PHYSICS OF MESON FACTORIES

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I believe that everybody is inclined to agree that speaking of any Physics or Science as it will be in five to ten years from now is largely pure "science fiction"; were this not so I would have a feeling of sympathy and also of commiseration for the generations to come. Instead, I envy them. The statement I made cannot of course be proved, but made plausible to everybody who, as I did, would like to look at the several programmes conceived in this way for all the large accelerators built in the past, from the Far East to the Far West. Of course, there is good and bad "science fiction" just as there is good and bad science. The difference is mainly due to quality of the imagination. The Nautilus of Jules Verne or his "Travel to the Moon" was certainly science fiction of outstanding quality, but very few may pretend to be such a genius as Verne. I, in particular, do not suffer from this illusion. Thus I shall avoid speaking of programmes, that is of experiments which unavoidably are a more or less direct extrapolation of today's situation, an extrapolation for which none of you needs my help.

So, instead of going along in this way and saying something which has been repeatedly and well presented by several persons in many places, I would like to introduce some opinions which are probably quite personal and at best can be considered as expression of the Devil's Advocate (or His Majesty's Opposition).

First. I feel that everybody is inclined to agree that one should not distinguish between Physics and Physics. For instance, I am inclined to think that theoretical and experimental physicists are different mainly from the competence they have acquired in the use of one specific technique more congenial to their way of thinking. Also, algebra and geometry are, and were, very frequently two different but equivalent ways to solve mathematical and physical problems, as shown, for instance, by Galileo, by Newton, etc.

For this reason, and I would say "a fortiori", one may be very reluctant to make a distinction between low, fairly low, high, very high, and extremely high energy physics. In only one sense will I personally be ready to support strongly a distinction between branches of physics, and this sense is determined by the epistemological value of the problems challenging the human mind. For instance, I admit that I am among the persons (and I believe there are quite a few) who would consider it more valuable for us, and for the generations to come, to invent some experiment on gravitons and gravitational waves (if they do exist) than to send a man on to the

Moon. The preference for the man on the Moon is largely due to a naïve concept of science, and also to the fact that it is easier by far to send a man to the Moon than it is to invent some crucial experiment on gravitation. In the latter field also, the Jules Verne imagination would not be able to make any good science fiction. If the epistemological value of science is considered,(and I feel that this is one of the obvious definitions of science) it is certain that today one may guess "a priori" the great epistemological value of the "elementary particles physics" for the years to come. But the development of this branch of physics is not, as we may argue on the basis of the last ten years' experience, a monopoly of the high or extremely high energies. Actually the history of the last ten years may be summarized by a Wolfenstein sentence : "For those who have wandered through the murky channels of the complex plane, as is popular these days, you undoubtedly have been told that the high-energy physicist who does experiments at very small angles (let us say Cocconi and company) is nothing more than a low-energy physicist lying on his side".

To explain a little the content of this sentence, let me drop the murky complex plane and consider some examples in the more familiar momentum diagrams.

Fig. 1 is what is called "the one-pion pole in pion production". The solid lines are a proton and an antiproton; the dotted ones are pions on or off the energy shell. The bubbles are the usual corruption of the Feynman vertices. If you look at the figure upwards, it is the reaction

$$p + \vec{p} \rightarrow 3\pi$$

that is, a proton-antiproton annihilation at rest via an intermediate one-pion state, having the same quantum number as the initial and final states. The lower half part of the diagram represents the fundamental π-N interaction and is determined by the coupling constant f^2; the upper half part is the $\pi + \pi \rightarrow \pi + \pi$, that is the pion-pion interaction; alias a way to produce the pion resonances. The diagram can now be read sideways. Then it represents the reaction $\pi + N \rightarrow N + \pi + \pi$ and then, again using the best energy interval, i.e. pions between 2 - 3 GeV, the most appropriate reaction to study (as shown, for instance, by the Bologna-Saclay group) the detailed features of the ρ, etc. Near threshold and guided by the same figures, one may instead look to the so-called ABC interaction, as in the experiment now in progress at the CERN Synchro-Cyclotron.

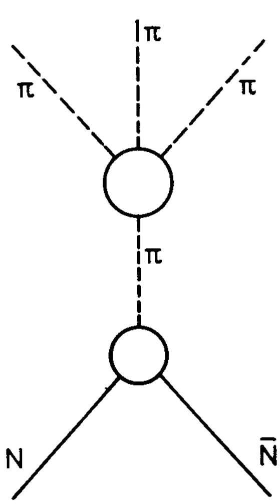

Fig. 1.

Similar considerations could be made comparing the electron-proton scattering à la Hofstadter with PAPEP, i.e. the $\bar{p}p$ annihilation into $(e^+e^-)(\mu^+\mu^-)$ pairs, the quasi-elastic p-p scattering à la Cocconi and company with the fairly low-energy π-N interactions which showed some time ago the resonant states of the π-N system, etc.

So it is clear that in these experiments the "energy" enters mainly as a technical parameter which has to be widely changed according to the approach follwed in trying to solve the same problem. Any text-book of physics starts with the enunciation of what may be called the principle of relativity of the methods of observation. This is not a trivial matter. It tells us that the way in which we describe and represent the natural facts is, or aims to be, unique as an everyday blessing for us, when one takes into account the intellectual pride and comfort from the fact that nature is congenial to us as we are congenial to it. The Avrogadro number and the Planck constant can be, and have been measured in ten different <u>really independent</u> ways, showing that some good physics was done in the past with a more fertile experimental imagination than that which may be discovered by comparing, for instance, today's programmes of the Brookhaven AGS and CERN PS machines. However, the argument could be generalized and somewhat reinforced (at least if one bases one's considerations on the past of electrodynamics) by making some direct comparisons between (very high intensity - fairly high energy) <u>versus</u> (extremely high energy - fairly high intensity).

Well known at CERN is the g-2 experiment in comparison with the high-momentum transfer μ-scattering experiment. In many respects (apart from some hidden singularities which one may suspect and has to check between an integrated function and the function itself), the very low-energy g-2 experiment is equivalent to the μ-scattering, at least up to about 1 GeV/c momentum transfer. It is true, of course, that the experiment of μ-scattering may become extremely interesting for momenta higher than 1 GeV/c; however, I am inclined to think that if, as I warmly hope, the Farley storage ring will work, it may provide results the value of which might not be inferior to those obtained with a direct and conventional μ-meson beam of average energy ten times higher. And this is apart from the consolation of having dropped a conventional experiment in favour of one conceived with some imagination. The list may continue, but as an exemplification I feel that you have had enough.

<u>Second</u>. The general aspect which I would now like to consider is what one may call the human-technical shadow of the machine size.

Let me start by making clear some assumptions, which may be wrong - and you will correct me. In the frame of my ignorance concerning machines I assume : 1) That it is possible to build proton accelerators in the range between 500 MeV and 3 - 4 GeV, able to produce correspondingly π and K beams with energies between 300 MeV and 1 GeV and with momentum spreads of the order of 10% and angular widths of a few degrees, having intensities at least one to two orders of magnitude higher than the beams which one may obtain with any other accelerators from 30 GeV upwards, including all possible known tricks to slow down particles. 2) That if one extends the energy up to 7-10 GeV, apart from the cost problem, a similar assumption could be made including the antiprotons, of course shifting the maxima of the energies for π and K. 3) That, as a normalization, the cost of an accelerator of 3-4 GeV with proton currents of around 10^{14-15} per second is around 150 million Swiss francs. (Johnson and Zilvershon, PA/WP/17, 1963).

Apart from the validity of these assumptions, I am fully aware that this is, even more than the first aspect previously discussed, a personal matter. My physics started in a small room in Arcetri, where Rossi and his co-workers had three experiments progressing at the same time. This was the period in which my main contribution to physics was the construction of Geiger-counters according to the secret prescription of Bothe. With all my deep veneration for Professor Bothe, the prescription was senseless and cost me hours and hours of painful restraint on my metabolism.

However, it is a fact that in my life physics went from experimental layouts of the order of 1 m^2, to layouts (neglecting the machine size) of the order of 300 m^2. Actually, such is roughly the extension of the neutrino experiment now in preparation at CERN. It is true that at the same time I went from the bicycle to the Caravelle, but I did not work with them. I used them, and this makes a great difference. Well, with these premises the point is that a high intensity - fairly high-energy accelerator may offer the advantage of being handled in a more personal and human manner; an aspect which may be for a while quite significant not only for the one-hundred-times made considerations upon personalities, individualism, etc., but also from the technical point of view. Here the size enters in two ways : 1) the equipment is more manageable and it is easier to have an over-all control of the experiments; 2) the development of new, really new, techniques starts always in a small way. The growth of emulsions from 30 to 3000 microns; the bubble chamber from 10 cm^3 to 2 m^3, tec., illustrate this point. For instance, I feel obliged to believe that superconducting magnets, polarized targets, systems of semiconductor detectors, and Charpak-type chambers (I hope) will follow the same pattern.

In the recent discussions on pion-factories, such as those summarized in the papers by Lapostolle and Michaelis, and Marshall and Wolfenstein, a list of very sensible experiments has been considered with some realism, where what is missing so far are either factors of 10-20 (not 1000!) in the intensity, or some compact wide-aperture quadrupole or magnet. I may, for instance, mention the μ^- capture in hydrogen and D gaseous targets, the weak decays of the π and K as far as all spectra, branching ratios, etc., are concerned. These are good simple examples and I would like to make some comments on them, limiting my considerations to a corner which could be easily extended. It is unnecessary to emphasize that the muon is the most mysterious particle we know, not because of any strange characteristics but for its too normal behaviour as a pure charged lepton, and lepton means light.

As far as we know, the μ meson, as the electron, interacts only electromagnetically and in pair with a neutretto, with the e-ν pair and weakly, of course, with all strongly interacting particles. As long as one believes that mass means interaction-energy, one is supposed to attempt to start by finding some small indicative difference among the electromagnetic and weak interaction of electrons and muons; and, of course, the more promising trend seems to be that of comparing the μ and the electron behaviour at higher and higher energies. Actually, one may naîvely expect to start to find something at momentum transfers of the order of the μ mass. For the electromagnetic field alone this is already excluded; but as far as the weak interactions are concerned, I do not think we can say the same. The tests of the Universal Fermi Interaction, i.e. of the validity limits of the Puppi triangle (if for a moment we neglect the strange particles) are not better than a few per cent, and the upper limit of the neutretto is at present extremely poor, i.e. eight electron masses. If one includes strangeness, the situation is by far more obscure. So, besides higher and higher energies I feel that also with higher and higher intensities and then very good collimation, studying decays and captures at very high rates, all these error limits can be pushed down to 10^{-3}-10^{-4}. Well known examples are those of the $\pi \to \mu + \nu_2$, $\pi \to e + \nu_1$ and the $\pi \pm \pi^0 + e + \nu$ decays. They are related not only to the universal Fermi interaction, but more specifically to the so-called weak magnetism effects in the μ and electron captures. So they are also strongly dependent on the description of the pion cloud. Actually the clothed nucleon absorbes the μ, and the e also, via intermediate π states of which again the most simple is the one-pion term, Fig. 2. This is equivalent to saying that we may compare the normal decays

$$\pi \to \mu + \nu_2$$
$$\to e + \nu_1$$

with pions in the energy shell, with the same reactions determined by virtual pions. This is not exactly **the same thing** because the Compton wave-length of the π and μ are comparable and 100 times smaller than that of the electron.

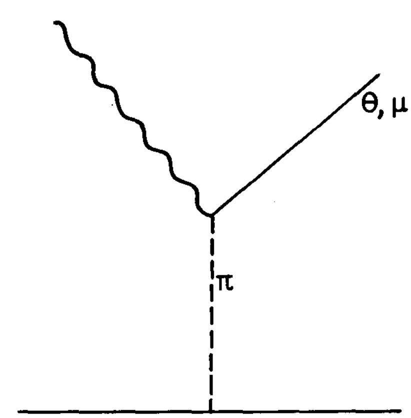

Fig. 2.

You may tell me now that this looks very much like science-fiction; so as a compromise I would like to affirm that the accurate study of the weak decays indicated before, together with the K decays, might be a mine of results of great epistemological value. In this respect one may consider that with high intensities and then good collimations many of these decays may be studied not only at rest but also in flight, i.e. in vacuum, without previous slowing down processes, taking full advantage from the experimental point of view of the Lorentz transformation as far as solid angles and detection apparatus are concerned.

Finally, switching to the always most relevant problem of beams, and going back to the μe mystery, let us consider proton-type accelerators such as those considered by L. Marshall in the Los-Alamos report of September 1962. That is, let us consider a 3 BeV 10^{15} protons accelerator. With angles of 5×10^{-4} sterad and momentum resolution of 3%, one may expect

1×10^{10} pions at 1 BeV/c,
5×10^{10} pions at 350 MeV,
1×10^{11} pions at 200 MeV.

With these pion beams one may hope to have, with a decay channel of about 20 m, a completely symmetric beam of 50/50 neutrinos and neutrettos with energies ranged between 100-300 MeV and fluxes ranged between 10^{10}-10^{11} neutrinos and neutrettos per square metre.

To end about neutrinos and weak interactions, let me remind you also of the possibility of obtaining under excellent experimental conditions high intensity monochromatic flux of neutrettos of 235 MeV, by stopping an intense beam of 500-100 MeV K^+, produced with an extracted proton beam in an external target.

Third. I have spoken so far mainly of weak interactions. I did not make any particular reference to strong interactions. Firstly, I do not feel competent enough; secondly, there are now many sound hopes for a quickly changing situation, and then for them the "bad fiction" is almost taken for granted. Thus, I only wish to make a vague remark. Strong interactions have been and probably will continue to be the highway to the discovery of new particles and to the determination of their quantum numbers. An isolated particle is a limited concept as the principle of inertia. Via an iteration procedure, a particle begins to exist because it interacts with another one, previously known through other interactions with even older particles. The measurements of the particle quantum numbers are always derived by symmetry, alias conservative principles acting as a limiting frame to the interactions. However, and for these reasons, one may believe that a great deal of unknown dynamics will come in a more understandable way when those symmetries are violated, allowing typical semi-forbidden transitions, etc. At least in this respect one may then consider that the availability of high intensity beams should also be very valuable for the improvement of our knowledge of strong interactions.

Fourth. In order to support not the validity of my convictions but my stubbornness regarding them, let me make a confession. Very often at CERN, and not only in the bad times, I dreamt of being a magician able to convert the PS into a (let us say) 8-10 GeV Argonne-type accelerator having less than half energy but with a factor 10 higher intensity. As a dream, it was after all not too imaginative. Considering it as a factory of π, K, $\bar{p}$ and ν, I felt many times and very strongly, that almost all physics done so far with the PS would have been done under better conditions. This is of course obvious if you consider the neutrino story. With a factor 10 higher intensity, one would not need to be so clever as the people of the Columbia group. Now we are anxious to get a few neutrinos above 3 GeV for the search of the heavy boson, and of course I am among those who at present instinctively consider those highest energy neutrinos almost the only useful neutrinos produced by the Van der Meer horn; but the heavy boson was not the first goal of the high-energy neutrino physics, and will not be the last. It may also be that its mass is heavier than 1.5 GeV. Then some convincing indication of it could be first found, for instance, in some less direct experiments, such as the radiative decays of π and K, or in precision measurements of the μ-decay spectrum. Here again I quote Wolfenstein, saying that the existence of the <u>two</u> neutrinos was already very strongly suggested by the lack of the $\mu \to e + \gamma$ transitions, as Neptune was discovered before having been seen by telescopes, by the orbit-perturbation theory. I may also add that, with a not incredible development of the present techniques, the evidence for or against the existence of the heavy boson might be found with less trouble from the shape of the spectra via the reactions

$$N + \bar{N} \to (e,\mu) + (\nu_1,\nu_2)$$
$$\bar{\mu} + p \to n + \nu \,.$$

The first is nothing else than one of the possible developments of the PAPEP experiment. But as I said, the dream of an Argonne-type machine was justified not only because of the neutrino experiments but practically of all the other experiments. I invite all of you to tell me what experiment done at the PS would not have been possible, at least for us, European beings, under easier conditions with an Argonne-type machine, that is a ~ 10 GeV machine with 10^{13} circulating protons. Probably, also for the diffraction scattering experiments (one of the few outstanding experiments done at the PS), intensity would have been a good compensation for energy and later on for the lack of some computing machines. I may add that probably having available this apparently more modest tool, the mythology of the high-energy would have been attenuated and the random search for unknown particles with inadequate instruments would have been avoided. Actually plenty of new particles were existing but it was not the energy which made their discovery possible, but the appropriate consideration of previous achievements and better preparation in beams and instrumentation.

To conclude I would like to tell a Florentine story. It is not very good but it allows many interpretations and probably it will represent my best contribution to this conference.

The Pitti and Strozzi palaces in Florence are among the most famous of the Renaissance. About the middle of the XVth century, Pitti and Strozzi were both extremely wealthy families; the Strozzi of very obscure origins, the Pitti of old high nobility. The period of fighting between Guelfs and Gibellinis was now over since about a century, and the games were now on a financial and political level. The main show of this type of power was simply to build splendid houses - which of course were also very costly.

The Strozzi were first, and the Strozzi palace was already in existence when the old Pitti asked Brunelleschi to prepare the plans for his new house. It took several years before it was completed, but it was really huge and magnificent with its monumental courtyard and an endless park all around. But this exploit ruined the Pitti family, which was completely broken when the official opening of the house was celebrated by a party attended by 300 persons. The head of the Strozzi family was amongst the guests. He was a nice, well-behaved old gentleman, and with great courtesy insisted on offering his congratulations to Pitti, the father. Pitti received the Strozzi's compliments with condescension, and then showing to him the courtyard of his palace (a wide garden enclosed by Roman arches and columns) said : "Do you know, your house will just fit into this empty space!".

A few weeks later the Strozzi gave a big reception and Pitti was there. Strangely enough, in the banqueting hall, together with a few armchairs, were several treasure-chests. Pitti, not surprised at the bad taste of Strozzi, said to him : "Very frankly I find it rather uncomfortable to sit on these chests". "You are very right, and I apologise for it", answered the old Strozzi, "but unfortunately this house is too small and all my strong-rooms are already filled with gold".

DISCUSSION

BLASER : May I ask you what your opinion is on storage rings and 300 GeV machines?

BERNARDINI : This machine should be built, but it cannot be the only machine on which the Europeans are supposed to work; otherwise, instead of pushing to develop this field it would depress it very quickly. Instead of spreading high-energy particle physics all over Europe, we have a monastery, perhaps in Geneva, of a few elite people who are able to think about God. I do not think this is what we have in mind. If only one machine is to be built then I would strongly recommend a machine of high flexibility that could be used as a 3 GeV machine as well as a 300 GeV machine, with some exaggeration. In other words, the idea that these machines should be so big is a little bit a "folie de grandeur" that I will leave it to a Napoleon, but not to the physicists.