International Centre for Theoretical Physics

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The First Edinburgh Medal to Professor Abdus Salam

On Saturday, 8 April, Professor Abdus Salam, Nobel Laureate 1979, Director of the International Centre for Theoretical Physics and President of the Third World Academy of Sciences, received the first Edinburgh Medal, awarded as part of the City's first Science Festival.

In his Edinburgh lecture delivered at the award ceremony, Professor Abdus Salam called for an International Centre for Science in Scotland open to the Third World, a suggestion which has found a favourable ground. Howard Firth, the Director of the Edinburgh Science Festival, hopes to do for science what the Edinburgh Festival has done for the arts[1].

The alumni and staff of the ICTP and TWAS join in congratulating Professor Abdus Salam for this new international award.

Edinburgh Medal Struck to Recognize Scientific Achievement

Professor Abdus Salam is First Recipient

by courtesy of
The City of Edinburgh District Council

The City of Edinburgh has created a major international award, to be known as the Edinburgh Medal, which will be presented once a year to a distinguished scientist. The recipient will deliver the annual Edinburgh Lecture under the general theme of "Science and Society".

The Edinburgh Medal was presented to the recipient during the annual to the recipient during the annual Edinburgh International Festival of Science and technology, the first of which was held in the city from 3rd to 12th April this year.

The Edinburgh Medal for 1989 has been awarded to Professor Abdus Salam, the physicist and Nobel Prize winner, who is Professor of Theoretical Physics at Imperial College, London, Director of the International Centre for Theoretical Physics and President of the Third World Academy of Science, both in Trieste (Italy).

The award was presented to Professor Salam by the Lord Provost of Edinburgh, the Rt. Hon. Eleanor McLaughlin.

The Science Festival was originally conceived by the City of Edinburgh District Council, and the Edinburgh Medal lecture will provide a platform for a scientist of world stature to speak on a topic of outstanding importance to science and the international community.

Professor Salam, who is regarded as one of the world's greatest scientists, received the Nobel Prize for physics in 1979 for his work in unravelling the structure of the forces involved in radioactive decay, the so-called 'weak interaction'. He has been involved in a series of breakthroughs in linking up the fundamental forces of the universe – gravity, electromagnetism, the weak interaction and the 'strong interaction' – that hold together the atomic nuclei.

Professor Salam who comes from Pakistan, has also worked for many years to developing opportunities for scientists in the Third World. These efforts led to the establishment of the International Centre for Theoretical Physics at Trieste, of which he is Director. Scientists from developing Director. Scientists from developing countries visit the Centre for contact and stimulus in research. Professor Salam is Chairman of UNESCO's Advisory Panel in Science, Technology and Society.

His work on the theory of the 'electroweak force' builds upon the idea of the great Edinburgh-born physicist, James Clerk Maxwell (1831-79), who united electricity and magnetism into the concept of the electromagnetic field. Professor Salam successfully brought the weak interaction – involved in radioactive decay – into the same overall pattern as electromagnetism.

Lord Provost Eleanor McLaughlin said to-day: *"The City of Edinburgh, which has so any historic connections with scientific advance, is now offering*

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[1] Mr. Howard Firth and two of his collaborators – Mr. Ron Webster from the UK Atomic Energy Authority, and Mr. Simon Armstrong from the Highlands and Islands Development Board – visited the ICTP on 2 and 3 May.

the Edinburgh Medal as a way of honouring men and women who have made an outstanding contribution to world science in our own time, and providing them with an international platform from which to address the world."

Mr. Howard Firth, Director of the Edinburgh Science Festival, said: *"Professor Salam is one of the world's outstanding thinkers, whose knowledge ranges from the frontiers of modern physics to the historic influences of Islamic scholarship on Western developments in mathematics and medicine. He is a man of global vision, whose work for international scientific co-operation deserves the widest support, and sets a standard for us all to follow."*

Scientific Centres Needed to Help Raise Standards in the Third World

by courtesy of
The City of Edinburgh District Council

Centres of scientific excellence, with a special mandate to train scientists from the Third World, should be set up in the industrialised nations, said Professor Abdus Salam, the physicist and Nobel Prize winner, on Saturday, 8 April. He suggested Edinburgh as the home of one such centre.

Professor Salam was delivering the first annual Edinburgh Lecture, in the Signet Library, Edinburgh, after having been presented by Lord Provost Eleanor McLaughlin with the Edinburgh Medal. The Medal has been founded by the City of Edinburgh, and will be presented annually to a distinguished scientist during the Edinburgh International
during the Edinburgh International
Festival of Science and Technology. The Medal is accompanied by an award of £5,000.

Professor Salam told a distinguished audience that the current situation in the developing world was so bleak that, without the active involvement of the international scientific community, it would not change. The crucial point must be a realisation by the world scientific community that here was a sphere where they could directly help and were expected to help.

Centres of scientific excellence would have to be created before developing countries could begin to enter into the spirit of a true scientific revolution. The problem was that such centres could not be created by the developing countries unaided.

Professor Salam played a prominent role in the creation of the International Centre for Theoretical Physics, which was set up in Trieste in 1964 by the International Atomic Energy Agency, the Italian Government and other bodies. Since 1964 the Centre has been attended by 36,000 scientists, 20,000 of whom were from developing countries. Some 4,000 scientists come every year. There is also another UN institution in Trieste for genetic engineering and there are plans to add three new components – for high technology, chemistry and earth sciences.

Professor Salam continued: *"I suggest that new Centres for Science should be created, particularly at Edinburgh and in Scotland with a special mandate to help the countries of the South, and also of course to help local Scottish industry in extending its hospitality and bettering itself. This is something that has happened in Trieste, where a number of new endeavours have opened up because we (the International Centre for Theoretical Physics) happened to be there. For example, the Synchrotron Radiation Laboratory has been set up, and a Research Area has been constructed on the basis of what we have been doing. I would like to suggest an institution which might be created in Scotland to be very closely connected with the one in Trieste."*

Stating that the Italian Government had promised 10 million dollars for each of the three new centres to be constructed in Trieste, Professor Salam said: *"I would like the Edinburgh Centre to be even more ambitious and start with 10 million pounds rather than 10 million dollars. It can take care of high*
dollars. It can take care of high
technology, for which there is need for hundreds of institutions."

Pointing out the problems faced by the developing countries of the South, Professor Salam said the industrialised countries were spending (in Gross National Product terms) seven to nine times more every year on science and technology than the Third World. *"We in the Third World are just not serious about science and technology: the profession of science and science-based technology is hardly a respectable or valid profession in the South."*

What could we do about this?

1. Ten per cent of the aid funds should be earmarked for science and technology. (This, he said, would amount to 3.5 billion dollars, and 10% of the world's spending on science and high technology could, without doubt, transform the South).
2. It should be considered as part of the birthright of scientific communities in a developing country that the country should have at least one complete central science library, containing all science journals and all scientific books. Arrangements should be made with publishers in the North that such books and journals be made available at a fraction of their present price. At least 50 developing countries could make use of this literature right away, he said.
3. There was a need to build up scientific infrastructure in developing countries. This could be done through co-operation among United Nations agencies, each contributing in disciplines relevant to their competence.

Professor Salam also said that an Associateship scheme, operated by his International Centre for Theoretical Physics at Trieste, had worked superbly as a device to counter the 'brain drain'. Associate members were scientists working in the developing countries, and each of these scientists was entitled to a number of visits. There were at present 319 Associate scientists from 62 nations. Out of 18,000 visits by these physicists, not one had departed from his own country through the operation of the Trieste centre.

Dirac Award Ceremony

The ICTP Spring School and Workshop on Superstrings (3 - 14 April
Workshop on Superstrings (3 - 14 April
1989) provided the opportunity to present David Gross – Eugene Higgins Professor at Princeton – with the 1988 Dirac Medal awarded to him in August last year. As one will recall, the second 1988 Dirac Medal went to E.S. Fradkin from the Lebedev Institute in Moscow. The ceremony took place on 10 April 1989 in the large lecture hall of the ICTP Main Building. Professor Abdus Salam, Director of the ICTP, and Professor S. Lundqvist, Chairman of the ICTP Scientific Council, introduced D. Gross to the audience and Prof. A. Zichichi from CERN, President of the Ettore Majorana Centre for Scientific Culture (Erice, Sicily) and of World Lab and also a Member of the ICTP

Scientific Council, presented the Medal, after which D. Gross gave a beautiful lecture on physics and mathematics at the frontier which is published in this issue.

There are numerous stories about P.A.M. Dirac. Prof. Zichichi recounted one.

As a student, Zichichi was disturbed with the idea that Dirac had suggested that the proton as the anti-particle predicted by his equations, before the positron was discovered. Twenty years later, Zichichi met Dirac and asked him why he made this suggestion since the mass of the proton is so different from that of the electron. Dirac told him that in those days when he was a young physicist, there used to be a seminar every week which was also attended by Piotr Kapitsa, a student of Rutherford's and Nobel Prize for Physics 1978. Each time Kapitsa appeared in the lecture room, he would ask: "Paul, where is the anti-electron?". Dirac answered, to confuse him in his proverbial laconic way: "That is the proton!".

A moment of the ceremony – (from left to right) Prof. D.J. Gross, Professor Abdus Salam and Prof. A. Zichichi. In the background, a portrait of P.A.M. Dirac.

Physics and Mathematics at the Frontier

by
David J. Gross,
Princeton University

This talk is largely based on a paper delivered at a symposium of the National Academy of Sciences and published in Proc. Natl. Acad. Sci. USA Vol. 85, pp. 8371–8375, 1988.

1. Introduction

Paul Dirac, one of the great pioneers and heroes of quantum mechanics, believed strongly in the deep connection between mathematics and physics. More than most, he believed that mechanics, believed strongly in the deep connection between mathematics and physics. More than most, he believed that the exploration of mathematical structures could, by itself, lead to the discovery of new and true physics. In this, as in many other things, Dirac was a pioneer. The pursuit of theoretical physics today, as never before, is in accord with Dirac's view of the deep and fruitful connections between fundamental physics. I therefore feel that it is particularly appropriate for me to discuss today, upon receiving the Dirac Medal, the interaction between physics and mathematics. I shall restrict myself to the field of elementary particle physics. Here, in the exploration of the fundamental laws of nature, mathematics and physics have had the longest union and the most fruitful exchanges.

I should, however, qualify my use of 'fundamental', a buzz word that might raise the hackles of my colleagues who object to statements that imply that one field is more fundamental than another. By fundamental I do not mean supreme, preeminent or dominant but rather basic, elementary and underlying. In that sense, the fundamental laws of physics are those that we would start with if we were to teach physics in a logical fashion, as opposed to the traditional historical method. In this approach the laws of fluid dynamics would be seen as a consequence of the microscopic laws of classical dynamics, themselves an excellent approximation to the nonrelativistic laws of the quantum mechanics of atoms. The atoms would be understood, in an excellent approximation, by nonrelativistic quantum mechanics that describes electrons interacting with nuclei; the nuclei would be understood as bound states of quarks and gluons – all these ingredients being part of the standard theory of elementary particle physics, which itself (together with the law of gravity) is part of who knows what. It is the business of elementary particle physics to search for the next rung in this ladder, to discover the 'who knows what' from which we could deduce our current somewhat the next rung in this ladder, to discover the 'who knows what' from which we could deduce our current, somewhat incomplete, description of matter and its interactions. It is this realm of fundamental physics that is intimately intertwined with mathematical research at the frontiers of mathematical study, where new patterns are being discovered and new edifices are being constructed.

This has been true from the beginning of modern physics, when Galileo first enunciated the proposition that the natural language of physics was mathematics. Newton, one of the greatest mathematicians of his day, invented the calculus of infinitesimals in order to calculate planetary orbits as well as to solve pure mathematical problems. In the following centuries there was little distinction between theoretical physics and mathematics, with many of the greatest contributors – Laplace, Legendre, Hamilton, Gauss, Fourier – being regarded as physicists by physicists and as mathematicians by mathematicians.

The twentieth century has witnessed two revolutions in physics and the completion of a theory of ordinary matter and its interactions. Once again we have called on mathematics to supply the tools and framework for this task. When Einstein created general relativity, the dynamical theory of space and time, in 1915, the necessary tools of differential geometry were available. They had been created by Gauss and by Riemann in the previous century. The effect of general relativity on mathematics was electrifying. Riemannian geometry became a central topic of geometry. The development of Quantum Mechanics built on the understanding of Hilbert spaces and influenced the development of functional analysis. Early particle physics drew heavily on the theory of continuous groups, which itself was partly motivated by the desire to understand the spatial symmetry of crystalline structure.

Nonetheless, during the middle part of this century mathematics and fundamental physics have developed in very different directions with little significant interaction between them. This was due, in part, to an atmosphere of increased abstraction in the mathematics community, as well as an insistence on rigid formal rigor as exemplified by the famous Bourbaki School. (This school, by the way, has had a disastrous effect on the style of mathematics writing; whereby authors are encouraged to remove from the description of their work all traces of intuitive reasoning or any hint at how they arrived at their ideas. This style, which lately is beginning to change, has made it difficult for nonspecialists to follow the progress of modern mathematics.) However, much of the reason for this separation was due to developments in physics. First, the early development of quantum mechanics and the early applications of quantum mechanics to elucidating the structure of matter required little mathematical sophistication. It has been said that *"In the 1930's, under the demoralizing influence of quantum theoretic perturbation theory, the mathematics required of a theoretical physicist was reduced to a rudimentary knowledge of the Latin and Greek alphabets."* Simple mathematical instruments largely sufficed for the first applications of quantum mechanics to the study of matter. During the first decades after the war the vistas of particle physics rapidly expanded. These times were dominated by experimental surprises and theoretical model building required little more than traditional mathematical tools.

This situation changed dramatically 10 years ago, when prompted by decades of experimental exploration, we arrived at the non-Abelian gauge theories of the strong, weak and electromagnetic interactions. These theories are now electromagnetic interactions. These theories are now universally accepted as yielding a complete description of all the interactions of matter at energies and distances that are experimentally accessible at present. This development is surely one of the most remarkable accomplishments of twentieth century science. Attention has more recently turned to the exploration of the structure of these theories and to even more ambitious attempts to construct unified theories of all the interactions of matter together with gravity. In the development of these gauge theories - the so-called "standard model" – it has happened that many significant physical problems lead to significant concepts in modern mathematics. Many of these concepts in fact were invented independently by physicists and by mathematicians.

Thus, for example, in 1931 Paul Dirac discussed, in one of the most beautiful papers in theoretical physics, the possible existence of elementary magnetic charges – magnetic monopoles. As you all know, there is no evidence in nature for the existence of magnetic monopoles. If you cut a magnet in two, instead of getting one piece with only a north pole and one with only a south pole, you get two new magnets each with its north and south pole inseparable. This is an experimental fact, but monopoles might exist and have merely eluded observation till now; in any case we may certainly contemplate their existence. Dirac investigated this possibility in the context of the newly developed quantum mechanics. He showed that in quantum mechanics magnetic monopoles made sense if and only if the product of their charge, g, with the electric charge of the electron, e, was an integer multiple of Planck's constant $\hbar : ge = n\hbar$. This was very exciting, since it meant that as long as there existed one magnetic monopole in the universe all charges had to be quantized in integer units of $\hbar/g$. In mathematical terms Dirac had discovered an integer that characterized the topological classification of vector bundles, mathematical constructs that were being invented at about the same time by mathematicians. These concepts have come to play a role of increasing importance in modern gauge theories and in modern mathematics.

We have borrowed much from modern mathematics but now the debt is being paid back. It is a fascinating fact that the geometries of 3 and 4 dimensions, which are precisely the number of space and of spacetime dimensions, at least macroscopically, are the most interesting from a mathematical point of view. Two dimensional surfaces are so small that they are easily analyzed. This classical analysis used tools which also played a central role in classical physics, particularly in electromagnetism. The solutions of Laplace's equation, for example, which is used to solve problems in electrostatics, can also be used to catalogue 2-dimensional surfaces. One shifts one's attention from the surface itself; a sphere, a torus, a sphere with 17 handles attached, etc.; to the properties of the solutions of Laplace's equation on the surface; say the number of independent nonsingular solutions of this equation. These classical results are so simple, as in the analysis of two dimensional surfaces, because Laplace's equation is linear.

Once you go to more than 4 dimensions there is so much room available that things become simple again. Three and four dimensions remain largely mysterious. Here methods developed in quantum gauge theories, using so called "instantons", were borrowed a few years ago by Donaldson, Taubes and Floer to deduce some deep and astounding properties of the geometry of three and four dimensional properties of the geometry of three and four dimensional spaces. It appears that the equations that govern four dimensional gauge fields, which underly modern particle physics, play a role in three and four dimensional geometry, similar to that played by Laplace's equations in two dimensions. In what is unlikely to be the final chapter in this saga, Witten has recently reinterpreted Donaldson's theory in physical terms, using it to speculate on a new phase of quantum gravity; and used quantum gauge theories to give a beautiful interpretation of certain invariants that allow one to classify knots in 3 dimensions.

Finally, recent developments in superstring theory, an ambitious theory that attempts to construct a unified quantum theory of matter and gravity, have begun to meet real mathematical frontiers. These theories have attracted much attention from mathematicians since they give strong hints of new connections between hitherto separate parts of mathematics. Many physicists believe that the final understanding of the structure of string theory will involve fundamental generalizations of geometry. Perhaps we are entering a golden era in the long history of cooperation between fundamental mathematics and physics. More on this later.

2. On the unreasonable effectiveness of mathematics in physics

Eugene Wigner wondered, almost thirty years ago, about the *"unreasonable effectiveness of mathematics in the natural sciences"*. The effectiveness of mathematics in physics is indeed impressive, and all too often taken for granted. Wigner argued that it is not at all obvious that mathematical concepts are appropriate for the description of natural phenomenon. These concepts are certainly not conceptually simple, conceptual simplicity is not one of the primary goals of mathematics, nor are they necessarily inevitable. However they are certainly useful. The mathematical formulation of physics often leads to a remarkably accurate description of many phenomena. This record of agreement provides convincing evidence that mathematics is the correct language for physics. Wigner pointed out that *"The enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and there is no rational explanation for it. It is not at all natural that 'laws of nature' exist, much less that man is able to discover them. The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve"*. I suppose he meant by the last remark that if we don't understand it we don't deserve it.

Indeed it is something of a miracle that we are able to devise theories which allow us to make incredibly precise predictions regarding physical phenomena and that we can carry out controlled experiments that allow us to measure these quantities to incredible precision. To give one of the most astounding examples, consider the magnetic moment of the electron. The electron can be crudely, very crudely, visualized as a little spinning ball of electric charge. The rotating charge gives rise to a current, the current to a magnetic field. Thus the electron has a *magnetic moment* μ (which determines the magnetic field it produces) proportional to its *spin* S;

$$\mu = g\,\frac{eh}{2mc} S\,.$$

Naively, the gyromagnetic ratio of the electron, g, should equal 2, and the deviation of g from 2 was one of the anomalies that stimulated the development of the relativistic
equal 2, and the deviation of g from 2 was one of the anomalies that stimulated the development of the relativistic
quantum theory of the electromagnetic field. After long, long calculations quantum electrodynamicists can predict and after careful, careful measurements atomic scientists can measure this parameter to *one part in hundred billion*! The result is:

(Using α^{-1} = 137.035 963 (15))

$$g_{theory} = $$
$$= 2\cdot\left[1+\frac{\alpha}{2\pi} - .328478445\left(\frac{\alpha}{\pi}\right)^2 + 1.183(11)\left(\frac{\alpha}{\pi}\right)^3 + \ldots\right]$$
$$= 2\cdot[1.000\ 159\ 652\ 459 \pm .000\ 000\ 000\ 123]$$
$$g_{experiment} = $$
$$= 2\cdot[1.000\ 159\ 652\ 193 \pm .000\ 000\ 000\ 004]$$

I am not sure which is more impressive, the theoretical accuracy or the experimental accuracy. Experimentally, this accuracy is achieved by the tour de force of trapping a single electron, for a very long time in a magnetic–electric bottle (a Penning trap). Theoretically, one must calculate up to fourth order in the perturbative expansion of Quantum Electrodynamics, with the fourth order term requiring the evaluation of 891 Feynman diagrams, an equivalent theoretical tour de force. The error is totally dominated by the uncertainties in the determination of the fine structure constant $\alpha = \frac{1}{137.03}$, which is the dimensionless constant that characterizes the strength of the electromagnetic force.

The ability to achieve this kind of precision is dependent on many fortunate circumstances – on our ability to isolate the studied physical phenomena from the environment, and on the invariance of basic physics under time and spatial translations so that one can repeat experiments elsewhere at some other time and on the smallness of the fine structure constant – features lacking in the discussion, say of social phenomena. However, they surely also depend on this miraculous overlap (or isomorphism, to use the mathematical term) between the pure mathematical structures that underlay quantum field theory and the real, material physical world. It is this seemingly perfect overlap, that is revealed by this and many other measurements, that is the source of Wigner's astonishment. As Einstein said, *"How is it possible that mathematics, a product of human thought that is independent of experience, fits so excellently the objects of physical reality"*. After all, in many other areas of human discourse our concepts are too fuzzy and our tools of analysis too crude to achieve such a precise match. If all we had to generalize about were the successes of economics or of political science we might agree with Pascal who said that *"Correctness and Truth are points so fine that our instruments are too blunt to touch them exactly"*.

Perhaps we can gain some insight into this mystery if we deepen it by examining yet another miracle in the connection between mathematics and physics.

3. On the unreasonable beauty of mathematics in physics

The mystery of the effectiveness of mathematics in fundamental physics is much deeper than just the miracle of its astonishing utility. After all it is no surprise that we need mathematics to deal with complicated situations involving systems composed of many parts all of which are in themselves simple. We have also learned recently that even simple systems whose microscopic laws of evolution are easy to describe can exhibit extremely complex behaviour. However, we might expect to be able to describe the microscopic laws in terms of simple mathematics. The
However, we might expect to be able to describe the microscopic laws in terms of simple mathematics. The
strangest thing is that for the fundamental laws of physics we still need deep mathematics, and that as we probe deeper to reveal the ultimate microscopic simplicity we require deeper and deeper mathematical structures. Even more, these mathematical structures are not just deep but they are also interesting, beautiful and powerful. As Dirac put it, *"It seems to be one of the fundamental features of nature that fundamental physical laws are described in terms of great beauty and power"* and *"As time goes on it becomes increasingly evident that the rules that the mathematician finds interesting are the same as those that Nature has chosen"*.

This hyperbole is full of ill defined terminology: interesting, beautiful, powerful. What do we mean when we say that an equation is beautiful, or that a physical concept is powerful? Consider the mathematical formulation of the 'standard model', the aforementioned theory of the strong, weak and electromagnetic interactions, that we believe describes all the constituents of matter and their interactions down to distances of 10^{-15}cm. The action that describes this theory, from which we believe that, in a frenzy of reductionism, we could describe all of low energy physics is given by the following mathematical expression:

$$S = \int d^4x\sqrt{g}\Big[\frac{1}{4}g_{\alpha\beta}g_{\gamma\delta}\{A^{\alpha\gamma}A^{\beta\delta} + TrB^{\alpha\gamma}B^{\beta\delta} + TrC^{\alpha\gamma}C^{\beta\delta}\}+$$
$$\frac{\theta_2}{32\pi^2}TrB^{\mu\nu}\widetilde{B_{\mu\nu}} + \frac{\theta_3}{32\pi^2}TrC^{\mu\nu}\widetilde{C_{\mu\nu}}+$$
$$\sum_{i=1}^{3}g_{\mu\nu}(\bar{Q}_i\gamma^\mu D^\nu_Q Q_i + \bar{L}_i\gamma^\mu D^\nu_L L_i)+$$
$$g_{\mu\nu}Tr(D^\mu_\phi\Phi)^\dagger(D^\nu_\phi\Phi) - V(\Phi)+$$
$$\sum_{ija}(\bar{Q}_i\Gamma^{ij}_{Qa}Q_j\Phi^a + L_i\Gamma^{ij}_{La}L_j\Phi^a) + R\Big]$$

Is this beautiful? Perhaps, but only in the eye of a very well informed beholder. Clearly the notion of beauty in mathematics, as in art, is an acquired taste. To appreciate mathematical beauty requires long education and training, and is always a subjective judgment. Nonetheless, there tends to be a large degree of consensus among mathematicians and physicists as to what is beautiful and what is not. In the above theory there is much that most of us feel is beautiful and there is much that is not. The beautiful parts are those that explain the forces of nature as arising from the powerful symmetry principles that are the sense of these 'gauge theories'. These are beautiful to physicists since from a simple principle of symmetry we deduce in an almost unique fashion the nature of the forces of nature, and the existence of the carriers of these forces – the graviton that underlies gravitational forces, the photon of light, the gluons that hold nuclei together and the W and Z mesons that are responsible for their radioactive decay. This part of the theory is also beautiful to mathematicians since these gauge theories provide interesting, very interesting as it turns out, mathematical structures – the fibre bundles I referred to above.

The ugly parts are those that describe the strange spectrum of matter. These don't follow from any symmetry principle and must be put in by hand, with many, much too many parameters in order to yield agreement with observation. In
and must be put in by hand, with many, much too many
parameters in order to yield agreement with observation. In
fact, it is largely due to this lack of beauty, as well as the large number of unexplained parameters, nineteen all in all, that we believe that this theory is not the end of the story – it is simply not pretty enough. Of course these two defects are correlated. In both mathematics and in physics the beauty and power of concept are strongly correlated. We find concepts and structures beautiful if they enable us to derive new results, understand new phenomena – if they are powerful.

The most beautiful part of the standard model is the idea of local, or gauge, symmetries. These are unlike the more familiar global symmetries of the world, according to which the laws of physics are unchanged if we perform a symmetry transformation on the whole world at once. A system, recall, possesses a symmetry if after making a geometrical transformation on it remains unchanged. For example physical systems possess rotational symmetry if they are unchanged under rotations about some axis. If the world possesses a gauge symmetry one can make local rotations by an amount which might differ from place to place. This symmetry first appeared in Maxwell's formulation of the laws of electromagnetism in the middle of the 19th century, although its full significance was not realized until the development of quantum mechanics. A theory which possesses a local gauge symmetry necessarily requires a special field (the gauge field, or mathematically the connection) with which one can connect objects that are separated in space. Associated with the gauge field is a particle, and a force which is mediated by this particle. In the case of electromagnetism the gauge field is simply the electromagnetic field, and the associated gauge particle is the photon of light, which mediates the electromagnetic forces between charged particles. All of electromagnetism is a consequence of this principle of local symmetry and the existence of charged matter. Remarkably all the other forces of nature, the weak forces responsible for radioactivity and the strong force that binds nuclei, are mediated by particles that are a generalization of ordinary light. The mathematical difference is that the gauge symmetry group is non–Abelian, in other words it matters in which order you carry out the group operations, unlike an Abelian group, such as that underlies electromagnetism, where it doesn't. This group is also much bigger so there are more kinds of light. In the case of the non-Abelian generalizations of gauge theory that appear in the standard model, the gauge particles are the 8 gluons and the W and Z particles that mediate the strong and the weak interactions. Of course, there are other dynamical differences between these forces, which arise because of the non–Abelian nature of the gauge group, which lead to the phenomenon of the symmetry breaking of the electro–weak interactions and the phenomenon of confinement of quarks and gluons for the strong interactions. It is for these reasons that we do not observe directly the existence of 11 other kinds of light rays.

The geometrical transformations need not take place in ordinary, 3-dimensional, space. They can take place in an internal space. Thus the gauge symmetry of electromagnetism can be thought of as a rotation about a circle – but not a circle in ordinary space but in some internal space. Mathematically, it is convenient to think of attaching to each point of ordinary space an internal space upon which the local symmetry acts. This combined object is called a fibre bundle. It is one of the most fruitful generalizations of ordinary geometrical objects and is of central concern to modern mathematics.

C.N. Yang, one of the inventors of non-Abelian gauge
and is of central concern to modern mathematics.

C.N. Yang, one of the inventors of non-Abelian gauge theories, tells the story of his meeting with the mathematician Chern, who had been a teacher of his in China, but with whom he had had little professional communication. Chern had done pioneering work on the classification of fibre bundles. Yang relates that when he learned that mathematicians had been talking for years about the identical structure that physicists had discovered he was very surprised. He remarked to Chern that *"It is both thrilling and puzzling, since you mathematicians dreamed up these concepts out of nowhere"*. Chern replied *"No, no, these concepts were not dreamed up. They were natural and real"*. This is a fascinating reply. Chern was expressing a point of view that, from my experience, is not uncommon among creative mathematicians – namely that the mathematical structures that they arrive at are not artificial creations of the human mind, but rather have a naturalness to them as if they were as real as the structures created by physicists to describe the so-called real world. Mathematicians, in other words, are not inventing new mathematics they are discovering it. The British mathematician Hardy said *"I believe that mathematical reality lies outside us, that our function is to discover or observe it, and that the theorems we prove and which we describe grandiloquently as our 'creations' are simply notes of our observations"*. Note that Hardy was a number theorist, the purest of pure mathematicians.

If this is the case then perhaps some of the mysteries that we have been exploring are rendered slightly less mysterious. If mathematics is about structures that are a real part of the natural world, as real as the concepts of theoretical physics, then it is not so surprising that it is an effective tool in analyzing the real world. Similarly, we might expect that physical and mathematical structures would share the characteristics that we call beauty. Our minds have surely evolved to find natural patterns pleasing. In other words we are turning Kant's view, that the source of mathematics is in the organizing power of the mind, inside out. Our minds are products of nature, functioning according to its laws and patterns, which are mirrored in the concepts of mathematics.

There is an obvious objection to this point of view. Theoretical physicists are constrained by experiment. Their constructions must not only be beautiful and powerful they must also be correct. They must agree with experiment and go beyond mere explanation to successful prediction. Mathematicians seem not to be constrained by these shackles. If physicists are searching for the one logical structure that describes the real world, mathematicians are exploring the space of all conceivable logical structures, only a portion of which overlaps the real, unique world. This is quite correct, nonetheless it does not contradict the idea of a common underlying structure which is a real feature of nature.

If it is the case that the concepts and structures that underlie fundamental mathematics and physics are common then it might be advantageous for workers in both fields to search for new ideas and structures in each others backyard. This strategy was promoted and followed by Dirac who said *"The research worker in his efforts to express the fundamental laws of nature in mathematical form should strive mainly for mathematical beauty."* and *"It may well be that the next advance in physics will come along these lines: people first discovering the equations and needing a few years of development in order to find the physical ideas behind the equation"*. Conversely, mathematicians should study the structures that physicists discover for possible hints of new mathematics. The absorption of structures from physics was of enormous
discover for possible hints of new mathematics. The absorption of structures from physics was of enormous importance in the early development of mathematics and this cross fertilization has recently been revitalized, not just in particle physics but also in the study of chaos in simple dynamical systems, in the discovery of fractal geometry and in many other examples.

The revitalization of the connections between mathematics and physics is especially true in the realm of elementary particle physics. Recent attempts to construct unified theories of matter and gravity have led to a radically new kind of theory – string theory, which gives hints of essential connections to many frontier areas of modern mathematics. String theory, which was originally discovered accidentally, about twenty years ago in an attempt to understand nuclear force, has emerged in recent years as a promising realistic theory of all the interactions, and for the first time a consistent theory of quantum gravity. To some extent string theory is a simple generalization of the ordinary framework of quantum field theory, in which the basic constituents of nature are not point-like, but extended one dimensional objects – strings. Remarkably, this seemingly minor extension from point-like particles to extended strings, without modifying in any other way the fundamental principles of physics, leads to an incredible structure. This structure implies that only forces that can exist are just of the kind we see – gravitational and gauge interactions. It can also produce the matter content of the world as we know it as well as the specific pattern of forces that we observe. It also has bizarre implications, requiring that space–time be ten dimensional. To agree with the crudest of observations it must be the case that six of the spatial dimensions are curled up into a little closed space so that we do not notice them. This can be achieved since, as a generalization of Einstein's theory of general relativity, the theory incorporates the dynamics of space–time, and possesses solutions with six compact, curled up, directions of space.

String theory has already provided many interesting mathematical connections. The theory makes use of deep structures in differential geometry and in algebraic geometry, connects to the theory of modular functions and finite groups. It even appears to have a place for branches of mathematics that I thought would never play a role in physics – like number theory and knot theory. I once described this development to a famous mathematician, who was intrigued by the theory and the mathematical ideas it drew upon. Being a mathematician, however, his first question to me was *"But is it physics?"*

The original highly optimistic, expectation that this theory, which in principle has the power to allow us to calculate all the parameters of the standard model as well as understand the reason behind many of its features, would lead rapidly to new predictions and tests, has undergone sober re-evaluation. It is not that there are any experimental contradictions, not are there any indications of internal inconsistency, rather it is clear that we don't yet know enough about the structure of the theory to control its dynamics sufficiently to make contact with experiment. Part of the problem is that we have stumbled upon this theory by accident, without knowing what the basic logical setting of the theory is or will be (it has been said that string theory is of the twenty first century, discovered by accident in the twentieth century).

A more immediate problem is that in trying to discover the principles of this theory and applying it to the real world to test its validity we are faced with the fact that the basic distance
principles of this theory and applying it to the real world to test its validity we are faced with the fact that the basic distance scale of the theory is very, very small. The fundamental length scale of string theory, or indeed of any unified theory of gravity and matter, is the Planck scale, the length that can be formed from the three dimensional fundamental constants of nature: Newton's constant of gravitation G, Planck's quantum constant h and the speed of light c:

$$l_{planck} = \sqrt{\frac{G}{hc^3}} \approx 10^{-33}\mathrm{cm}$$

(Alternatively, we can express this scale in units of time: $t_{planck} \approx 10^{-44}$ seconds, or in units of mass: $M_{planck} \approx 10^{19}$ $M_{nucleon}$). Unfortunately, this length scale is smaller by seventeen orders of magnitude than the smallest distances that we can see with our most powerful microscopes, our most energetic particle accelerators. The fact that this number is so small is responsible for some of the most striking features of our universe. For example, the reason stars are so big is that at the scale of the radius of ordinary atoms and nuclei gravity is very weak (because this scale is seventeen orders of magnitude below the Planck scale). Thus gravitationally bound aggregates of nuclei can contain approximately $\left(\frac{M_{planck}}{M_{nucleon}}\right)^3 \approx$ 10^{57} nuclei before collapsing.

The value of this number presents us with one of the major problems of theoretical physics, since it is very difficult to give a natural explanation for a number which is so small. This is the *hierarchy problem*, which is one of the motivations behind the construction of unified theories. In any case, it implies that string theory is an attempt to extrapolate far, far beyond present day experiment. There are two sides to this problem. First, it is difficult to guess the nature of physics at energies seventeen orders of magnitude removed from present day experiment. Second, even if we were lucky and had an idea of the physics at these incredibly small Planckian distances it would be very hard to make our way up to the distances at which measurements are done at present. There is a lot of physics that occurs along the way that we must understand if we are to make contact with experiment.

Is there any chance of direct experimental of string theory? I certainly do not know, but I do not think it is impossible. For example some people are very disturbed by theorists who imagine extra dimensions that cannot be seen directly. I did some historical research and discovered that the first person to object to more than three dimensions was Ernest Mach who wrote in 1883 that *"Spaces of more than three dimensions may be used, but it is not necessary to regard these as anything more than mental artifices"*. What was Mach talking about? He certainly was not aware in 1883 of Kaluza-Klein theory nor of string theory. What he was referring to was the interest in higher dimensional theories which was provoked by the mathematical work of Riemann. He noted that *"The use of the fourth dimension was a very opportune discovery for the spiritualists and for the theologians who were in a quandary about the location of hell"*. I certainly hope the spiritualists don't find out about ten dimensions!

Mach's criticism should provide no solace for the opponents of Kaluza-Klein or of string theory because on the same page he noted that atoms, which cannot be perceived by the sense, are *"these metal mental expediences have nothing to do with the phenomenon itself"*. Mach did not believe in atoms because he thought that you could not observe them. Little did he know that only 22 years later Einstein and atoms because he thought that you could not observe them. Little did he know that only 22 years later Einstein and Smoulokowski would realize that the observation, long before, of the botanist Brown would provide us with indirect evidence of the existence of atoms, that couldn't be perceived by the senses directly but only via their effect on small particles suspended in a solution. Maybe this is how strings, or extra spatial dimensions, will eventually show up, as effects that might be lying around today and that we cannot yet recognize.

An extrapolation of this enormity is unprecedented in the history of physics. One has every right to express scepticism as to the chances of success of such a risky venture, as some of my colleagues have recently done in a vocal and public fashion. It does little good to remind these critics that at high energy we have learned that the correct scale of distances is logarithmic (i.e. physics changes as the the logarithm of the distance scale for very short distances), so that an extrapolation by a factor of 10^{17} is really only a factor of log $(10^{17}) \approx 40$. It is equally useless to point out that we have no choice but to attempt such an extrapolation if we wish to address fundamental questions.

What is clear is that new strategies are required in today's climate, different from those that we employed in previous decades when our field was led by the nose by experimental discovery. Not that we don't expect new experimental discoveries. All unified theories, string theory included, predict much new physics that could be seen at the Superconducting Super-Collider, which we hope and trust will be built. Experiments with the SSC, although they will not take us to the energy scale at which gravity becomes as strong as the nuclear force, will be of crucial importance in providing clues to the connection between Planck scale physics and our, low energy, world. Without the SSC or similar machines particle physics will die.

A more interesting and practical question is: even if we were to succeed, how long will it take? This is hard to predict. Let me give a mountain climbing analogy. It used to be that as we were climbing the mountain of nature the experimentalists would lead the way. We lazy theorists would lag behind. Every once in a while they would kick down an experimental stone which would bounce off our heads. Eventually we would get the idea and we would follow the path that was broken by the experimentalists. Once we joined our friends we would explain to them what the view was and how they got there. That was the old and easy was (at least for theorists) to climb the mountain. We all long for the return of those days. But now we theorists might have to take the lead. This is a much more lonely enterprise. In the past we always knew where the experiments were and thus what we should aim for. Now we have no idea how large the mountain is, nor where the summit is. Thus it is very hard to predict how long it will take to make substantial progress.

At this moment, however, when we are faced with no experimental surprises or paradoxes and when string theory hints at deep mathematical structures, Dirac's strategy has become more and more appealing. Many string theorists are exploring the mathematical structures that have been thrown up by string theory in the hope that they will provide the underlying framework for the theory and give clues as to its dynamics.

Our critical colleagues denounce these efforts, indeed all of string theory and call it by the dirtiest name they can come up with – recreational mathematics. Although I resent being called a recreational mathematician I admit that there is a valid called a recreational mathematician I admit that there is a valid (albeit small) point to these criticisms. They remind us of the danger, in following the Diracian dictum, of turning into mathematicians. This for some theorists is an ever present temptation. This would not be good outcome for physics, nor I suspect for mathematics. Let us remember some of the differences between mathematics and physics.

The bottom line for mathematicians is the proof of their theorems, the logical consistency of their results. The final judge of theoretical physicists is experiment. Personally I feel that experiment is harsher mistress than consistency. Dirac, motivated as he was by mathematical ideas, nevertheless stated: *"I am not interested in proofs but only in what nature does"*. Indeed, when faced with the astounding prediction of his relativistic electron equation that there should exist a positively charged particle with precisely the same mass as the electron and no evidence for such a particle (the positron was discovered by Anderson five years later), he contemplated abandoning some of the beautiful symmetry of his theory in order to identify the positron with the proton, which is 1836 times heavier than the electron. Weyl, who more than any other mathematician in this century saw mathematics and physics as an organic whole said, in contrast, *"My work always tried to unite the true with the beautiful, but when I had to choose one over the other, I usually chose the beautiful"*. Ironically, in the case of the positron, it was Weyl who recognized the charge conjugation symmetry of the Dirac equation and who objected strenuously to the identification of the positron with the proton. In the end the positron was discovered with the same mass as the electron in accord with the symmetry of the Dirac equation. Weyl was proved right and beauty prevailed. In this case truth and beauty were the same, consistent with my message that most of the time there need not be conflict between these two principles. Nevertheless, if a choice between beauty and truth arises each of us must retreat to our individual corners of security.

Mathematicians think differently and have different habits of work than physicists, even when they are exploring similar structures. Mathematicians love to generalize, to extend their concepts to the most general possible case, to construct the most inclusive possible theory. Physicists are of course interested not in the most general case but in the special case of the real world. They also work by simplification, idealization and by the construction of specific examples. We might say that mathematicians labour to construct interesting and useful definitions, from which good theorems flow, physicists to construct interesting and useful models from which good predictions flow.

Mathematicians and physicists also have different strengths. I find the most remarkable attribute of great mathematicians in their power of abstraction. They are capable of feats of abstraction which leave me breathless. I suspect that mathematicians similarly admire physicists for their intuition, by which they are able to use mathematical formalism much as a poet uses language. Unlike mathematicians they are allowed to neglect the constraints of rigour, to guess what is true without proving it, proceeding as rapidly as possible to confront ones ideas with experiment. The Soviet mathematician Manin agrees. In discussing the advances that led to the standard model he remarked *"The choice of a Lagrangian on the unified theory of weak and electromagnetic interactions ... the introduction of Higgs fields, the subtraction of vacuum expectation values and other sorcery, which leads, say, to the prediction of neutral currents – all this leaves the mathematician dumfounded"*.

Finally physicists and mathematicians are taught to think differently. Even the chronology of the standard curriculum is different for the two fields. Physics is always taught historically, from the bottom up. We start with classical mechanics, then proceed to teach non relativistic quantum mechanics and only at the last stages teach modern, relativistic physics. This allows us to teach our students physical intuition by allowing them to practice on concrete everyday phenomena. Modern mathematics is often taught from the top down. This teaches the power of abstraction. Manin writes that *"It would be wonderful to master both types of thinking, just as we master the use of a right and a left hand"*.

This is probably impossible, it must violate some kind of uncertainty principle:

$$\Delta\ \text{Mathematics} \times \Delta\ \text{Physics} \geq C$$

In any case both approaches are necessary. We need each others' special talents and insights. Let us continue the collaboration and extend it.

But Vive la difference!

A World Lab Branch in Trieste

After the lecture by D. Gross, A. Zichichi took the floor for an extremely important announcement. He said that the World Laboratory[2] has opened a branch in Trieste and that this choice had been encouraged by the Italian Minister been encouraged by the Italian Minister of Foreign Affairs, Mr. Giulio Andreotti, since the ICTP, in its 25 years of activity, has contributed so much to the scientific development of the Third World. This choice had not been inspired by feelings, but by facts. Zichichi also said that the idea of the World Lab actually came from Abdus Salam and that for him the reason for choosing Trieste was a question of memory and gratitude.

[2] See Issue No. 7/8, October 1987. Prof. C. Villi (Italy) is the President and Prof. H. Dalafi (Iran) is the Secretary of the Trieste Branch of the Laboratory.

World Science

by courtesy of
CERN Courier,
Vol 29, No. 2, March 1989

The World Laboratory and the Third World Academy of Sciences are examples of ambitious new global examples of ambitious new global ventures using the established broad base of science and technology in the industrialized nations as a springboard for important projects in and among developing countries.

Established in 1986, the World Laboratory aims to promote truly global, open cooperation in technical and scientific research, with free circulation of information and researchers. The bottom line is an impressive list of ongoing multidisciplinary projects, with a wide geographical spread.

Under its president Antonino Zichichi, and with its main coordination centre in Geneva and with regional coordination centres in Moscow and Beijing, World Lab's ongoing programme is grouped into four main areas. The Archimedes programme covers geological (seismology, volcanology, ...) and environmental (climate, pollution, ...) monitoring and modelling, together with computer projects in the education and health areas. The Eloisatron basic physics programme includes the Eloisatron programme includes the Eloisatron project for a multithousand GeV proton collider, ongoing plans at CERN's LEP electron-positron collider, and the establishment in China of Advanced Centres for Science and Technology and for Astrophysics, together with neutrino and cosmic ray studies at the new Italian Gran Sasso underground Laboratory and elsewhere, detector research and development, and basic theory. Under the heading 'Improvement of Modern Life' comes a series of projects aiming for advances in food technology (production, processing and storage), medicine (15 projects), and progress in environmental and ecological sectors; three projects deal with advanced technologies such as coal slurrying and new clean energy sources. The final development areas is the field of controlled nuclear fusion.

Each of these four areas is grouped into well defined projects, each with its own clearly defined objectives and one or more directors.

One major success is the establishment of the China Centre for Advanced Science and Technology under T.D. Lee, who has been very influential in getting this project off the ground. The aim is to provide qualified Chinese students with hands-on experience in new technologies, bridging the gap between university education and the modern research environment. The first World Lab building has recently been completed in Beijing.

In addition to field work, World Lab progress is regularly reviewed at international meetings and seminars.

The aim of the Third World Network of Scientific Organizations (TWNSO), established last year with its headquarters in Trieste, Italy, is to promote the role of science and technology in developing countries. TWNSO, under the presidency of Abdus Salam, is an offshoot of the Third World Academy of Sciences, which has pushed the cause of international scientific collaboration since its establishment in 1983.

With support from industrialized nations pledged and with more prospects in the pipeline, TWNSO's membership includes 80 organizations drawn from 60 countries. Aims are to promote the development and application of science and technology both within the Third World and through Third World participation in international schemes. Areas such as space science, controlled thermonuclear fusion, biotechnology and high technology in general are seen as
thermonuclear fusion, biotechnology and
high technology in general are seen as
having a potentially strong impact on economic and social development in Third World countries. As well as welcoming with enthusiasm the TWNSO cause, ministers attending the inaugural meeting in Trieste also pledged to produce results in their home countries.

In addition to a strongly regional structure, with Asian, Arab, African and Latin vice-presidents, TWNSO has three project-oriented standing committees dealing with global projects, hazards and programmes.

Report on Cold Fusion

by
K. Tahir Shah[3]
(ICTP/SISSA)

Cold fusion (or sub-barrier fusion) of two nuclei is not a new discovery[4]. However, since the announcement by Martin Fleischmann of Southampton University, UK, and Steven E. Jones of Brigham Young University, Utah, USA, on March 23, 1989, there has been excitement and confusion regarding the nature of large-energy output nuclear fusion at low temperatures. Major laboratories around the world rushed to duplicate cold fusion experiments of these two scientists. At the time of writing this note, though the results[5] obtained at IBM, AT&T Bell Laboratories, Brookhaven National Laboratories, USA, were negative, other researchers at Frascati (Italy), Moscow University, USSR, Texas A&M University and Georgia Institute of Technology, obtained positive results confirming a substantial degree of fusion rate at low temperature. The contradictory nature of experimental data have raised many theoretical questions which are open to discussion and research.

What is cold fusion? When two nuclei come close enough they fuse together and release energy. But to achieve this they must cross the Coulomb barrier which typically requires a temperature 10^6-10^7K. The alternative to this brute force method of fusing two nuclei is quantum mechanical tunneling process. In this case, such a high temperature is not required, though the probability of fusion depends on how close two nuclei can come. It has been known since 1947 that it is possible to reduce Bohr radius (or the size) of an atom replacing electrons by a negative mu meson. For instance,

$$\mu^- + H \rightarrow (p\mu^-) + e^-$$

When a μ^- is captured by an atom, its Bohr radius decreases to

$$a_\mu = a_o \frac{m_{e^-}}{m_{\mu^-}} \cong 250 \text{ fermi}$$

where a_o is electron's Bohr radius, m_{e^-}

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3 K.T. Shah, S. Khadkikar and F. Krmpotic from ICTP attended the Cold Fusion Meeting on 12 April 1989 at Erice (Sicily, Italy).

4 For instance in:

1) nuclear physics, significant literature exists on heavy nuclei sub-barrier fusion (see e.g., "Lecture Notes in Physics" # 219, *Fusion Reactions Below the Coulomb Barrier*, MIT Conference, 1984);

2) atomic physics, mesomolecular processes have been known for over 35 years (see e.g., L. Bracci and G. Fiorentini, *Physics Reports* 86, pp. 169-216, 1982).

5 On April 14, 1989, an international conference was held at Erice (Trapani, Italy) to discuss the status of cold fusion.

(1) AT&T Bell Lab.; M.M. Broer et al. – Duplication of Fleischmann and Pons' experiment.
 (a) Used: Palladium wires, rods, films and foils, all dipped inside D_2O/H_2O.
(b) Calorimetry experiment used Pb D_2O/H_2O.
 (b) Calorimetry experiment, used Pb, Platinum.
 Conclusion: no confirmation of Fleischmann and Pons results; upper limit ≤ 0.6 neutron/sec/cm^2.

(2) Brookhaven Nat. Lab. (BNL)
 (a) Palladium in (Pd-D) (Electro-chemical group)
 Nothing exceptional was observed (Feldberg and Reilly).
 (b) Experiment with Palladium in D_2O, LiOD (like LiOH). Some neutron count; (consistent with claim but no heat) (U. Isaacs and A. Davenport).

(3) IBM (Yorktown) Zeigler et al.
 Material: rod, sheet and foil.
 Result: Nothing above background upper limit << 10^{-3} fusion/cm^3-sec.

(4) Frascati (INFN) Celani.
 Preliminary experimental results suggested a lowering of neutron background. There was a suggestion to repeat the experiment at Gran Sasso (underground) where neutron background is low. On April 23, 1989, there was news that they
background is low. On April 23,
1989, there was news that they
 observed successfully a clear signal of neutron flux. There were three experimental groups.
 Group one: Neutron flux substantial above background.
 Group two: F/P experiment duplicated.
 Group three (Scaramuzzi et al.): Low temperature experiment.
 High neutron flux was observed at a pressure of 50 atm at low temperatures.
 According to an article in *Nature* 338, 13 April 1989:
 Texas A&M University – Charles Martin.
 Duplication of F/P experiment –≅ 60-80% more energy output than input.
 Georgia Tech – James Mahaffey, Neutron 15 times background.

meson, respectively.

This is about 207 times smaller than electronic hydrogen atom. The electroneutrality and small dimension of $(p\mu^-)$ mesonic atoms permit them to come to nuclei within a distance of 2.56 $\times 10^{-11}$cm. This enhances barrier tunneling probability. Fusion becomes possible due to quantum mechanical tunneling through Coulomb barrier.

E.g., the reaction rate for d-d reaction

$$D + D \rightarrow t + p$$
$$\rightarrow {}^3H_e + n$$

$(\lambda)dd \cong 10^{-74} sec^{-1}$ (electronic)

while for muonic case

$(\lambda)dd \cong 10^{11} sec^{-1}$.

Indeed a very large increase.

The muon catalysed fusion was observed experimentally by L. Alvarez in 1957. In principle, two ordinary nuclei can also fuse together but the probability of such reactions in HD molecules is 10^{-24} per year per cubic meter of liquid HD. Thus, in the water of all oceans a nuclear fusion occurs every some 10^7 years! In the past, many theoretical calculations suggested the occurrence of cold fusion catalysed through cosmic ray muons but the rate was not enough to be of any practical consequence.

Fleischmann and Pons[6] consider the following reaction:

$${}^2D + {}^2D \rightarrow {}^3T\ (1.01\ MeV) + {}^1H\ (3.02\ MeV)$$
$${}^2D + {}^2D \rightarrow {}^3He\ (0.82MeV) + n\ (2.45\ MeV)$$

at room temperature (300K°). In their experiment, a neutron flux of about 3 times the background, corresponding to a fusion rate of $10^{-19} sec^{-1}$ per deuterium was observed. The following are important conclusions.

a) Excess enthalpy generation is markedly dependent on the applied
markedly dependent on the applied
current density (i.e., magnitude of the shift in the chemical potential) and is proportional to the volume of the electrodes. We are dealing with a phenomenon in the bulk of the Pd electrodes.

b) Enthalpy generation can exceed 10 Watt cm^{-3} of the Palladium electrode (maintained for 120 hours with heat in excess of 4MJ cm^{-3} of the electrode was liberated).

If it is ${}^2D + {}^2D \rightarrow {}^3He + n$ type fusion than one should observe $\cong 10^{11}$-10^{14} neutrons instead of $4 \times 10^4\ sec^{-1}$ for this amount of heat.

Heat is being generated by other neutronless nuclear or some unknown chemical reaction.

In his paper, Fleischmann himself remarked: *"The observation of the generation of neutrons and of tritium from electrochemically compressed D^+ in Pd cathod is in itself a very surprising result and, evidently, it is necessary to reconsider the quantum mechanics of electrons and deutrons in such host lattices. In particular, we must ask: is it possible to achieve a fusion rate of $10^{-19}\ sec^{-1}$ for ${}^2D + {}^2D$ reactions mentioned above for cluster of deutrons (presumably located in the octahedral lattice positions) at typical energies of 1 eV?"*

In a similar experiment by S.E. Jones *et al.* only a neutron flux of some five times the background was observed. There was no heat generation (BYU preprint 1989).

The results of Fleischmann, Jones and others suggest some other mechanism is responsible for catalysis of nuclear fusion at low temperature. At present there are no explanations as to how such a large amount of energy and neutron release become possible.

[6]See J. "Electromagnetical Chem." 261 (1989), 301-308.

IAEA Board of Governors at the ICTP

On 13 March, a group of twelve high-rank diplomats on the Board of Governors of the IAEA paid a one-day visit to the ICTP. They were accompanied by the Representative of the Italian Government to the IAEA Ambassador C. Taliani, Prof. M. Zifferero, Deputy Directory General for Research and Isotopes of the IAEA, and Dr. Spanke from UNESCO.

Professor Abdus Salam, Director of the ICTP, welcomed the group and introduced it to the many facets of the Trieste reality. After which, the Deputy Director, Prof. L. Bertocchi, described the various sectors of activity of the ICTP, followed by a description of the Third World Academy of Sciences by its Executive Secretary, Professor M.H.A. Hassan. Counsellor G. Rosso Cicogna, Project Leader, concluded the exposition, by describing the present status of the feasibility study for a new International Centre for Sciences comprising a Centre for High-technology and New Materials, a Centre for Pure and Applied Chemistry and a Centre for Earth Sciences and Environment. After the discussion which followed the presentation, the

The Members of the IAEA Board of Governors had also a tour of the premises of the ICTP. They were shown around the Computer Room by the Head of the Scientific Computing Services, Dr. A. Nobile.

Members of the Board visited the premises of the ICTP. In the afternoon, the Members of the Board met participants from all over the world and were given the opportunity to discuss the problems which scientists from developing countries are faced with.

The Trieste Science Link Committee had in the meantime, organized a programme of sight-seeing for the wifes of the diplomats. The group included Ambassador M. Shenstone (Canada), Chairman of the Board, Ambassador R.E. Guyer (Argentina), Ambassador M.J. Wilson and Counsellor G.R. Hogg (Australia), Counsellor Wang Chuanying and Counsellor Xia Yunfu (China), Ambassador J. Morales Pedraza (Cuba), Ambassador J.R. Hiremath (India), Scientific Attaché J. Iljas (Indonesia), Scientific Counsellor S. Merza Mahmoud (Iraq), Ambassador Takanori Kazuhara (Japan), Ambassador A.H. bin Ali (Malaysia), Ambassador T.A. Mgbokwere (Nigeria), Ambassador G.E. Clark (UK) and Ambassador R.M. Timerbaev (USSR).

In the afternoon, the IAEA Board of Governors split up and each Member met a group of scientists, mainly from his own country of origin. Prof. M. Zifferero (Italy) (right), Deputy Director General of the IAEA, attended the meeting with South American scientists.

The meeting of the IAEA Board of Governors took place at the Adriatico
The meeting of the IAEA Board of Governors took place at the Adriatico Guest House of the ICTP.

Associate Members Present at ICTP in 1989

KEY:				
	AP	Atomic Physics	GEO	Geophysics
	APPL.MATH	Applicable Mathematics	MATH	Mathematics
	ASTRO	Astrophysics	MATH PHYS.	Mathematical Physics
	BIO	Biophysics	NP	Nuclear Physics
	CLIMA	Climatology	PP	Plasma Physics
	COMM	Communications Physics	SE	Solar Energy
	COMPUT. PHYS.	Computational Physics	SOIL	Soil Physics
	EP	Elementary Particles	SS	Solid State

Associates Visiting ICTP in 1989

	Name	*Field*	*Member State*	*Arrival*	*Expected Dep.*
1.	ABDALLA, E.	EP	Brazil	1.4	15.4
2.	AHMAD, S.A.	AP	India	29.1	1.4
3.	ALIAGA-GUERRA, D.	SS	Peru	14.2	14.5
4.	AULAKH, C.S.	EP	India	5.4	28.6
5.	BAETA, R.D.	SS	Ghana	22.4	30.6
6.	CHIDUME, C.	MATH	Nigeria	19.4	18.7
7.	DERELI, T.	EP	Turkey	2.4	18.4
8.	EL-SHERBINI, T.E.	AP	Egypt	16.3	1.5
9.	FAMUREWA, O.	BIO	Nigeria	1.4	31.5
10.	FON, W.C.	AP	Malaysia	15.4	15.6
11.	GONDAL, M.A.	AP	Libya/Pakistan	27.1	25.2
12.	HUSSAIN, F.	EP	Pakistan	6.4	15.4
13.	KAAHWA, Y.	SS	Uganda	19.4	18.7
14.	KHADKIKAR, S.B.	NP/EP	India	31.3	29.6
15.	KHAN, I.A.	MATH	India	8.4	5.7
16.	KRMPOTIC, F. (Senior)	NP	Argentina	14.2	31.3
17.	LI Zhong Yuan	pp	China	23.3	21.6
18.	MBEMBA, G.	SS	Congo	14.4	30.7
19.	RUIZ-CLAEYSSEN, J.C.	MATH	Brazil/Peru	12.1	23.2
20.	SEKANDI, S.E.B.	COMM. PHYS.	Uganda	29.1	25.3
21.	SINGH, L.P.	EP	India	5.3	21.8
22.	SRITRAKOOL, W.	SS	Thailand	30.3	11.5
23.	SUH, B.S.	BIO	Korea	8.1	20.2
24.	VILCA, F.	MICRO	Peru	12.1	25.3
25.	WANG Shui	GEO	China	22.1	21.4
26.	YUNUS, A.	BIO	Bangladesh	18.4	5.7
27.	ZHANG Li-Yuan	SS	China	11.1	14.4
28.	ZHOU Hui-lan	GEO	China	4.1	4.4

Associates Expected in 1989
(Second List)

	Name	*Field*	*Member State*	*Visit Period*
1.	AKYÜZ, S.	AP	Turkey	19 Jun
2.	AYDIN, R.	AP	Turkey	5 Jul
3.	BAIG, A.M.	AP	Pakistan	Jun-Sep
2.	AYDIN, R.	AP	Turkey	5 Jul
3.	BAIG, A.M.	AP	Pakistan	Jun-Sep
4.	BHANTHUMNAVIN, V.	AP	Thailand	6 Aug
5.	GAMAL, Y.E.	AP	Egypt	25 Jun
6.	KOREK, M.	AP	Lebanon	Jul
7.	OBADA, A.	AP	Saudi Arabia/Egypt	mid Jul
8.	RAZMI, M.S.K.	AP	Pakistan	last wk Jun
9.	HE, J. (Senior)	APPL.MATH	China	26 May
10.	GÜRSES, M.	ASTRO/APPL.MATH	Turkey	25 Jun
11.	SOBOUTI, Y.	ASTRO/EP	Iran	6 Jul
12.	BORZI, C.	BIO	Argentina	Sep
13.	FAKHFAKH, Z.	BIO	Tunisia	15 May
14.	SEVERCAN, F.	BIO	Turkey	Jul
15.	TORRES-HERNANDEZ, J.L.	BIO	Mexico	1 Aug
16.	YEBOUA, A.F.	BIO	Ivory Coast	Sep
17.	CEPPI, E.A.	CLIMA	Argentina	6 Nov
18.	OMOTOSHO, J.B.	CLIMA	Nigeria	31 Jul
19.	OTHMAN, M.	CLIMA/SE	Malaysia	5 Nov
20.	JAIN, V.K.	COMM	India	17 May
21.	SEVERCAN, M.	COMM/MICRO	Turkey	1 Jul

Associates expected – contd.

22. SAMARANAYAKE, V.K. (Senior)	COMP/EP/NP	Sri Lanka	early '89
23. BISOI, A.K.	COMPUT. PHYS.	India	15 Sep
24. GRAVE DE PERALTA, L.	COMPUT. PHYS.	Cuba	15 Jun
25. BAAQUIE, B. (Senior)	EP	Singapore/Bangladesh	2 May
26. ÇELIK, T.	EP	Turkey	24 Jun
27. CHO, Y.	EP	China	15 Jun
28. DHAR, A.	EP	India	10 Jun
29. DURU, I.H.	EP	Turkey	1 May
30. EL-HASSOUNI, A.	EP	Morocco	15 Jun
31. FANG, L.Z.	EP	China	Summer
32. KAMRAN, M.	EP	Pakistan	Jun
33. KAUL, R.K.	EP	India	1 Jun
34. MA Zhong-Qi	EP	China	Jun
35. MARINO, E.C.	EP	Brazil	12 Jun
36. OH, C.H.	EP	Singapore/Malaysia	22 May
37. RUEDA, A (Senior)	EP	Puerto Rico	1 Jun
38. SAHDEV, D.	EP	India	1 Jun
39. TEH, R.	EP	Malaysia	5 Jun
40. WOLDE-GHIORGIS, W.	EP	Ethiopia	early Summer
41. ZANELLI, J.	EP	Chile	15 Jul
42. ZHAO, Z.Y.	EP	China	15 Jul
43. CHU, Y.	EP/ASTRO	China	Jun
44. QADIR, A.	EP/MATH/ASTRO	Pakistan	7 Jun
45. ABDEL WAHAB, M.M.	GEO	Algeria/Egypt	10 July
46. ADEDOKUN, J.A.	GEO	Nigeria	22 Sep
47. ADJEPONG, S.K.	GEO	Nigeria	11 Sep
48. ANANE-FENIN, K.	GEO	Liberia	May
49. EL-HADJ TIDJANI, M.	GEO	Benin	15 Oct
50. GHALEB, A.F.	GEO	Egypt	20 Jun
51. KOLAWOLE, L.B.	GEO	Nigeria	1 Aug
52. KUNARATNAM, K.	GEO	Sri Lanka	1 Oct
53. TANTRIGODA, D.A.	GEO	Sri-Lanka	Sep
54. AFUWAPE, A.U.	MATH	Nigeria	Aug
55. AHSAN, J.	MATH	Pakistan	15 Jun
56. ALEMU, Y.	MATH	Ethiopia	11 Jun
57. ASGHAR, S.	MATH	Pakistan	1 Jun
58. AZAD, H.	MATH	Pakistan	May
59. CHAUDHARY, M.N.	MATH	Pakistan	Jun
60. DABBOUR, A.E.S.A.	MATH	Egypt	Jul
61. DISSANAYAKE, U.N.B.	MATH	Sri Lanka	1 Sep
62. DJAFARI-ROUHANI, B.	MATH	Iran	Jul
63. DZINOTYIWEYI, H.A.M.	MATH	Zimbabwe	mid May
64. ELWAKIL, E.	MATH	Egypt	17 Jul
65. [illegible]	MATH	Nigeria	Aug
64. ELWAKIL, E.	MATH	Egypt	17 Jul
65. IMORU, C.O.	MATH	Nigeria	Aug
66. JAMEEL, M.	MATH	Pakistan	Jun
67. KARAKURA, F.	MATH	Burundi	1 Aug
68. KHALIL, F.	MATH	Bangladesh	16 Sep
69. LI, S.	MATH	China	28 Jun
70. MAHDAVI-HEZAVEHI, A.	MATH	Iran	Sep
71. MIATELLO, R.J.	MATH	Argentina	Dec
72. MSHIMBA, A.S.A.	MATH	Tanzania	end Apr
73. OLUBUMMO, A. (Senior)	MATH	Nigeria	8 Jul
74. ONYANGO-OTIENO, V.P.	MATH	Kenya	1 Jun
75. SEADE, J.	MATH	Mexico	Nov
76. SHABANI, J.	MATH	Burundi	3 Jul
77. SHAFII-DEHABAD, A.	MATH	Iran	10 Jul
78. SHRESTHA, G.	MATH	Nepal	Jun
79. THAHEEM, A.B.	MATH	Pakistan	1 Jun
80. ZAFARANI, J.	MATH	Iran	21 Aug
81. MUTANGADURA, S.A.	MATH.PHYS.	Zimbabwe	7 Aug
82. AWIN, A.M.	NP	Libya	18 Jul

Associates expected – contd.

83. GUPTA. R.K.	NP	India	20 May
84. GUPTA, S.K.	NP	India	7 May
85. JAQAMAN, H. R.Y.	NP	West Bank	May
86. MAVROMATIS, H.	NP	Lebanon	19 Jun
87. MOULAY, M.	NP	Algeria	mid Jul
88. MSHELIA, E.D.	NP	Nigeria	1 Aug
89. SHARMA, S.K.	NP	India	1 May
90. WAHEED, A.	NP	Pakistan	Summer
91. ABU-ASSALI, E.I.	PP	Syria	Summer
92. AHMAD, M.	PP	Pakistan	10 May
93. CHAUDHRY, M.B.	PP	Pakistan	15 May
94. EL-ASHRY, M.Y.	PP	Egypt	15 May
95. GRATTON, J.	PP	Argentina	May
96. HUSSEIN, A.M.	PP	UAE/Egypt	1 Jun
97. JHA, L.N.	PP	Nepal	1 May
98. KRISHNAN, V.S.	PP	India	3 Jun
99. LEE, S.	PP	Malaysia	1 May
100. MOFIZ, U.A.	PP	Bangladesh	3 May
101. NYABUL, M.	PP	Zaire	Feb/Mar
102. RAPOZO DA CUNHA, C.	PP	Brazil	13 May
103. SEN, A.	PP	India	4 May
104. SMITH, A.J.	PP	Sierra Leone	May-July
105. ADEGBOYEGA, G.	SE	Nigeria	Sep
106. ARIAS, M.	SE	Puerto Rico/Ecuador	20 May
107. BAMIRO, O.A.	SE	Nigeria	20 Aug
108. BANSAL, N.K.	SE	India	11 Sep
109. BARRY, M.B.	SE	Guinea	9 Sep
110. BHARGAVA, A.K.	SE	India	Sep
111. CHENG, R.G. (Senior)	SE	China	7 Sep
112. EL-DESSOUKI, M.S.	SE	Egypt	5 Jul
113. HUSAIN, S.	SE	Bangladesh	1 Aug
114. IBRAHIM, M.	SE	Bangladesh	15 Aug
115. INAN, D.	SE	Turkey	1 Jul
116. MBOW, S.M.	SE	Senegal	16 Jul
117. NANDWANI, S.S.	SE	Costa Rica/India	10 Sep
118. SAMUEL T.D.M.A.	SE	Sri Lanka	9 Sep
119. YOUS, B.	SE	Morocco	Aug
120. AINA, P.O.	SOIL	Nigeria	31 Aug
121. OBI, M.E.	SOIL	Nigeria	5 Sep
122. ABDULLAH, T.	SS	Pakistan	15 May
123. BREZINI, A.	SS	Algeria	12 Jun
124. CALDEIRA	SS	Brazil	19 Jun
125. ERCELEBI, A.Z.	SS	Turkey	Jun
126. FOGLIO, M.E. (Senior)	SS	Brazil	11 Jul
125. ERCELEBI, A.Z.	SS	Turkey	Jun
126. FOGLIO, M.E. (Senior)	SS	Brazil	11 Jul
127. GHASSIB, H.B.	SS	Jordan	Jun
128. GONG C.	SS	China	27 May
129. HAO Bai-lin	SS	China	10 Jun
130. IQBAL, Z.	SS	Pakistan	Summer
131. ISLAM, A.K.M.A.	SS	Bangladesh	16 May
132. KHWAJA, Y.	SS	Pakistan	1 Jun
133. KUN, H. (Senior)	SS	China	1 Aug
134. LINARES, J.	SS	Peru	18 Jun
135. MAJID, C.A.	SS	Pakistan	29 May
136. MARTIN, H.O.	SS	Argentina	Jun
137. MESKINI, N.	SS	Tunisia	17 Jul
138. ONG, O.C.	SS	Singapore	26 May
139. PROTO, A.N.	SS	Argentina	Sep
140. RAHMAN, S.M.M.	SS	Bangladesh	1 Jul
141. RAMASWAMY, R.	SS	India	1 Jun
142. SADIQ, A. (Senior)	SS	Pakistan	27 May
143. TAO, R.	SS	China	25 May

Associates expected – contd.

144. YU, Li-sheng	SS	China	28 Jun
145. HARIDASAN, T.M.	SS/SE	India	April

Future Activities at ICTP in 1989

Fourth Workshop on Perspectives in Nuclear Physics at Intermediate Energies	8 - 12 May
Spring School on Plasma Physics	15 May - 9 June
Working Party on Modelling Thermomechanical Behaviour of Materials	29 May - 16 June
Working Party on Fracture Physics	29 May - 16 June
Second ICFA School on Instrumentation in Elementary Particle Physics	12 - 23 June
Miniworkshop on "Strongly Correlated Electron Systems"	19 June - 21 July
Research Workshop in Condensed Matter, Atomic and Molecular Physics	19 June - 29 September
Interface between Quantum Field Theory and Condensed Matter Physics (Anniversary Adriatico Research Conference)	20 - 23 June
Summer School in High Energy Physics and Cosmology	26 June - 18 August
Quasicrystals (Anniversary Adriatico Research Conference)	4 - 7 July
Workshop on Superstrings	12 - 14 July
Conference on Supermembranes and Physics in 2+1 Dimensions	17 - 21 July
Strongly Correlated Electron Systems (Anniversary Adriatico Research Conference)	18 - 21 July
Symposium on "Highlights in Condensed Matter Physics"	1 - 3 August
Workshop on Phenomenology in High Energy Physics and Cosmology	16 - 18 August
Topical Meeting on Variational Problems in Analysis	28 August - 8 September
Computations in Physics and and Physics in Computation (Anniversary Adriatico Research Conference)	5 - 8 September
Adriatico Working Party on Condensed Matter Properties of Neutron Stars	11 - 29 September
Workshop on Materials Science and Physics of Nonconventional Energy Sources	11 - 29 September
Conference on Lasers in Chemistry	18 - 22 September
Workshop on Interaction between Physics and Architecture in Environment Conscious Design	25 - 29 September
Trieste Conference on Recent Developments in Conformal Field Theories	2 - 4 October
Fifth College on Microprocessors: Technology and Applications in Physics	2 - 27 October
Workshop on Soil Physics	9 - 27 October
College on Differential Geometry	30 October - 1 December
25th Anniversary Conference on "Frontiers in Physics, High Technology and Mathematics"	31 October - 3 November
25th Anniversary Conference on "Frontiers in Physics, High Technology and Mathematics"	31 October - 3 November
Workshop on Telematics	6 - 24 November
ICTP & INFN Course on Basic VLSI Design Techniques	6 November - 1 December
Third Autumn Workshop on "Atmospheric Radiation and Cloud Physics"	27 November -15 December

For information and applications to courses, kindly write to the Scientific Programme Office.

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