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- Mathematics - A Science in the Ivory Tower or a Key for Developing Technologies ?

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Mathematics is an esoteric and inexpensive subject. It, therefore, exists in almost every university in all parts of the world. It has a reasonable prestige as a cultural activity, neither being useful nor doing any harm. Mathematicians may often enjoy this situation; nobody disturbs them, they belong to a small but international, strongly interacting scientific family. They have only few students, but one or two of them are really brilliant. Nowadays students sometimes disappear, they change over to computer science, or vacant positions in the mathematics department are taken away. Then these mathematicians are startled: "Why just now, when my field is getting more important and my research is even more successful?" But after some time (since their research does not really need students or partners in the same department),they forget about these same department),they forget about these troubles.

Mathematics, on the other hand, is a key for the key technologies of our time. "Apparently, too few people recognize that the high technology that is so celebrated today is essentially a mathematical technology" writes Edward E. David, President of Exxon R&D as a chairman of an NSF committee which dealt with the official support for mathematics in the United States. Winners of Nobel Prizes for medicine (Cormack) and economics (Debreu), not to speak of several winners for physics, got their Prizes for pieces of work which were mainly or essentially mathematical. Mathematics, as a tool, has gained enormous importance because even complicated mathematical models can be evaluated and produce optimal design and reliable prediction - shortly, really useful numbers - with the aid of modern computers. There is, therefore, a transition in the technological development from real models to mathematical models.

There is obviously a gap between this kind of mathematics as a tool - let us call it "industrial mathematics" - and the traditional mathematics which is taught in schools and universities, produced by professors, their assistants and students. This gap is a fairly new historical phenomenon. There used to be strong interaction between technical and physical problems and the creation of mathematical ideas - except during the last 50 years. During that time, mathematics "emancipated from the pressure of non-mathematical questions, but lost contact with reality. To be honest, things are changing and more and more mathematicians are getting interested in problems originating from science, economics of technology. Groups doing "mathematical science", "industrial mathematics", "mathematical "industrial mathematics", "mathematical computing" and so on, are born almost everywhere, at least in the industrialized world. Here they begin to compete with the "pure artists", not accepting any longer to be considered "second class mathematicians"

It is my impression that developing countries are not as far yet. As far? Some of my colleagues would prefer our university not to be "as far". But before discussing how far one should go, let us have a glance at the consequences for the developing countries. Abdus Salam, Nobel Prize Winner and Director of the International centre for Theoretical Physics, discusses similar questions. His main concern is that "technology transfer must always be accompanied by science transfer"; that science is the technology of tomorrow and tat, whenever we speak of science, it must be broad-based in order to be effective for applications. I would even go as far as to say that if one was being Machiavellian, one might discover sinister motives among those who try to sell us the idea of technology transfer without science transfer. There is nothing which has hurt us in the Third World more than the recent slogan in the richer countries of "Relevant Science". What I am wondering about is whether a very pure science which does not care for applications at all is not as

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dangerous as the opposite. Is it not also possible to discern sinister motives among those who educate theoreticians in such a way that they consider applications to be below their dignity? I am sure that there is no Machiavelli working this way consciously, but the result, at least in mathematics, seems to be such that the description at the beginning of this article is a good approximation also for developing countries.

There is a nice little story on two balloon pilots who ask somebody on the ground where they are. After a while they get the answer: "In a balloon". So they come to the conclusion that the man must be a mathematician, since he thought before answering and gave a very precise answer which, in the end, was completely useless.

This little story could have happened also in the Third World. Indeed, all science is relevant in principle, but in some cases one even has to work scientifically to make it really relevant. And here we come to the question of what industrial mathematics is or should be. Managers in high-tech companies in industrialized countries have a simple answer: the mathematics done by the scientists of their companies. At companies like Siemens, there are more than a thousand mathematicians, engineers and physicists doing mainly mathematical work. Chemical companies like BASF have at least several hundreds of them. Though they would certainly profit from a stronger cooperation with "open-minded" academic mathematicians (and, vice-versa, academic mathematics would profit from it), this is not what I have in mind with respect to developing countries.
mind with respect to developing countries.

Industrial mathematics, as I understand it, means to build bridges from the world of (pure) mathematics to the world of real problems. This bridge-building had some tradition (forgotten for more than 50 years) in Germany: the famous geometer Felix Klein established a cooperation between industry and academic mathematics already at the end of the last century by founding the "Göttingen Society for the Support of Applied Physics and Mathematics" of which men of industry like Siemens, Linde and Krupp and scientists like Caratheodory, Courant, Hilbert, Minkowski, Nernst, Prandtl and Schwarzschild became members.

What the ideal of industrial mathematics is you may understand best thanks to three names of scientists whom Klein brought to Göttingen, thereby initiating the very high prestige this university gained at the beginning of the century: Hilbert, Runge and Prandtl. Hilbert, as an open-minded but certainly pure mathematician, produced new mathematical ideas, concepts and theories - shortly, the raw material for mathematical models. Runge was one of the first numerical analysts (every student knows the Runge-Kutta methods), a man who helped to evaluate these models by solving the equations and producing numbers which contained the information about the solution of practical problems. Finally, Prandtl built the models and used Hilbert's and Runge's ideas to describe the behaviour of fluids. It is certainly not easy to make a choice similar to the one Klein made, but this is not the main point. What I want to illustrate is, in which sense all kinds of mathematics are relevant - one needs abstract mathematical ideas, the ability to use them for modelling and the algorithms and the processors to use the models for the solution of practical problems. Each one of these factors without the others is somehow useless: instruments without the ability to be adapted to new problems - models which are too simple for treating realistic tasks (or too complicated to be evaluated) - theories which cannot be applied.

The famous American mathematician, Paul R. Halmos, wrote that "applied" mathematicians do not normally accept the distinction between pure and applied mathematics, whereas pure mathematicians do. In this sense, I am a pure mathematician. Moreover, Halmos maintains that 'applied' needs 'pure' but not vice-versa, the relation
'pure' but not vice-versa, the relation
being like that between an ant-eater and an ant. I wonder whether he thought about mathematicians in developing countries - but even in the industrialized world he is wrong: the and-eaters are ant-feeders as well!

We have to educate mathematical generalists instead of specialists. Those mathematicians who want to help their societies by solving their problems must have a broad knowledge in different fields in order to find the appropriate tool. To be an expert even in a highly applicable topic (say, stochastic differential equations) may be extremely useful in a country full of high-tech industry. This expert may wonder whether there are practical problems fitting into his competence. But if there are none, he will more often be lost. A general (mathematical) problem-solver has no a priori information about the mathematical ideas he needs when meeting a problem. He may be faced with time series analysis, control problems, free boundary value problems, the numerical solutions of ordinary differential equations with algebraic side conditions, geometrical difficulties in computer-aided design, or - rather often - with something which is obviously mathematical in nature but does not yet exist as a complete theory. Yet, it is trivial to say that no generalist is able to know enough. He may discover that a problem fits into a certain mathematical 'box' but he shall not normally be an expert in this 'box'. However, it is sufficient to know an expert and to transfer the problem to him - this, in my opinion, is the meaning of the words "scientific community". A set of "monads" (in the sense given by Leibniz), of blind introverted individuals working on a speciality cannot form a community. But mathematicians, if monads at all, are not voluntarily such. If somebody asks for advice on a particular field, they are mostly very willing to communicate and help (many of the 'pure' like it very much to be at least a little bit 'applied' - and are happy to find an application).

Therefore, one has to identify the 'box' and to know an expert in that 'box'. This is easier to accomplish with a little bit of organization; some European groups are forming a European Consortium for Mathematics in Industry (ECMI) for that reason. ECMI will help to find the 'box' (by organizing schools in which specialists can explain to generalists the main ideas of their specialities), find the expert (by
specialities), find the expert (by
organizing conferences and lists), and educate industrial mathematicians (by introducing a common graduate programme with exchange of teachers and students). Maybe - and I will try to realise this idea - ECMI will also help developing countries in building up centers for industrial mathematics (by organizing workshops in these countries or creating some kind of partnership, in order to give the scientific community there the critical size).

Industrial mathematics should therefore be regarded as neither a new discipline nor the trivial version of pure mathematics, but, in fact, the bridge between theory and practice, again for mathematics in finding new ideas, problems and possibly prestige too, again for industry to find better solutions; - a chance to establish a better scientific community to help each other and, last but not least, to encourage friendship.

History of the Weak Interactions

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By courtesy of: CERN Courier
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In 1898 Rutherford discovered that the so-called Becquerel ray actually consisted of two distinct types of radiation: one that is readily absorbed which he called alpha radiation, and another of a more penetrating character which he called beta radiation. Then, in 1900, the Curies measured the electric charge of the beta particle and found it to be negative. That, at the turn of the century, began the history of the weak nuclear interaction. From the very start the road of discovery was tortuous, and the competition intense.

A letter written by Rutherford to his mother expressed the spirit of research at that time: 'I have to keep going, as there are always people on my track. I have to publish my present work as rapidly as possible in order to keep in the race. The best sprinters in this road of investigation are Becquerel and the Curies...' Rutherford's predicament is very much shared by us to this day.

Soon even more runners appeared: Otto Hahn, Lise Meitner, William [illegible] Wilson, von Bäyer, John Chadwick, Niels Bohr, Wolfgang Pauli, Enrico Fermi, Charles Ellis, George Uhlenbeck, and many others. We know that to reach where we are today took nearly a whole century and a large cast of illustrious physicists. Yet probably any modern physicist is only three handshakes away from these pioneers (for some perhaps only two) - you shake Jack Steinberger's hand, which shook Fermi's hand, which shook all those other hands.

In the mid-1960s, Lise Meitner came to New York and I had lunch with her at a restaurant near Columbia. When K.K. Darrow joined us, Meitner said 'It's wonderful to see young people.' To appreciate this comment, you must realize that Darrow was one of the earliest members of the American Physical Society and at that lunch he was over 70. But Lise Meitner was near 90. I was quite surprised when she told me how she started her first postdoctoral job in theory with Boltzmann, a contemporary of Maxwell. That shows us how recent even the classical period of our profession is.

After Boltzmann's unfortunate death in 1906, Meitner had to find another job. She said she was grateful that Planck invited her to Berlin. However, upon arrival, she found that because she was a woman she could only work at Planck's institute in the basement, and only go in and out through the servants' entrance. At that time, Otto Hahn had his laboratory in an old carpenter's shop. Lise Meitner decided to join him and to become and experimentalist. For the next thirty years, their joint work shaped the course of modern physics.

In 1908 they found that the absorption of beta particles through matter followed an exponential law. From that they concluded beta rays are of unique energy. It was Wilson, in 1909, who drew an opposite conclusion that the beta rays are heterogeneous in energy. But soon Hahn and von Bäyer found line spectra, which again confused the issue. This was cleared up by Chadwick in 1914, who established the continuous beta spectrum.

With the advent of quantum theory, Meitner, in 1922, raised the question concerning the origin of the continuous spectrum. She reasoned that a nucleus, presumably quantized, should not emit electrons of varying energy. Could it be electrons of varying energy. Could it be that the observed inhomogeneity was introduced after the expulsion of the electron from the nucleus? A series of experiments by Ellis and others quickly established that this is not the case. This then led to Bohr's suggestion that perhaps energy was not conserved in beta decay. Pauli countered this by formulating the neutrino hypothesis. Fermi then followed with his celebrated theory of beta decay. This in turn stimulated further investigation on the spectrum shape, which did not agree with Fermi's theoretical prediction. This led to other ideas, and the confusion was only cleared up completely after World War II, in 1949, by C.S. Wu and R.D. Albert.

New Horizons (1949-1953)

In 1946, the pion was not known. Fermi and Edward Teller had just completed their theoretical analysis of the important experiment of M. Conversi, E. Pancini and O. Piccioni. I attended a seminar by Fermi on this work. Where he arrived at the conclusion that the 'mesotron' (the observed particle) could not possibly be the carrier of strong forces hypothesized by Yukawa. Fermi's lectures were always superb, but that one to me, a young man not yet twenty and fresh from China, was absolutely electrifying.

One lucky break in my life was to have Jack Steinberger as a fellow student at Chicago, because he told us that the muon decays into an electron and two neutrinos. This made it look very much like any other beta decay, and stimulated M. Rosenbluth, C.N. Yang and myself to launch a systematic investigation. Are there other interactions, besides beta decay, that could be described by Fermi's theory?

We found that muon decay and capture resembled beta decay. This began the 'universal Fermi interaction'. We then went on to speculate that, in analogy with electromagnetic forces, the basic weak interaction could be carried by a universal coupling through an intermediate heavy boson which I later called W^+ for weak.

Naturally we went to Enrico Fermi and told him of our discoveries. He was extremely encouraging. With his usual deep insight, he immediately recognized the further implications beyond our results. He put forward the problem that if this is to be the universal interaction, then there must be reasons why some pairs of fermions should have such interactions, and some pairs should not. teractions, and some pairs should not. For example, why does the proton not decay into a positron and a photon, or into a positron and two neutrinos?

A few days later, he told us that he had found the answer; he then proceeded to assign various sets of numbers, +1, -1, and 0 to each of these particles. This was the first time to my knowledge that both the laws of baryon-number conservation and of lepton-number conservation were formulated together to give selection rules. However, at that time (1948), my own reaction to such a scheme was to be quite unimpressed: surely, I thought, it is not necessary to explain why the proton does not decay into a positron and a photon, since everyone knows that the identity of a particle is never changed through the

[1] *Professor T.D. Lee shared the Nobel Prize for Physics with C.N. Yang in 1957.*

emission and absorption of a photon; as for the weak interaction, why should one bother to introduce a long list of mysterious numbers, when all one needs is to say that only a few combinations can have interactions with the intermediate boson. (Little did I expect that soon there would be many others.)

Most discoveries in physics are made because the time is ripe. If one person does not make it, then surely another person will do it at about the same time. In looking back, what we did in establishing the universal Fermi interaction was a discovery of exactly this nature. This is clear, since the same universal Fermi coupling observations were made independently by at least three other groups, O. Klein, G. Puppi, and J. Tiomno and J.A. Wheeler, all at about the same time. Yet Fermi's thinking was of a more profound nature. Unfortunately for physics, his proposal was never published. The full significance of these conservation laws was not realized until years later. While this might be the first time that I failed to recognize a great idea in physics when it was presented to me, unfortunately it did not turn out to be the last.

In the early fifties, extensive efforts were made to determine the space-time transformation properties of beta decay and so give an insight into the underlying mechanisms. A 1953 experiment on helium-6 decay seemed to rule out the theoretical idea of the intermediate boson, and I became quite depressed.

The Theta-tau Puzzle (1953-1955)

During a recent physics graduate qualifying examination in a well-known
American university, one of the
American university, one of the questions was on the theta-tau problem. Most of the students were puzzled over what theta was; of course they all know that tau is the heavy lepton, the charged member of the third generation. So much for the history of physics.

In the early 1950s, theta referred to the meson which decays into two pions, whereas tau referred to the one decaying into three pions. Experiments showed that these mesons had different intrinsic parities (behaviour under mirror reflection), but on the other hand had the same lifetime and the same mass. This was the puzzle.

My first efforts were all on the wrong track. In the summer of 1955, Jay Orear and I proposed a scheme to explain the puzzle within the bounds of conventional theory. We suggested a cascade mechanism, which turned out to be incorrect.

The idea that parity (left/right symmetry) is perhaps not conserved in the decay of these particles flickered through my mind. After all, strange particles are by definition strange, so why should they respect parity? The problem was that, after you say parity is not conserved in these decays, then what do you do? Because if parity non-conservation exists only in theta/tau, then we already have all the observable facts, namely the same particle can decay into either two or three pions with different parity. I discussed this possibility with Yang, but we were not able to make any progress. So we instead wrote papers on parity doublets, which was another wrong try.

The Breakthrough (1956)

In 1956, I had second lucky break, this time because Jack was my colleague at Columbia. Discussing with him the definitions of the decay angles in the disintegration of hyperons (heavy relatives of the nucleon, carrying strangeness) I realized how non-conservation of parity might be revealed if the data were analysed the right way.

Very soon, Jack and his collaborators (R. Budde, M. Chretien, J. Leitner, N. Samios and M. Schwartz) had their results, and the data were published even before Yang and I published our theoretical paper on parity non-conservation. There was a suggestion that mirror symmetry was being violated in hyperon decays, but because of the limited statistics, no conclusion could be drawn.
Nevertheless, except for the high
Nevertheless, except for the high standard of Jack and his group, this might have been claimed as the first indication of parity non-conservations.

However, on the theoretical side there was still the question of parity conservation in ordinary beta decay. In this connection, about two weeks later, I had the further good fortune of having Yang join me. This led to our discovery that, in spite of the extensive use of parity in nuclear physics and beta decay, there existed no evidence at all of parity conservation in any weak interaction.

Several months later followed the decisive experiments by C.S. Wu, E. Ambler, R. Hayward, D. Hoppes and R. Hudson, at the end of 1956, on beta decay, and by R. Garwin, L. Lederman and M. Weinrich and by J. Friedman and V. Telegdi on other decays.

From then on we entered the modern period: theta and tau became the kaon, the transformation properties of beta decay were finally determined, and the weak interaction was unified with electromagnetism in the electroweak picture.

The Modern Period

At present, there seems to be a divergence in the viewpoints of theorists and experimentalists. The experimentalists are full of problems, looking for solutions - money problems, managerial problems, scheduling problems, etc. On the other hand, the theorists think they already have the ultimate solution and that there is no problem. Superstrings may well be the theory of everything (TOE), but how about calculating things like the Higgs mass, quark-lepton masses, etc.? Therefore, instead, I would like to go over our experience and try to extract not the laws of physics, but the laws of physicists.

We all know that to do high energy physics requires accelerators. When each new accelerator is proposed, theorists are employed like high priests to justify and to bless such costly ventures. Therefore it pays to look at the track record of theorists in the past, to see how good their predictions were before experimental results. Looking at the important discoveries made in particle physics for more than three decades, it is of interest to note that, with the exception of the antinucleon and the intermediate bosons W and Z^0, none of these landmark discoveries was the original reason given for the con-
struction of the relevant accelerator
struction of the relevant accelerator.

When Lawrence built his 184-inch cyclotron, the energy was thought to be below pion production. Therefore, after the cyclotron was turned on, even though pions were produced abundantly, for a long time nobody noticed them.

The progress of particle physics is closely tied to the discovery of resonances, which started at the Chicago cyclotron. Yet even the great Enrico Fermi, when he proposed the machine, did not envisage this at all. After the unexpected discovery of the first nucleon resonance, for almost a year Fermi expressed doubts whether it was genuine.

A similar story can be told about the next landmark discovery. When the Cosmotron was constructed at

Brookhaven, some of the leading theorists thought that the most important high energy problem was to understand the angular distribution of proton-proton collisions, which remains mysteriously flat even at a few hundred MeV, although at that energy the dynamics of the collision are quite complicated; many different levels are all involved. Why should they conspire to make a flat angular distribution? But as it turned out, when the energy increases the angular distribution of proton-proton collisions no longer remains flat and becomes quite uninteresting. Instead, it was production and decay dynamics of strange particles that put the Cosmotron on the map.

We could go on and on, and the same pattern would repeat itself. This leads to my first law of physicists: 'Without experimentalists, theorists tend to drift.' There is no reason for us to believe that it will change, nor should we expect too much from our present theorists for the prediction for the future.

The density of great discoveries per unit time is quite uniform and averages out to about one in two years. Let us hope that this long-standing record of constant rate of discovery can be maintained. In order to achieve that, we must have good experiments.

We now come to my second law of physicists: 'Without theorists, experimentalists tend to falter.'

A good example is the history of the Michel parameter, which governs the shape of the spectrum of the electrons produced in muon decay.

It is instructive to plot the experimental value of this parameter against the year when the measurement was made. Historically it began with zero and then slowly drifted upwards; only after the theoretical prediction in only after the theoretical prediction in 1957 did it gradually become 0.75. Yet, it is remarkable that at no time did the 'new' experimental value lie outside the error bars of the preceding one!

Scientific Theories and the Goal of Physics

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This article examines the logical structure of fundamental physical laws and draws attention to the importance of questions, as opposed to answers.

There is a tendency for scientists to regard the fundamental laws of physics as being derived logically from initial assumptions, in much the same way as mathematical theorems are derived axiomatically.

It is interesting to draw a rough analogy here with the legal system in our country. Before an Act of Parliament becomes law, there is a fairly long drawn-out consultative process followed by various votes and debates through Parliament. A particular law may be quite contentious and the voting may be very close indeed; also, shortcomings in the proposed law may be hotly debated and compromises reached as a result of stiff opposition. In the end, however, the law finally appears on the statute books and is applied by the courts, but in the application process the latter are not allowed to take into account Parliamentary debates prior to the Act becoming law. As far as the courts are concerned, Acts of Parliament simply appear out of the blue on the statute books. Here, the closed nature of the legal system is such that a strict demarcation between the law and its antecedents is both practical and desirable.

As far as scientific theories are concerned, there is a mythology that initial assumptions and axioms can be assumed at the outset, the theory following on logically afterwards with little (if any) regard to what led up to the axioms and assumptions in the first place. I say 'mythology' because, in spite of the way many theories are presented, such a logical progression from (a) to (z) belies the fact that (a) was only suggested in the first place by virtue of arguments the first place by virtue of arguments that emanated from (z) - or from somewhere else.

Scientific theories do not (and should not) have the closed nature of a legal system, although sometimes for convenience an artificial barrier or limit to the theory may be applied (for example events prior to the genesis of a Big Bang universe). However, we must not read too much physical reality into these constrictions on a theory, even though their removal may cause problems.

Logical Basis of Physics?

It is quite possible to practise physics very successfully without worrying about the logical basis of the subject, particularly as there is an empirical link with the real world. This is considered enough of a constraint to ensure that successful theories are not in need of any 'logical' justification, other than correspondence with the 'real world'. This opinion is unfortunately part of the mythology of science which has grown up as a result of the virtual abandonment of philosophical teaching to scientists.

I believe that there is a certain parallel between the state of physics now and that of mathematics some years ago. The foundations of most of what we refer to today as 'standard mathematics' had been discovered long ago, but at the expense of a proper logical basis. Even the validity of such concepts as negative numbers was disputed, although they had been used with great success for many hundreds of years. It was recognised that a proper formal basis for mathematics was needed, and when this was developed not only was existing mathematics better understood but new branches resulted.

Induction and Questioning

Science is based on the inductive method of generalising from data and extrapolating laws which are corroborated by further data. The paradox is that this method, however successful, can never lead to laws that represent the 'truth'. Following Sir Karl Popper, it is clear that whereas laws may be falsified in principle, they can never be validated by observations.

How is it that we know anything in science, given this unsafe methodology? One way of rationalising the situation is to look at an example of a successful set of laws, a good one being 'gravitation and the laws of motion'.

One of the first questions Galileo answered was 'how do objects fall?'. He answered was 'how do objects fall?'. He found that the distance an object falls is proportional to the square of the time it takes. Later, Newton answered the question 'what is the natural state of motion of objects and how do they affect each other?'. His answer introduced the notion of universal gravitation and Newton's laws. This was followed by Einstein's question 'what would happen if an observer approached the speed of light?'. His answer, embodied in his special and general theories of relativity, now leads to the question 'how are the fundamental forces of nature interrelated?'.

Note that all the answers supersede each other, rendering the preceding one either irrelevant or of only limited validity. The questions are the

I believe that the progress of science as we know it derives from just a sequence of questions. The answers we make to these questions, rather than leading to unattainable truth via induction, are at best of limited validity and at worst irrelevant. Successful questions serve only to lead on to deeper ones in the sequence. Irrelevance here is seen as much worse than being false; in many cases wrong answers to questions can lead to other theories and more questions, whereas irrelevance merely leads to a cul-de-sac.

I think that the importance of questions in physics - as opposed to answers - should be upgraded. We can make progress in science through inductive arguments even though the ultimate goal of truth is impossible. The future of fundamental physics is not seen here as the problem of finding the ultimate equations unifying the four forces of nature; it goes far beyond that to asking the right sequence of questions which shows how well we understand the universe.

Computational Physics

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By courtesy of: CERN Courier (January/February 1987)

Computers have for many years played a vital role in the acquisition and treatment of experimental data, but they have more recently taken up a much more extended role in physics research. The numerical and algebraic calculations now performed on modern computers make it possible to explore consequences of basic theories in a way which goes make it possible to explore consequences of basic theories in a way which goes beyond the limits of both analytic insight and experimental investigation.

This was brought out clearly at the Conference on Perspectives in Computational Physics, held at the International Centre for Theoretical Physics, Trieste, Italy, from 29 to 31 October 1986. It was directed by Fred James (CERN), Alvise Nobile (Trieste), and Claudio Rebbi (Brookhaven and Boston).

[1] *Prof. Fred James was a Director of the School on Advanced Techniques in Computational Physics (6 - 28 October) and Conference on Perspectives in Computational Physics (29 - 31 October), both held at ICTP in 1986.*

The birth of computational physics can be traced back to the late 1960s with the first journals, conferences and schools on the subject. Although enormous progress has been made since then and whole new fields such as lattice gauge calculations have started up, it is clear that computational physics is still in its infancy. In fact Ken Wilson (Cornell) in his invited talk compared the current situation in computational physics with that of experimental physics at the time of Galileo when important discoveries were made using rudimentary microscopes, telescopes and leaning towers. Four hundred years later, experimental physics has developed techniques capable of penetrating many orders of magnitude deeper into matter and into the universe.

By analogy we expect that four hundred years from now it will be possible to perform calculations many of orders of magnitude more complex, and future computing engines compared with those of today will be like today's particle colliders compared with the microscopes of Galileo's time.

For Wilson, perhaps the greatest algorithmic challenge facing contemporary computational physics is demonstrated by the problem of electronic structure. Here is an area where the basic theory, quantum electrodynamics, is known to be valid to an extremely high accuracy, sufficient to predict the physical, chemical, and biological properties of all atomic and molecular states. However current calculation techniques are barely powerful enough to compute gross properties of systems involving a few hundred electrons, and interesting chemistry and biology taking place in the region up to millions of electrons at least.
the region up to millions of electrons at least.

But the conference was not only devoted to dreaming about the future: world experts in many areas of computational physics reviewed the state of the art from all points of view: physics algorithms, software techniques, and hardware developments, as well as their interrelationships.

Supercomputer architecture was of course a topic of interest to all participants, and was covered in talks by several physicists and representatives of computer manufacturers. The physics areas covered in greatest detail, in addition to several aspects of electronic structure, were lattice gauge theory, stellar dynamics, and many-nucleon systems.

One of the highlights of the conference was the real-time demonstration of the possibilities of cellular automata by Tom Toffoli (MIT). In these discrete systems, each successive state is derived from the previous state by a relatively simple rule, which may be deterministic or partly random. By varying the replication rule, Tom was able to model many mathematical and physical phenomena from shock waves to fractal growth and stellar dynamics. Using his own special hardware board under the control of an Olivetti M24 personal computer he was able to calculate successive states of the automata with sufficient speed that large-screen colour projection gave an uncanny feeling of observing the evolution of complex continuous systems obeying known 'physical' laws. This provides considerable insight into important phenomena like order and disorder, phase transitions, stability and reversibility in physical systems.

The conference took place immediately after the School on Advanced Techniques in Computing in Physics, held also at ICTP and with the same organizers. The three-week school offered in-depth courses on pure computing topics (programming languages, operating systems, networking, etc.), numerical and symbolic analysis techniques, and physics applications, to 150 students selected from over 400 applicants, and coming mostly from the developing countries.

These 'students' turned out to be highly qualified computational scientists, which made for an unexpectedly lively and stimulating school. One of the lecturers even remarked that it was his most responsive audience, despite having given similar remarked that it was his most responsive audience, despite having given similar talks as seminars in some of the world's most prestigious laboratories. One explanation was the school's highly selective acceptance procedure, but it is also a clear sign that competence in computing is increasing fast in many less developed countries. Only a few years ago, access to a reasonable computer meant the installation of an entire computer centre, with everything from a false floor to a team of systems analysts, all of which was beyond the possibilities of many countries. Nowadays the same computer power is available just by plugging in a PC. The effects of this quiet revolution have been spectacular.

Most of the 150 School participants stayed on for the conference, where they were joined by about 70 more people (nearly all from Western Europe and North America).

One evening was devoted to an open discussion of the future of Computational Physics. The presence of large delegations from developing countries made it a natural forum for computational physics research in the poorer regions of the globe. One western participant said he had never before been at a conference where so many developing countries had been represented, and it clearly came as a surprise to him and others that it was not only possible to carry on computational physics research in such places, but that it was being done actively. The obvious conclusion is that this enormous source of intellectual potential cannot be neglected.

Ken Wilson's summary talk covered the major outstanding issues for computational science as he saw them and as they were brought out at the conference. His first point was: what is quality research? Or equivalently, what research will still be respected four centuries from now?

The great algorithmic challenges remain: electronic structure (solution of the Schrödinger equation), turbulence, function minimization (for protein folding, spin glasses, etc.), quantum field theory, and stellar evolution. The difference in the time-, length-, and energy-scales is impressive.

Communications issues are also very important. The usual language of Computational Physics, Fortran, has long been recognized as inadequate in many respects, especially as a vehicle for explaining what a program is expected to do. Yet none of the many other languages has had widespread acceptance.

There are other important aspects of the publication issue. Where should new
the publication issue. Where should new
papers be published, where and how should programs be published? The list of journals needed for a complete Computational Physics library is enormous. And there is networking, which both solves and introduces many problems, but does not obviate the need for centres of excellence.

The economic aspects of the research cannot be neglected. The future of Computational Physics will depend on how we interact with the large scientific computing market, which has a huge industrial base representing about $ 10 billion per year, and is truly international. With the technology advancing rapidly on many fronts, the prospects for Computational Physics are apparently limited only by our own skill and imagination.

A New International Centre in Yamoussoukro

An International Centre for Advanced Scientific Studies will be created in Yamoussoukro, Côte d'Ivoire. The idea was launched by Professor Abdus Salam, Director of the International Centre for Theoretical Physics, while visiting, upon invitation, Government and university officials in Côte d'Ivoire and other francophone African countries - Benin, Cameroon, Congo, Gabon, Mali, Niger, Senegal and Zaire - in January this year.

The President of Côte d'Ivoire, Mr. F. Houphouët-Boigny, strongly supports the idea and has immediately promised funds for a building and for the operation of the Centre whose Charter includes research and training in mathematical sciences, informatics, communication physics, biotechnology and geophysics.

The ICTP will, of course, assist the new institution and, as a first step, will hold there its 1988 Workshop on Microprocessors - Science and Technology . These workshops have been held in Trieste every second year since 1981 and in Sri Lanka, Colombia and the People's Republic of China in 1982, 1984 and 1986 respectively.

The coordinators of the new Centre in Yamoussoukro are Prof. Saliou Touré (Institut de recherches mathématiques, 03 B.P. 2030, Abidjan, Côte d'Ivoire) and Dr. J.P. Ezin from Benin, presently working at the ICTP as a Visiting Mathematician.

J. Chela- Flores Elected to the Latin-American Academy of Sciences

Professor Julian Chela-Flores from Venezuela has recently been elected a full member of the Latin American Academy of Sciences. He received the communication from Professor Carlos Chagas while at the ICTP as a guest scientist.

Prof. Chela-Flores was educated as a theoretical physicist at Chelsea College University of London where he took his B.Sc., M.Phil. and Ph.D. degrees . He has also taken a strong interest in theoretical biology as well as in the relation of science and technology to economics and social development.

He worked at the Instituto Venezolano de Investigaciones Científicas (IVIC) as an Associate Researcher first and then as Full Associate Researcher from 1971 to 1978. He became a Full Professor at the University Simon Bolivar (USB) in 1978 and has held an external professorship at the Instituto Internacional de Estudios Avanzados (IDEA) since 1981. He was Dean of Research at USB from 1979 to 1985.

His connection with the ICTP goes back to 1971 when he came for the first time as a research fellow. He was appointed as an Associate Member from 1972 until 1974 and a Senior Associate Member from 1976 to 1985. He paid several additional visits to the ICTP as a guest scientist.

Professor Chela-Flores has published 37 scientific papers.

ICTP Consultant in Dar-es-Sala

Dr. L.K. Shayo has been appointed as a Consultant to the ICTP for all matters relating to the coordination of the mathematics activities in Africa carried out in cooperation with the ICTP, and to the Workshop on Fabrication of Physics Equipment to be held in East Africa. Dr. Shayo has been an Associate Member and a visiting mathematician at the ICTP.

His address is:
Department of Mathematics
University of Dar-es-Salaam
P.O. Box 35062
Dar-es-Salaam
Tanzania
Tanzania

Wigner Medal

Professor Feza Gürsey from Yale University received the Wigner Medal from the Group Theory Foundation in October 1986 "in recognition of his essential role in the discovery of symmetries in particle physics". Professor Gürsey was one of the first Associate Members of the ICTP. He had been appointed in 1964 together with Professor Riazuddin (Pakistan), J. Tiomno (Brazil) and B.M. Udgaonkor (India) while he was working at the Middle East Technical University in Ankara (Turkey) as a Professor of

Physics. He was named Josiah Willard Gibbs Professor of Physics at Yale University in 1977.

The Wigner Medal is awarded every second year "for outstanding contributions to the understanding of physics through group theory".

ICTP Activities during the First Quarter of 1987

Five activities were on the 1987 ICTP Calendar for the first quarter. They were: the Third International Workshop on "Total Energy and Force Methods", the Second Workshop on Mathematics in Industry, the International Workshop on Remote Sensing and Resource Exploration, the Spring College on Geomagnetism and Aeronomy and the Winter College on Atomic and Molecular Physics.

The Workshop on Mathematics in Industry (2 - 27 February) was directed by Professor H. Neunzert (Kaiserslautern, Federal Republic of Germany), Prof. A. Fasano (Florence, Italy) and Prof. C. Storey (Loughborough, UK). The Workshop included a symposium during its last week. The topics for discussion included free boundary problems, optimization theory, control theory, system theory, identification and model reduction and numerical aspects. This issue includes an article by H. Neunzert in which he illustrates his views on the subject.

Remote Sensing and Resource Exploration was the topic of another international workshop (9 February - 6 March) the programme of which was set up by Dr. F. El Baz (Boston, USA) and Dr. V. Cappellini (Florence, Italy).
Dr. V. Cappellini (Florence, Italy).
Remote sensing, methods and techniques, data acquisition and handling, monitoring of change in the environment, mapping and charting were comprehensively reviewed in the first part of the workshop while case studies of land resource surveys, mineral detection, underground water exploration and agricultural and forestry resources were discussed in the second part. The remainder of the Workshop was dedicated to the discussion of working groups reports on the use of remote sensing in developing countries and on data bases and their utilization.

Remote sensing has made great progress in the last few years thanks to the American satellites of the Landsat series which have an image resolution of 80 metres and are able to record visible as well as infrared light, and thanks to the French "Spot" with a resolution of 10-20 metres. Another step forward is due to the use of a radar for topographical surveys from the space in two of the US Shuttle missions.

From 2 to 27 March, the ICTP held a Spring College on Geomagnetism and Aeronomy which covered the following main topics: the Earth's main magnetic field, its secular variation and model of the core field; electromagnetic induction processes; magnetotellurics and magnetovariational studies; upper atmosphere with special emphasis on the ionosphere, equatorial electrojet currents and field, radar observations, radio wave propagation through the ionosphere, plasma irregularities, electric current in the Earth's environment; hydromagnetic waves, magnetic pulsations - production, propagation and interplanetary control; geomagnetic disturbances, magnetospheric processes, sub-storms, indices of geomagnetic activity; solar, interplanetary and planetary magnetic fields and planetary ionospheres; and studies of the upper atmosphere through optical airglow.

The study of the magnetism of the earth, a field which only thirty years ago seemed to have provided answers to all problems, has instead made considerable progress but still presents many intriguing features. The complexity of the investigation is due to the fact that it involves a large number of variables and these cannot be treated in a scale model. Ionospheric phenomena involving the interaction with the outer space are also difficult to reproduce in the laboratory. However, the use of satellites is extremely useful for studying these problems.

This College was a continuation of one of the major activities of the Interdivisional Commission on Developing Countries (ICDC) of the International Association of Geomagnetism and Aeronomy (IAGA). It was directed by Prof. R.G. Rastogi (Bombay, India), F. Mariani (Il Universita' di Roma) and G.K. Rangarajan (Bombay, India).

The fourth activity was the Winter College on Atomic and Molecular Physics (9 March - 3 April), directed by Profs. E. Arimondo (Pisa, Italy), S.R. Svanberg (Lund, Sweden) and B.C. Tan (Kuala Lumpur, Malaysia). It covered the following chapters: atomic and molecular structure and spectra; analytical laser spectroscopy (sample chemical analysis); combustion diagnostics; diagnostics for semiconductor fabrication processes; diagnostics of plasmas; isotopic selective processes and diagnostics; atmospheric diagnostics; hydrospheric diagnostics; spectroscopic plant diagnostics; surface diagnostics (fundamental and industrial); and medical diagnostics using optical spectroscopy.

Total Energy and Free Electron Lasers was the topic of an international workshop which was held from 14 to 16 January and which concentrated on the following subjects: novel techniques for self-consistent ab-initio computations; electronic correlation; applications of molecular dynamics; structural stability of clusters, surfaces, interfaces and bulk solids; structural phase transitions; ab-initio lattice dynamics and electron-phonon interaction; and lattice relaxations.

The programme was set up by an international committee headed by prof. A. Baldereschi (Trieste, Italy, and Lausanne, Switzerland).

A total of 420 scientists accounting for 230.31 man/months took part in these activities. Two hundred and seventy-four of them were from developing countries.

Research Scientists and Associates for the International Centre for Genetic Engineering and Biotechnology

The ICGEB has asked us to circulate the note which follows. As ICTP has developed a close collaboration with ICGEB, we shall be grateful to all readers who pass on the information contained in
who pass on the information contained in
this note to their colleagues working in Biotechnology.

The International Centre for Genetic Engineering and Biotechnology (ICGEB) is an intergovernmental organization being established by 39 countries as a centre of excellence devoted to the application of genetic engineering and biotechnology to accelerate economic development. The United Nations Industrial Development Organization (UNIDO) assists the member countries of the ICGEB in establishing the Centre and is currently implementing an interim programme for a period of three years by which time the Centre is expected to function as an autonomous intergovernmental organization.

The Centre has two Components, Trieste, Italy, and New Delhi, India. Early work in Trieste concerns molecular aspects of DNA replication in human cells and the molecular, immunological and pharmacological aspects of human papilloma and rotavirus infections. At New Delhi the initial focus is on the molecular aspects of plant biology, hepatitis virus and parasitology with special emphasis on protozoan infections. The assignment of successful candidates will be based on the Component of applicant's preference and the availability of the requisite research topic.

The Centre is under the directorship of Prof. Irwin C. Gunsalus. The Trieste Component is headed by Prof. Arturo Falaschi. Prof. K.K. Tewari, proposed by the Director as head designate, is advising him on the development of the New Delhi Component. Positions are now available as follows:

Research Scientists: Research Scientists are being recruited at levels from Assistant to Senior Research Scientist, with equivalence to academic attainment of Assistant to full Professor at major internationally recognized universities. Ph.D. candidates with recent postdoctoral experience, to be appointed at the assistant Research Scientist level, can,after in-depth review according to the Centre's guidelines, receive promotion. For candidates with established recognition in their scientific field and demonstrated experience and leadership, senior appointments are available. Fluency in English is essential.

Research Associates: Research Associate appointments will be available for participation, in Trieste and New Delhi, with Research Scientists in the areas listed above. Recent Ph.D. graduates in the physics in biological sciences with emphasis on chemistry, biochemistry, molecular and cell biology. Immediate research interests include molecular genetics, plant and animal molecular biology, molecular virology, parasitology, bacterial physiology and fermentation. Preference will be given to candidates with publications in peer reviewed journals with strong training in chemistry and biology.

In general, initial appointments will be for one to three years depending on the experience and qualifications with salaries and allowances according to U.N. system scales and conditions of employment. Research associates will receive a stipend under special contract arrangements.

Please send résumé with three letters of recommendation to Prof. I.C. Gunsalus, c/o Mr. H. Creydt, Head, Project Personnel Recruitment Branch, UNIDO, P.O. Box 300, A-1400 Vienna, Austria.

ICTP Statistical Digest for 1986

The statistical data which follow illustrate in a quantitative way the activity of the ICTP in 1986. They show the participation of scientists expressed in number of scientific visitors and in number of man/months in the various components of the activities of the Centre, i.e. (a) research, (b) training-for-research (extended courses, workshops, conferences and other meetings), (c) training at Italian laboratories and (d) major courses and workshops held outside Trieste through the ICTP office of External Activities, as well as the breakdown according to geographical area. Two histograms show the evolution of participation in the ICTP activities since 1982 in number of scientific visitors and man/months.

Evolution in the Last Five Years

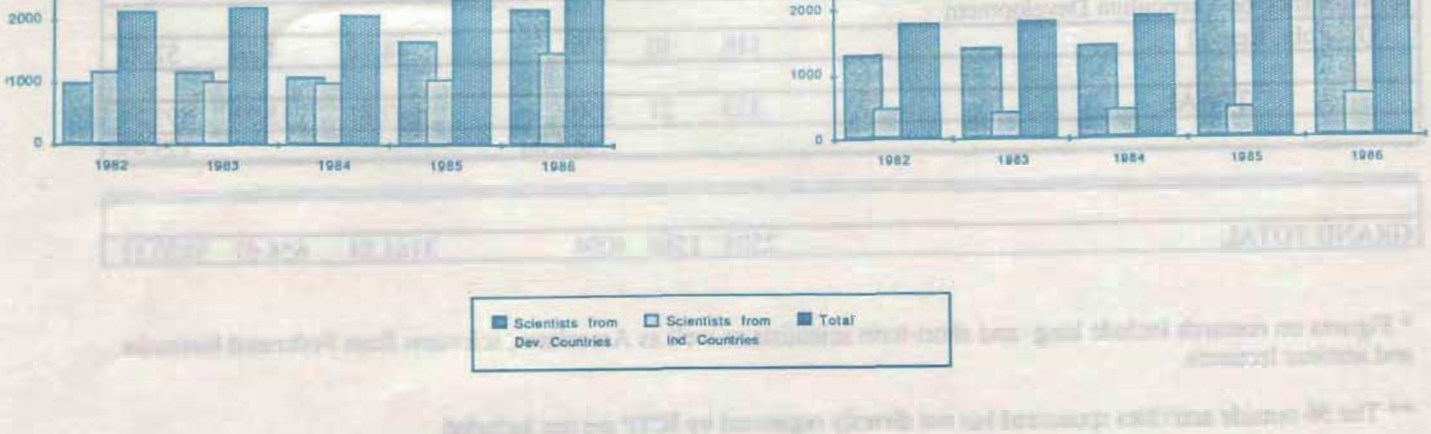

Table I

No. of scientific visitors:			
	from developing countries	2180	
	from industrialized countries	1471	
	TOTAL		3651
No. of man/months:			
	from developing countries	3146	
	from industrialized countries	674.5	
	TOTAL		3820.5
No. of activities (courses)		36	
No. of countries represented		109	
No. of Associates (visits)		131	
No. of Affiliates (visits)		430	
No. of preprints		401	

Table II

Statistical Summary of the Activities held at and outside ICTP

	No. of Visitors			No. of Man/Months		
	Dev.	Ind.	Total	Dev.	Ind.	Total
At ICTP:						
(a) Research: High Energy	133	80	213	295.41	152.88	448.29
Condensed Matter	94	18	112	185.90	48.29	234.19
Mathematics	71	6	77	166.64	24.54	191.18
Other	182	61	243	212.62	13.71	226.33
TOTAL	480	165	645	860.57	239.42	1099.99
			(15.8%)			(28.8%)
(b) Training for Research (courses)	1705	1394	3099	1423.46	403.32	1826.78
			(75.7%)			(47.8%)
Outside ICTP:						
(a) Training at Italian Laboratories	127	-	127	773.92	-	773.92
(b) Regional College on Microprocessors (Hefei, China)	80	17	97	55.17	11.73	66.90
(c) Workshop on Curriculum Development (Nairobi, Kenya)	116	10	126	48.72	4.20	52.92
TOTAL	323	27	350	877.81	15.93	893.74
			(8.5%)			(23.4%)
GRAND TOTAL	2508	1586	4094	3161.84	658.67	3820.51

* Figures on research include long- and short-term scientists as well as Associates, scientists from Federated Institutes and seminar lecturers.

** The 56 outside activities sponsored but not directly organized by ICTP are not included.

Table III

Training for Research Activities in Chronological Order and Participation

Title	Dates	Scientists from Dev. Countries	Total
Winter School Epitaxial Electronic Materials	*13 - 24 January*	52	108
School on Physics in Industry	*27 Jan - 14 Feb*	80	104
Symposium on "Perspectives in Particle Physics"	*6 - 7 February*	21	60
Workshop on Reactor Physics	*17 Feb - 21 March*	61	81
Workshop on Optical Fibre Communication	*24 Feb - 21 March*	68	90
International Conference on Synchrotron Radiation	*7 - 11 April*	15	77
Spring School and Workshop on Superstrings	*7 - 18 April*	63	218
Workshop on Medical Diagnostic Equipment	*14 - 19 April*	7	20
Spring College in Condensed Matter on "Order and Chaos"	*21 Apr - 13 June*	116	156
Workshop on Solar and Wind Climatology	*21 April - 16 May*	61	91
Workshop on Dynamical Systems	*20 May - 6 June*	48	87
Summer Workshop in High-Energy Physics	*30 June - 15 August*	100	146
Research Workshop in Condensed Matter	*16 June - 5 Sept*	195	247
Quantum Chaos	*17 - 20 June*	37	80
Dynamical Screening and Spectroscopy of Surfaces	*24 - 27 June*	10	64
Relativistic Many-Body Problems	*30 June - 4 July*	10	85
Heavy Fermion Systems	*15 - 18 July*	16	44
Environmental Physics: Atmospheric Aerosols	*22 - 25 July*	15	40
IV Trieste IUPAP Semiconductors Symposium	*28 July - 1 August*	10	55
School on Applications of Nuclear Gamma Spectroscopy	*11 - 16 August*	12	13
Physics of Condensed Matter at High Pressures	*11 - 29 August*	33	47
Colloque sur la Science des Matériaux pour l'Energie	*26 August - 11 Sept*	54	71
Physics of Structure and Complexity	*2 - 5 Sept*	17	56
Workshop on Curriculum Development in Physics, Mathematics and Computer Science (Nairobi, Kenya)	*1 - 13 Sept*	116	126
Autumn Course on Seismology	1 Sept - 3 Oct	89	117
Third Summer College on Biophysics: Membranes	*8 Sept - 10 Oct*	92	121
Spinors in Physics and Geometry	*11 - 13 Sept*	18	47
Workshop on Global Differential Geometry	*15 - 19 Sept*	13	54
Regional College on Microprocessors (Hefei, China)	*22 Sept - 17 Oct*	80	97
Workshop on "Aspects of Confinement."	*29 Sept - 3 Oct*	25	57
School on Advanced Techniques in Computational Physics	*6 - 31 Oct*	109	196
College in Neurophysics: "Organization of the Brain"	*13 Oct - 7 Nov*	63	100
Topical Meeting on "Astrophysics Particles"	*3 - 5 Nov*		16
Second Autumn Course on Mathematical Ecology	*10 Nov - 12 Dec*	126	196
Workshop on Representation Theory of Lie Groups	*10 - 28 Nov*	64	82
Workshop on Representation Theory of Lie Groups	*10 - 28 Nov*	64	82
Meeting on Elementary Particle Phenomenology	*24 - 27 Nov*	4	64

Future Activities at ICTP

1987	
Spring School and Workshop on Superstrings	*1 - 15 April*
School on Polymer Physics	*27 April - 15 May*
Espaces fibrés: leur utilisation en physique	*27 April - 1 May*
Workshop on "Nonlinear Charge Density Wave Systems"	*4 May - 17 July*
Workshop on Surface Science and Catalysis	*4 - 9 May*
Spring College in Materials Science on "Metallic Materials"	*11 May - 19 June*
Third Workshop on Perspectives in Nuclear Physics at Intermediate Energies	*18 - 22 May*
Spring College on Plasma Physics	*25 May - 19 June*
ICFA School on Instrumentation in Elementary Particle Physics	*8 - 19 June*
New Scales Effects on Low-energy Precision Experiments	*22 - 24 June*
Research Workshop in Condensed Matter, Atomic and Molecular Physics	*22 June - 4 September*
Synchrotron Radiation and Free Electron Lasers	*23 - 26 June*
Summer Workshop in High Energy Physics and Cosmology	*29 June - 7 August*
One-dimensional Organic Conductors: Chemistry, Physics and Applications	*30 June - 3 July*
High Temperature Superconductors	*6 - 8 July*
Vacuum in Non-relativistic Matter-radiation Systems	*14 - 17 July*
Scanning Tunnelling in Microscopy - Fundamental Experimental and Theoretical Progress	*28 - 31 July*
Interatomic Forces in Relation to Defects and Disorder in Condensed Matter	*11 - 14 August*
Working Party on "Physics of Porous Media"	*17 - 28 August*
Workshop on Materials Science and the Physics of Nonconventional Energy Sources	*26 August - 18 September*
The Path Integral Method with Applications	*1 - 4 September*
Workshop on Telematics	*7 September - 2 October*
Workshop on Economics, Modelling, Planning and Management of Energy	*14 - 25 September*
Workshop in Interaction between Physics and Architecture in Environment Conscious Design	*21 - 25 September*
Fourth College on Microprocessors: Technology and Applications In Physics	*5 - 30 October*
College on Soil Physics	*2 - 20 November*
College on Riemann Surfaces	*9 November - 18 December*
Second Workshop on Cloud Physics and Climate	*23 November - 18 December*
1988	
College on Variational Analysis	*11 January - 5 February*
Second School on Advanced Techniques of Computing in Physics	*18 January - 12 February*
Workshop on Complex Analysis	*8 - 19 February*
Workshop on Applied Nuclear Theory and Nuclear Technology Applications	*15 February - 18 March*
College on Laser Physics: Semiconductor Lasers and Integrated Optics	*22 February - 11 March*
Workshop on Optical Fibres	*14 - 25 March*
Conference in Biotechnology	*21 - 25 March*

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

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