper

REPETITION OF WHITEHEAD'S EXPERIMENT...

was, of course, not pleasant for me.

As in the case of my previously submitted paper ("Drag-of-light" experiments), you have not presented motivations for the rejection.

I submit now a third paper entitled (PACS 41.10)

VERY EASY DEMONSTRATION OF THE VIOLATION OF THE ANGULAR MOMENTUM
CONSERVATION LAW AND OF THE FAILURE OF CONVENTIONAL ELECTROMAGNETISM.

I beg you, however, in the case of rejection to present motivations.

You certainly will object that the paper is not from the domain of applied physics, but from the domain of fundamental physics. Well. I have submitted this paper to the following physical journals: PHYS. REV. LETT., INT. J. MOD. PHYS., NUOVO CIMENTO, CAN. J. PHYS., ANN. DER PHYS., J. FRANKLIN INST., FIZIKA, PROC. IEEE, SPEC. SC. TECHN., GALILEAN ELECTRODYNAMICS. It was rejected by all of them. Quite all rejection letters, referees' comments, my objections and referees' counter-objections are published in the different volumes of my series THE THORNY WAY OF TRUTH (TWT). If you would like, I can submit copies of the WHOLE corresponence with these journals.

Almost all referees of the above journals accept that my Bul-Cub machine without stator WILL ROTATE , but they do not fall on their knees before this WONDER, as according to all of them the "opposite" angular momentum is "taken by the electromagnetic field", and thus there is no violation of the angular momentum conservation law. Thus according to all these referees my machine is not worth the paper on which its description will be presented.

I hope that you, as an editor of a journal on APPLIED PHYSICS, will remain amazed that I bring into rotation a body of about 2 kg which is suspended on an axle, without acting to it by external forces, and you will agree that this TREMENDOUSLY IMPORTANT EXPERIMENTAL FACT is to be communicated to the scientific community.

In the case that you will reject the paper, I beg you (or your referee) to answer the following three questions:

1. Will, according to you, the Bul-Cub machine without stator rotate when alternating current will be conducted to it along a vertical wire coming from infinity and going to infinity?

2. If the first question will be answered positivey, will, according to you, the angular momentum conservation law be violated?

3. If the second question will be answered negatively, where, according to you, will be "stocked" the opposite angular momentum?

I shall NOT accept a rejection of my paper, if these questions will be not answered.

I resubmit also my paper REPETITION OF WHITEHEAD'S EXPERIMENT..., because only by realizing that the polarization current is NO   current (my repetition of Whitehead's experiment shows THIS), and by taking into account the well-known THEORETICAL conclusion that Newton's third law is NOT valid at the interaction of UNCLOSED loops, one easily understands WHY my Bul-Cub machine without stator DOES ROTATE.

Enclosed is the paper which Dr. J. Maddox published on the Kennard's experiment considered in my paper VERY EASY DEMONSTRATION... and my comments which Dr. Maddox promises to publish, but instead he published the stupidity of A. J. Bunting (NATURE, **346**, 694).

Hoping to receive your acknowledgement for reception and then in due time also your decision,   Sincerely yours: *S. Marinov*
Edit. note. There was NO answer!

STEFAN MARINOV
Morellengasse 16   5 October 1990
A-8010 GRAZ — AUSTRIA

Tel. 0316/377093

Dr. John Maddox
NATURE
4 Little Essex Street
London WC2R 3LF

Dear Dr. Maddox,

I write you just after having spoken with you. I said to you that your English is bad and I repeat this statement. You must begin to construct your sentences in the clear way in which I construct my English sentences. Only if writing so, you will be well understood by your readers. I have, however, to note, that your bad English construction result to a certain degree from:

1) your wrong physics concepts,

2) your bad habit to write lies.

If you cannot correct the first source of your bad linguistic presentations, then I BEG YOU VERY MUCH, evade to take water from the second source.

I hope that you will not become angry by reading this letter. I am not exploding, Dr. Maddox, I am a calm man. But I think it is time to stop your play with the wrong promises and eternal deceptions. If you wish to maintain contact, then fulfil your promises. If you don't wish, write this clearly and I shall no more bother you every second day on the phone.

I showed by theory and experiments that space and time are absolute categories. I showed by theory and experiments that the laws of conservation can be violated. Finally there is a FUNCTIONING PERPETUUM MOBILE in Switzerland. I need only space to publish this information in a widely read journal, as my publications have a limited circulation.

You will object that the readers of NATURE are too stupid to understand my papers. Well, Dr. Maddox, I agree with you that the readers of NATURE (as Dr. Bunting) are stupid, but I definitely don't agree that they are idiots. And I write in such a manner that ALL readers of NATURE will understand WHAT have I done. I need only space.

I have a question. Can I buy TWO PAGES in every issue of NATURE which will be entitled MARINOV'S PAGES. I shall pay you EVERY week 1000 £ for these two pages and I shall print there only scientific matter written by me and by other persons. You will correct only my English. If you would agree, phone me, so that we can sign a contract. Then you shall see what a song will Bondis and Wheelers begin to sing.

I give apart an additional note[*] which is to be added to my Letter to the Editor "HOW DR. MADDOX BLABBED OUT THE SECRET ABOUT THE GOAT'S EARS OF KING ALBERT".

I shall phone you on Monday to learn whether you have sent to me the promised HISTORICAL letter.

* See it on p. 304 of this volume.

With love: *Stefan Marinov*

Stefan Marinov

PS. Dear Dr. Maddox, I have the following request: Give to your wife your article, the enclosed LETTER of S. A. Hayward, my comments to your article and my comments to the letter of Dr. Hayward and ask her, whose English is the better. Then, if you are a honest persron, tell me her verdict.

FIVE YEARS TO HAMMER IN ALL YOUR HEADS THE MOST SIMPLE AND OBVIOUS TRUTH!!!!

FIVE YEARS!!!!

<u>Marinov's note.</u> The letter of Dr. Hayward and my comments are published in TWT-III, pp. 191 and 198.

Marinov

SIR—I wish to state that the diagram ~~kxxxxxxxxx~~ by John Maddox (*Nature* 346. 103; 1990) is not a "conundrum", but the diagram of an experiment carried out by E. H. Kennard (*Phil. Mag.* 33, 179: 1917). The result*s* of Kennard's experiment (which I call the rotational Kennard experiment) are the same as predicted by my absolute space-time theory. When the loop consisting of two concentric circles is transformed into an elongated rectangle, I call this the inertial Kennard experiment.

reproduced /

˜s ʌ

Variations of the inertial Kennard experiment (when moving the wire with respect to the loop and when moving the loop with respect to the wire) have been carried out by Francisco Müller and published in ~~HWT VH~~ (p. 319), noting that Müller ~~fusing~~ a flat permanent magnet instead of a loop: see *Galilean Electrodynamics* 1, 27 [1990].

s)

my book <u>The Thorny way of Tru</u> *used / V. II —*

˜s/s)

I carried out the inertial Kennard experiment for the case when wire and loop move together and succeeded in measuring the Earth's absolute velocity *with an* with the electromagnetic experiment. I submitted a report on this experiment to *Nature*, but it has not been published.

Thus the problem is not to give answers to a conundrum, as the answers are given by Nature itself (I do not mean the journal *Nature*), but to recognize the absolute character of the electromagnetic phenomena and the failure of the principle of relativity.

Concerning the perpetual motion à la *imshurst* Wimhurst machine mentioned at the beginning of Maddox' article, the truth is the following: the name of this machine is *STATIKA* TESTA-TIKA, its inventor is the spiritual head of the Christian community METHERNITHA in the village Linden, 30 km south ~~wards from~~ Bern ~~Paul Baumann~~ My machines violate the laws of angular momentum and energy conservation, but I have not been able ~~to I could not succeed~~ to close the energetic circle *ome of* and to run them eternally.

˜s ʌ

Paul Baumann, /

≡ of / o)

of / o)

s/

V

STEFAN MARINOV
Institute for Fundamental Physics.
A-8010 Graz. Austria

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA

6 November 1990

Dr. John Maddox
NATURE
4 Little Essex Street
London WC2R 3LF

Dear Dr. Maddox,

You deceived me three times in three consecutive days. You said me thrice that you have dispatched the proofs of my comments on your "conundrum", and I vainly awaited for them. Finally, after phoning to Miss Sheehan, it became clear that the proofs have not been sent. Miss Mary promised to do this, and in two days the proofs arrived.

You have not corrected my "bad" English, as you said on the phone that you have to do, as all sentences which remained in the text were written exactly as they were written by me. No, Dr. Maddox, you have not corrected "my English", you have MUTILATED my comments, omitting the text where I pointed out all lies which you piled up in your "conundrum".

The shortening of my comments has not been dictated by the necessity of saving place, as you know pretty well that any line written by my pen is 1000 times more worth than 1000 lines in any of the stupid papers printed in NATURE. I present PROOFS on the violation of the principles of relativity and equivalence, I present PROOFS on the violation of the laws of conservation. Finally I try desperately to bring to the attention of the scientific community the information on the first perpetuum mobile constructed on this planet. When TESTATIKA will begin to supply electricity to the first car on the world's highways, Saddam Hussein and the Golf crisis will disappear as a nightmare does with the first rays of the sun. Why you go out of your skin to stop the spreading of this information? - Is it true that I promised to pay you Ł 10,000 if after visiting TESTATIKA you will declare on the pages of NATURE that it is NOT a perpetuum mobile? Yes, it is true. Why then you lie that I have brought this machine to London and it did not work? And why you do not wish to recognize this lie on the pages of NATURE? You have to publish my comments and then you have to excuse yourself for having printed a lie. ONLY IN THIS WAY THE INFORMATION ON TESTATIKA WILL REACH THE WORLD.

Look, Dr. Maddox, you are much more clever than the editors of the other physical journals. It became clear for you that relativity has lost the battle and that I am the winner. Join my camp. Give a kick to the stupid theory of relativity. At the present time you have to do nothing else than to publish my contributions. Don't make comments, only publish. Publish then the criticism which the "relativists" will submit, although, I repeat once more (see my letter to you of the 23 July 1990), none of the "relativists" will dare to critisize me, as all of them will be afraid to ridiculize themselves. By publishing my contributions your authority and the authority of NATURE will raise quickly up. The first step which you have to undertake in this direction is to publish the integral text of my comments to your "conundrum" and the letter "Relativity and Electromagnetism" which since about a year you promise to publish.

But if you do not wish to win renown and you intend to remain a poor servant of a party which has lost the battle, then publish the mutilated version of my comments. I do all to help you and to save you from the relativity quagmire. But one cannot save someone against one's own will.

Enclosed is the introduction to the eighth volume of TWT which will go to print in a couple of days (together with this letter). Thus, at the end of November TWT-VIII will be on sale in all scientific bookstores in London. As you can read in this introduction, I DEFEND you against the attacks of people who do not know WHO ARE YOU.

I am still expecting the Ł 100 which you owe me since more than 6 months and I am looking forward for your answers to all questions posed in my letters of the 27.II. and 21.III.90.

With love:

S. Marinov

Stefan Marinov

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

6 November 1990

Dr. Petr Beckmann
GALILEAN ELECTRODYNAMICS
Box 251
Boulder
CO 80306

Dear Prof. Beckmann,

To my letter to you of the 2 August 1990 I have not received an answer. Please, be so kind to inform me whether this letter has reached you, and which are your decisions.

Now I SUBMIT to your journal the following contributions:

1) The preface to my book TWT-VIII, of which, of course, you have to cancel the last sentence on p. 1 and the whole p. 2.

2) The text "Marinov" which, I hope, will appear in NATURE and you have to copy the printed text from this journal, after its publication. If at the time of publication of GALILEAN ELECTRODYNAMICS this text will not appear, you have to recompose it.

3) My letter to Dr. Maddox of the 6 November 1990.

4) My Letter submitted to NATURE entitled "How Dr. Maddox blabbed out the secret about the goat's ears of King Albert".

5) My letter submitted to the Editor of Am. J. Phys. entitled "Relativity and Electromagnetism".

I beg you to publish all these materials in the next issue. If you will do this, your journal, surely, soon will become a world's scientific focus.

Hoping to receive your answer soon,

Sincerely yours,

J. Marinov

Stefan Marinov

PHYSICS LETTERS A

13/11/90

Dear Dr Marinov

We enclose a report on your paper V1607. Please furnish any response in triplicate.

Yours sincerely,

J.P. VIGIER

P.R. HOLLAND

Editorial note. Marinov answers the above letter with his letter of the 20.XI.90.

REPORT ON THE PAPER ENTITLED

"ON THE ELECTRIC INTENSITIES INDUCED IN RAILGUNS

BY S. MARINOV.

The present referee will restrict his comments to one point, on the basis that each link in the chain of the argument should hold true. Equation (1) gives the force acting on a unit charge, viz.

$$\underline{E} = - \operatorname{grad} \phi - \partial \underline{A} / \partial t + \underline{v} \times \operatorname{curl} \underline{A}$$

This equation is consistent with an electric field given by

$$\underline{E} = - \operatorname{grad} \phi - \partial \underline{A} / \partial t$$

At least for v<<c. This is entirely correct, but the argument is not developed further. It is true that the electric field is partly due to the changing magnetic field, and partly due to the position of the charges in space.

STEFAN MARINOV
Morellenfeldgasse 16
A-8010 GRAZ — AUSTRIA
Tel. 0316/377093

20 November 1990

Prof. J. P. Vigier
PHYSICS LETTERS A
Institut H. Poincare
11 Rue P. et M. Curie
F-75231 Paris Cedex 05

Dear Prof. Vigier,

Thank you very much for your letter of the 13 November 1990 and for the enclosed refe ree's report on my paper V1607. I wish, however, to note that my paper was submitted on the 6 August and you send me the referee's comments (which have been written in some fi minutes) more than three months later. A journal for rapid publications must proceed qu cker. On the other hand, as you are a capacity in the field and as you publish actively papers on the Ampere-Grassmann controversy, I think, that you had to take the responsib lity to be a referee of my paper.

The equation giving the force acting on a unit positive electric charge, which I cal GLOBAL ELECTRIC INTENSITY,

$$(m/q)\mathbf{u}_0 = \mathbf{E}_{glob} = - \text{grad}\phi - \partial\mathbf{A}/\partial t + \mathbf{v}\times\text{rot}\mathbf{A}, \tag{A}$$

where m is the mass of an electric charge q moving with a velocity $\mathbf{v}$ and acquiring an acceleration $\mathbf{u}_0$ (FIRST PROPER ACCELERATION) when crossing a reference point where the el tric and magnetic potentials of the surrounding system of electric charges are ϕ and A, is the well-known Lorentz equation, which I call the NEWTON-LORENTZ EQUATION, especiall when it is written in the "Newtonian" form

$$(m/q)\mathbf{u}_0 + d\mathbf{A}/dt = - \text{grad}(\phi - \mathbf{v}.\mathbf{A}), \tag{B}$$

which clearly shows that only the FULL KINETIC FORCES of two interacting charges (see the left part of equation (B)) are equal and oppositely directed quantities but the sim KINETIC FORCES (see the left part of equation (A)) are NOT.

The referee writes that equation (A) is "consistent with an electric field"

$$\mathbf{E} = - \text{grad}\phi - \partial\mathbf{A}/\partial t. \tag{C}$$

An "equation" can be not consistent with a "field". An equation can be consistent wi another equation. It is obvious, and any student in the MIDDLE schools knows, that the electric intensity defined by the equation (C), which is the sum of the COULOMB ELECTRIC INTENSITY and the TRANSFORMER ELECTRIC INTENSITY, i.e.,

$$\mathbf{E} = \mathbf{E}_{coul} + \mathbf{E}_{tr} = - \text{grad}\phi - \partial\mathbf{A}/\partial t, \tag{D}$$

gives only a part of the force acting on the unit test electric charge put at the refe- rence point; the other part is given by the MOTIONAL ELECTRIC INTENSITY

$$\mathbf{E}_{mot} = \mathbf{v}\times\text{rot}\mathbf{A}. \tag{E}$$

What is here more to discuss? Where the "argument" is to be developed further? Why th referee blocks the discussion of the scientific problems by beginning to discuss banali ties?

The referee then writes: "It is true that the electric field is partly due to the cha ging magnetic field, and partly due to the position of the charges in space."

The electric field (i.e., a PART of the force acting on a unit electric charge) is du NOT to the time change of the "magnetic field" (i.e., of the magnetic INTENSITY B) at th reference point crossed by the test charge, but to the time change of the magnetic PO- TENTIAL A at that point. I shall give the most simple example, so that the referee will IMMEDIATELY agree with me (if he is a HONEST person), and will give a kick to the wrong concepts of conventional electromagnetism.

If there is a very long coil along which alternating current flows, the change in ti of the "magnetic field" (i.e., of the magnetic intensity, B) is different from zero onl IN the coil and it is equal to zero OUTSIDE the coil. Thus, if we wind a secondary coil over the primary coil, according to the above statement of the referee (and according to conventional electromagnetism), an induced current can be NOT observed in the secondary coil. However, any child knows pretty well, and there are milliards of transformer in th world which support the children's prediction, that an induced current IS observed in the secondary coil. Why is this current induced? - Because the time change of the magnetic POTENTIAL A outside the primary coil is DIFFERENT FROM ZERO. (The people who assert that the magnetic potnetial can be "observed" only in   quantum physics by the help of the

Bohm-Aharonov effect are either blind or liers or both.)

The electric field (i.e., I repeat, a part of the force acting on the unit test charge) is not "partly due on the position of the test charge in space". It is due to -gradΦ at the reference point and to $-\partial A/\partial t$. And on nothing else. To obtain the global electric intensity, i.e., the global force acting on the test charge, we have to add also $v \times rot A$. And the problem is closed. There is nothing more to discuss.

Of course, the referee (as well as conventional physics) is free to call the sum of E_{coul} and E_{tr} electric field and $B = rot A$ magnetic field. However, the less will he use these notions in his writings and mental speculations, the better for him. If one wishes to understand electromagnetism, one has to forget for a while the notions intensities (i.e., fields) and operate only with the notions potentials. But if the referee (and conventional physics) do not wish to operate with the potentials and try always to make calculations with the intensities, this is THEIR affair.

I developed my absolute space-time theory and gave description of crucial experiments carried out by me and by other persons in 15 (fifteen) books and in numerous papers. About 50 papers have been published, but other 50 papers go from one journal to another, receiving, on an average, 20 rejections each. The sequence of documents THE THORNY WAY OF TRUTH, of which now the eighth volume is published (every volume of more than 300 pages) gives the proof of my assertions.

The problem now is not to show the logical inconsistency of the IDIOTIC theory of relativity and the wrong concepts of the Maxwellians. The problem, first of all, is to inform the scientific community about the experiments carried out by me and by other persons which contradict the PREDICTIONS of the Maxwell-Einstein concepts. The rejection of these theories will follow then AUTOMATICALLY.

I submitted so many EXPERIMENTAL papers to PHYSICS LETTERS A, beginning some 20 years ago when Dr. ter Haar was the editor. After 7 or 8 publications, all my following papers have been SYSTEMATICALLY rejected. The arguments which have been presented were always on the POOR level on which the arguments of the present referee are.

The scientific community must finally become aware that the principle of relativity (and the principle of equivalence, too) and the laws of energy and angular momentum conservation have been disproved by my EXPERIMENTS. The scientific community must become aware that Newton's third law can be violated and that the displacement current is NO current.

Dear Prof. Vigier, I am addressing you with the question: Can I again submit ALL papers which you have rejected in the last years, basing your rejections on opinions of referees which are mathematical invalids?

If you will say "No", you will prolong the agony of relativity and of Maxwell electromagnetism and the life of physics based on the laws of conservation for some months, maybe or 1-2 years. But for no more! Also people will continue to write papers in favour of Ampere's formula describing the interaction of current elements, although my experiments with incomplete circuits showed that this formula is WRONG. Why fill the journals with unnecessary stuff?

Instead to agree to accept for examination ALL MY REJECTED papers, you, of course, can reject also the present paper V1607.

I can do nothing more against your decisions as:

Le pain, le couteau, ainsi que notre sueur
sont dans les mains de bourreau, appelé EDITEUR.

Avec mes meilleurs sentiments,

Stefan Marinov

GALILEAN ELECTRODYNAMICS
Box 251
Boulder, CO 80306
tel. 303-444-0841

November 16, 1990

Dear Dr Marinov:

I have received your letter of Nov. 6.

Formally speaking, none of your proposed contributions qualify for publication in this journal, because like all scientific journals, it publishes original papers only, and all five items have been submitted for publication elsewhere, with some of them already published.

That is all I would have to answer, but let me continue on a personal level. Judging from some of your writings, I think you are very talented, but the form of your papers is, to say the least, bizarre. You miss no opportunity to name things after yourself (not only tasteless, but also suspicious, because if something is really good, it is given a name by *others*); you engage in personal attacks and ridicule; you choose gutter press headlines for scientific papers; you end what is to be a scientific paper with ``Ignorant criminals!''; and more.

I will be happy to have a good and serious scientific paper reviewed, and if recommended for publication, I will print it, even though I do not hold the same opinions in physics as you do -- as you have seen the papers in GE have not been limited to my own theory. But such a paper would have to have a serious title, contain an integrated derivation without holes to be filled by referring to obscure and effectively unavailable books or journals, be void of any personal attacks or denigrations, not contradict the experimental evidence (or at least explain convincingly why the laws of thermodynamics may not hold -- your ``perpetuum mobile machine''), be void of any personal self-aggrandizement by naming things after yourself -- in short, it would have to be a regular scientific paper as in all other journals.

None of the papers you have sent me so far meet these very elementary criteria. If you write a good paper that does, I will be happy to have it reviewed and publish it if recommended. But if you send me letters and little bits and pieces that have already been submitted or published elsewhere, assuring me of ``becoming a world's scientific focus'' if I print them, you are wasting my time, of which I have absolutely none to spare, and I will leave them unanswered.

I would, however, much prefer the former alternative.

Yours sincerely,

Petr Beckmann

Petr Beckmann
Editor

STEFAN MARINOV
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Dr. P. Beckmann
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Dear Dr. Beckmann,

Thank you very much for your letter of the 16 November, as well as for Nr. 6 of GE which arrived a day earlier.

By publishing his conundrum (although filled with lies), Dr Maddox offered an excellent possibility to the supporters of the absolute space-time concepts to "win the play", in the case that this "conundrum" will be largely discussed in the press, as the rotational and inertial Kennard experiments are lethal for relativity - logically invincible and experimentally childishly simple. The way in which Dr. Maddox deformed my comments on his "conundrum" will certainly not give rise to a world-wide discussion. I offered you the possibility to open this discussion on the pages of GE and to make thus your journal a "world's scientific focus". You denied. I can only say: "ŠKODA" (Czeque: ALAS).

I thank you very much for the advices which you give me respectively to the names of the machines invented by me and the formulas introduced by me in the theory. You find the names given by me as tasteless. Perhaps you are right, but you know that it is senseless to argue about tastes. I send you an article published in FOUND. PHYS.*, where I defend the name of the Marinov transformation. But you write (and I thoroughly agree) that it will be senseless to discuss such kind of problems in our correspondence.

Following your suggestions, I submit now to GE my paper

REPETITION OF WHITEHEAD'S EXPERIMENT FOR DEMONSTRATING THAT DISPLACEMENT CURRENT
IS A PURE MATHEMATICAL FICTION.

Some remarks to the paper. (Ed. note: published in TWT-VII, p. 26):

1) The captions of §§ 2 and 4: The word "Marinov's" can be substituted by "Author's".

2) The "obscure" and "unavailable" references 1 and 3 can be easily dropped out. I shall do this, if you will require it, in the case that the paper will be accepted for publication, as otherwise I have to rewrite the whole paper (I always submit neatly written papers). I should like only to note that every book which is on free sale cannot be "obscure". If you, however, mean the content of this book, it will be better to give opinions after having seen the book. Finally, the 15 books on physics published by me can be obtained in a week by everybody who will send my $ 25 for a volume.

3) The reproductions of Whitehead's drawings are not good, although I did my best when photocopying them. Of course, I can redraw them, but, I think, we have to give to the readers of GE the original drawings.

4) The photograph will be submitted in the case that the paper will be accepted for publication. On the other hand, I see that you do not print photographs in GE. But I think, you have to.

I hope that the paper will satisfy your requirements. If you would suggest some other changes, you can introduce them, without asking for my permission, as I steem you highly and I have a complete confidence in you. I accepted even Maddox's mutilation of my comments, as this was the only way to appear FOR A FIRST TIME in NATURE, after so many articles written ON ME there.

I send you information on the discussion of anti-relativity in the Soviet Academy of Sciences. I think, it will be very good, if you will inform the readers of GE about this denigration, as Ginsburg's "враг науки" is very near to Stalin's "враг народа". Alexandrov is the ex-President of the Academy. Ginsburg is the leading Soviet theoretician. As in SU the whole system is swaying, the relativists there are much more afraid for their positions than in the West. (See this material on p. 10 of this volume.)

I hope to meet you in Pulkovo at the conference in September 1991.

I repeat, if you would like to publish information on Bartocci's conference in June on Ischia, write me. Maybe, we can meet at both conferences.

Tolchelnikova wrote me that Wigner has expressed the desire to become member of the scientific committee of the Pulkovo conference.

Yours, *[signature]*

*FOUND. PHYS., **13**, 1241 (1983).

EPILOGUE

It is since about a year that Dr. Stefan Naidenov, head of the Laboratory BIOELEC-TRONICA, Sofia, Bulgaria, has found a way to "burn the water" by adding to it a minimal quantity of a liquid which is his own invention and SECRET.

Dr. Naidenov has demonstrated burning of water at the international Ecoforum (Sofia, September 1989). Then he made the same demonstrations on the Bulgarian TV.

In September 1990 I visited twice Dr. Naidenov in his laboratory and he acquainted me in general with his invention. By adding a special green liquid, he dissosciates the hydrogene atoms from the oxygene atom and then burns the hydrogene.

According to Dr. Naidenov his invention has common points with the invention of the Portuguese Andrews (I do not know how exactly the name must be spelled) in 1913 which then was "rediscovered" by Guido Frank in Canada after WWII (Dr. Naidenov has an abundant documentation on these two inventions).

Dr. Naidenov showed me only a video. By adding some cubic cm of his green liquid in a liter of water he drove a Volga-car as long as by 1 l of benzine. By putting the same greenly water in a burner, Dr. Näidenof produced a flame about 1 m long and some 20 cm thick.

Dr. Naidenov is ready to sell his discovery to any country, compagny or person who will pay him $ 50,000,000,000.

Abundant information on Dr. Naidenov (who turned out to be my student colleague) will be published in my book "Изыди, Сатана!", Part II, which will soon be published in Sofia.

People interested to buy Dr. Naidenov's invention for the above mentioned sum can contact me, so that I introduce them to Dr. Naidenov.[*]

<div align="right">Stefan MARINOV</div>

[*] I ask 0.001% of the sale sum for the mediation.

C O N T E N T S :

The eighth part of the collection of documents THE THORNY WAY OF TRUTH (TWT) ist dedicated quite the whole to the Ampere-Grassmann controversy, i.e., to the problem whether the interaction between two current elements is to be described by the Ampere (1823) or Grassmann (1845) formula. The first one is in conformity with Newton's third law, while the second one violates it. Although Marinov's experiments with unclosed circuits violated Newton's third law and thus definitely resolved the controversy in favour of Grassmann, further investigations are needed for clearing the basic problem about the interaction between moving charges. Marinov's theoretical and experimental research is presented which leads to the conclusion that the pushing force which acts on the Ampere bridge can be not calculated and the only way for establishing its magnitude is the experimental measurement. This conclusion offers a highly interesting and instructive illustration on the touching points between mathematical abstraction and physical reality and on the "astuteness" of Nature in hiding its weaknesses. Another big problem to which the present volume is dedicated is the violation of the energy conservation law in plasma physics. Marinov is photographed above when inspecting one of the plasma converters of the Bulgarian scientist Cyril Chukanov, the thermal output power of which superates the electric input power. Although the measurements are not still conclusive, Marinov intends with this presentation to turn the attention of the scientific community to this topic; many other scientists purport the same claims, as the Russian plasma-physicist Alexander Chernetski, on whose over-unity plasma converter information is also given.   Price: $ 25