fore 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 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 larger 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.

By Fred James

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At the ‘Jackfest’ marking the 65th birthday of Jack Steinberger (see July/August 1986 issue, page 29), T. D. Lee gave an account of the history of the weak interactions. Lee was a graduate student with Steinberger under Enrico Fermi in Chicago from 1946, and went on to win the 1957 Nobel Physics Prize with C. N. Yang for their suggestion that the weak interaction does not conserve parity (mirror symmetry). This edited version* omits some of Lee’s tributes to Steinberger, but retains the impressive insight into the subtleties of a key area of modern physics by one who played a vital role in its development.

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 Wilson, von Baeyer, 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 pio-

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* The full version is available as a ‘Yellow Report’ No. 86-07 from CERN Scientific Information Service.
neers (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 an 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 Baeyer 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 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 $W^\pm$ 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. 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, $+1, -1, 0$, 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 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 questions was on the theta-tau problem. Most of the students
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were puzzled over what theta was; of course they all knew 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 standard of Jack and his group, this might have been claimed as the first indication of parity non-conservation.

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 V and Z⁰, none of these landmark discoveries was the original reason given for the construction 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

(Photo Joe Pineiro, Columbia)

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 of 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 or 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 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!