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B.Pontecorvo

SUMMARY OF THE FIRST PART OF THE NEUTRINO-72 EUROPHYSICS CONFERENCE, BALATONFÜRED

ЛАБОРАТОРИЯ ЯДЕРНЫХ ПРОБЛЕМ

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Introduction

There have been already so many talks and discussions at our conference that it is really difficult for me to make a summary. Notice that most of the talks had a review character.This makes my task even more difficult.And then it occured to me that when I will be back in Dubna my friends will ask me: how about the conference in Hungary? I will have to answer them and give my impressions. So I decided that right now I would tell you what I will tell the people in Dubna in a few days. Of course, one has to use some subjective criteria for selecting among the papers and the comments in the discussion. Now I wish to emphasize that the omission in my summary of communications presented here does not mean in any way that they do not deserve being mentioned. The fault for the omission must be traced to the criteria I used:

First, there will be an unmistakable experimental bias in my summary.

Secondly, I am going to spend little time on problems about which there are no new experimental data, even if they are very important. This seems reasonable since such problems, as a rule, have been treated recently in review papers. Thus I neither will talk about "second class" currents, which were discussed in the interesting talk of Dr. Pietschmann, nor about muon physics, which was treated by Telegdi in a beautiful lecture rich of "first class" jokes.

Thirdly I am going to talk mainly about ambitious and difficult investigations in which somebody tries very hard to find and measure something, but does not see anything. You certainly have noticed that at our conference most of the experiments has such character. Search experiments usually give results which very improperly are called "negative". At the conference there were presented brilliant and brave experiments, yielding very low upper limits, the significance of the results being very great. However, the fact remains that results are presented not in terms of a measured quantity being equal to a certain value but through the $<$ sign. I must say that this is becoming more and more frequent, and that is one of the reasons why life is much harder for people doing experiments than for theoreticians.

Thus our conference, at least the first half of it, is an "inequality" meeting, where new effects were searched for at an incredible sensitivity level. On the basis of this "inequality" principle I am led to make the following classification of the material in the summary:

1) The $K_L \to 2\mu$ puzzle (plenty of inequalities)

2) Solar neutrinos (upper limit)

3) Lepton charge conservation (upper limits)

4) "Stable" heavy leptons ("negative" results)

5) Antineutrino electron scattering, reactor + electronics (upper limit)

6) Neutral currents, accelerators + bubble chambers (upper limits).

The $K_L \to \mu^+ + \mu^-$ puzzle

The Oakes report and its discussion were terminated only a few minutes ago, so that it would be tiresome for you if I were now continuing to talk in detail about the $K_L^0 \to 2\mu$ puzzle.

When correcting the magnetophone tape, however, I decided that I had to write something on the puzzle, mainly in order to mention such points in the discussion which were not known, at least to me, before our conference.

As is well known, the puzzle consists in the following (see also the extensive review paper of Dolgov, Okun and Zakarov, ITEP, No. 924): according to experiments

$$\left(\frac{\Gamma K_L \to 2\gamma}{\Gamma K_L \to all}\right)_{exp} = (5 \pm 1) . 10^{-4}, \qquad (1)$$

$$\left(\frac{\Gamma K_L \to 2\mu}{\Gamma K_L \to all}\right)_{exp} \leq 1.8 . 10^{-9} \text{ (90 \% confidence level).}$$

Theoretically one can obtain the following lower limit:

$$\left(\frac{\Gamma K_L \to 2\mu}{\Gamma K_L \to 2\gamma}\right)_{theor} \geq 1.2 \cdot 10^{-5}. \quad (3)$$

From (3) and (1) one gets

$$\left(\frac{\Gamma K_L \to 2\mu}{\Gamma K_L \to all}\right)_{exp.theor.} \geq (6.0 \pm 1.2) \cdot 10^{-9}$$

in contradiction with (2).

As you know, the puzzle is a serious thing. The reason is that the theoretical bound (3) is quite reliable: one calculates only the imaginary part of the amplitude, which arises from transitions on the mass shell $K \to$ intermediate real states $\to 2\mu$; the real part of the amplitude can only increase the probability transition and this is why you get a lower limit of the rate. The two photon intermediate state, which has been observed experimentally, should dominate the imaginary part of the amplitude,because other states have much less space phase.

In the discussion Marschak (as well as Okun and collaborators in the quoted paper) reminded us that frequently in the past puzzles arose from wrong experiments. However, as Telegdi has emphasized, the best experts think now that the experimental results (1) and (2) are correct. If the experiments are right, the puzzle must be solved. Many theoretical proposals have been made in which some kind of cancellation of the 2γ imaginary part of the amplitude is invented "ad hoc ". Most of these proposals (violation of CPT invariance, violation of unitarity of the S-matrix, introduction of new particles with "necessary" properties) are neither attractive nor plausible, in the opinion of many physicists.

As you know, Christ and Lee, instead, made a proposal which is quite attractive. They note that because of the usual (small) CP violation, $K_L = K_2 + \epsilon K_1$ (with the usual notations). To suppress the $K_L \to 2\mu$ decay these authors assume that the necessary cancellation is due to the $K_1 \to 2\mu$ decay. Since ϵ is small, the $K_1 \to 2\mu$ amplitude must be much larger than the $K_2 \to 2\mu$ amplitude; in addition in the $K^0 \to 2\mu$ decays there must be a strong CP violation, in order that the final states in $K_1 \to 2\mu$ and $K_2 \to 2\mu$ may interfere to cancel the two photon contribution. The analysis by Oakes of various theoretical bounds and experimental limits has shown that an experiment designed to detect the $K_S \to 2\mu$ decay if its relative probability $\frac{\Gamma K_S \to 2\mu}{\Gamma K_S \to all}$ is larger than 10^{-7} would either confirm or exclude definitely the Christ and Lee schema.

Now we heard at the conference that several experiments are being performed at present to detect the $K_S \to 2\mu$ decay at the necessary sensitivity level. According to an information of Telegdi one of these (at CERN) has already given one good $K_S \to 2\mu$ decay candidate. Similar experiments will be performed at the Argonne Laboratory and within a year there should be a definite answer. It was also very interesting to hear from Telegdi that an experiment designed to detect the $K_L \to 2\mu$ decay if its relative probability is larger than 2.10^{-10} is being prepared by the Croning group. This is an order of magnitude below the previous result (2) of Clark et al.

Now two words about a comment by Marschak. Since he will give his full talk later at this conference, I will not go into the real business now, limiting myself to few remarks. The work of Marschak and collaborators on the "strong cubic intermediate vector boson" model is not new (1969), but it seems that its relevance for the $K_L \to 2\mu$ puzzle was not generally recognized and became clear only at our conference. I would say that the proposed theory is in fact a model of the phenomenological Christ and Lee proposal. In this sense even if the model does not appear to some people on estetical grounds, it is certainly of great interest, since it is not an "ad hoc" proposal, and " required" the Christ and Lee schema even before the $K_L \to 2\mu$ puzzle exploded. Other good things of the model, in my opinion, are: first, the fact that the well known "weak" CP violation is a consequence of the theory, where basically there are strong CP violations, and secondly, the predictions of gross CP violations in the production of intermediate bosons, and in various processes, such as $K^+ \to \pi^+ e^+ e^-$, $K^+ \to \pi^+ \mu^+ \mu^-$ and, of course, $K^0 \to 2\mu$.

Solar Neutrinos

1) The Brookhaven experiment

Now I am going to talk on solar neutrinos and I will spend more time on this problem than on any other problem. Of course I will start with the famous Brookhaven National Laboratory experiment. This is a brave experiment of Davis et al. and I would say it is one of the few experiments which are being performed without competition. The same can be said of the Reines experiment on $\tilde{\nu}_e - e^-$ scattering with reactor antineutrinos, about which I will talk later. Suppose that Davis were feeling like going in vacation for a few years: there is no the slightest risk that somebody else would perform an analogous experiment in the meantime!

I will say a few words to convay to the theoreticians the difficulty and the scale of this experiment. Deep underground there is a mass of about 600 tons of C_2Cl_4. If events of the reaction

$$\nu_e + Cl^{37} \to A^{37} + e^-$$

are originated within such "swimming pool" by solar neutrinos, you get a few A^{37} atoms. This is a radioactive noble gas and it is possible to extract these few atoms from the tremendous amount of C_2Cl_4 with a small amount of argon carrier and by He purging. The argon fraction is then separated again from the large helium one and is introduced inside a small proportional counter, in which one measures the characteristic energy emitted in the K capture decay of A^{37}. If you understand the difficulty in pushing the effective background of the counter down to about one count per month, then you will realize how acrobatic this experiment is.

My impression is that the necessary checks were done carefully and, as a person who has been working quite a lot with proportional counters, partially in order to prepare an experiment similar to the one which Davis is doing, I am very impressed by the improvement in the counter used at Brookhaven.

The improvement is due to the fact that, in order to decrease the effective background, also the pulse time of rise is measured (in addition to the pulse amplitude spectrum). This gives a substantial rejection factor for pulses originated by (background) particles, which inside the counter are less localized than the Auger electrons from K capture in A^{37}.

Here are the results:

the A^{37} production rate is 0.18 ± 0.10 events/day. In part this rate is due to the muon background (muons produce protons which produce A^{37} via the reaction $Cl^{37}(p,n)A^{37}$). This background, partially measured and partially calculated (see the talk of Young) is 0.12 ± 0.04 events/day. The difference is 0.06 ± 0.14 (it is safe to add the errors).

Thus solar neutrinos have not yet been detected. The capture rate of solar neutrinos in the detector is less than one in five days (70% confidence level). This corresponds to a rate $\leq 10^{-36}$ sec^{-1} (Cl^{37} atom)$^{-1}$ = 1 SNU (solar neutrino unity, according to a convenient notation of Bancall).

There are two astrophysical conclusions which can be made with reasonable certainty. As it will be seen below, the Brookhaven detector is due to high energy neutrinos from B^8 (very few but very effective). So the first conclusion, made by Davis is that the Sun emits much less B^8 neutrinos than expected. The other conclusion is that the C-N cycle is of little importance in the Sun, since otherwise the A^{37} counting rate should be much higher.

2) Interpretation

The interpretation of the Brookhaven experiment was given at the conference by Bahcall.

The thermo-nuclear reactions of the hydrogen cycle in the Sun are shown below together with their expected relative percentage:

$$\left.\begin{array}{l} H^1(p,e^+\nu) \\ (99.75\%) \\ \\ H^1(e^- p,\nu) \\ (0.25\%) \end{array}\right\} H^2(p,\gamma)He^3 \left\{ \begin{array}{l} (He^3,2p)He^4 \;(\sim 86\%) \\ \\ (a,\gamma)Be^7 \left\{ \begin{array}{l} (e^-,\nu)Li^7(p,a)He^4 \;(\sim 14\%) \\ (p,\gamma)B^8 \to \nu + e^+ + \underbrace{Be^{8*}}_{He^4 + He^4} \\ \qquad (\simeq 0.02\%) \end{array}\right. \end{array}\right.$$

Fig. 1

In the following table, presented by Bahcall in his interesting talk, are given the expected capture rates due to neutrinos produced by different nuclear reactions in the Sun, and expressed in SNU.

You will see the serious discrepancy already mentioned between the measured rate and the theoretical expectation, which is the sum of all the contributions in the table (a possible small contribution from the C-N cycle is not included).

Neutrino source	Maximum neutrino energy (MeV)	Expected capture rate in the Cl-A detector (SNU)
$H^1(p,e^+\nu)H^2$	0.42	0
$H^1(e^-p,\nu)H^2$	1.44 (monoenergetic)	0.26 ± 0.03
Be^7	0.86 (90%) (both mo- 0.38 (10%) noenergetic)	1.0 ± 0.2
B^8	14.1	4 - 7

Is the discrepancy serious enough to force us to draw revolutionary conclusions about the Sun or about the neutrino properties? My opinion is: no. Let us look at the table. Most of the expected A^{37} rate is due to B^8 neutrinos, which represent a very small fraction of the total number (B^8 neutrinos are very energetic and consequently very effective). The reactions leading to the production of B^8 are quite unimportant from the point of view of the structure of the Sun. The A^{37} rate due to B^8 neutrinos was calculated by using currently accepted solar parameters; well, astrophysicists will have to change these parameters, and the Sun will nevertheless shine as before; seriously speaking, I mean that the Sun with new parameters will not substantially differ from what we think it is now. For the time being there is no astrophysical tragedy: the Brookhaven result is very important, since it will help to change the current solar parameters in the right direction. I think that this is not far from the opinion of Bahcall, although may be I am more conservative than he is.

After the Bahcall lecture there has been a comment by Chudakov. He said that in Moscow Kopysov and Fetisov suggested that maybe the low flux of solar B^8 and Be^7 neutrinos is due to the existence of a resonance in Be^6: the existence of a resonant Be^{6*} state will decrease the concentration of He^3, and consequantly of Be^7, which arises in the $He^3(\alpha,\gamma)Be^7$ reaction, and of course of B^8 (see Fig. 1).

I think that this possibility is rather unlikely, since the resonance must be at the needed energy, but the proposal is very interesting and reasonable. After the comment was delivered Chudakov found out that this suggestion had already been made by Fowler and asked me to apologize for him: he just did not know about the Fowler work. Anyway the suggestion is an instructive example of "what might be true": the Sun structure practically would not change, while the flux of B^7 and B^8 neutrinos would substantially decrease.

Now if we look at the remaining (other than B^8) contributions in the Table, you will see that their sum is not in serious disagreement with the result of Davis ($\leq$ 1 SNU, 70% confidence). Thus I repeat the conclusion: there is no reason to think that the Sun is substantially different from what we believe it to be and, even more emphatically, there are no reasons to believe that the neutrinos have very exotic properties.

3) The Future

Now let us see how the future looks like. Prof. Davis is going to get very interesting results in the near future using his improved counter and probably will either detect solar neutrinos or get to the limit his experiment permits (~ 0.5 SNU at the given depth underground). As Bahcall pointed out, if you get a sensitivity of ~ 0.3 SNU and you still do not see neutrinos you really have got something very exotic. This is so because the expected rate for *pep* neutrinos (~ 0.3 SNU) is known quite well. As a matter of fact the total flux of solar neutrinos is obtained directly from the Sun luminosity and from the basic fact that there are liberated ~25 MeV energy, when 4 protons are transformed into one α particle + $2e^+ + 2\nu_e$. Unfortunately it is not a simple matter to reach the sensitivity 0.3 SNU, especially at a depth of "only" 4300 m H_2O eq., where now is located the Brookhaven detector.

What to do in the future? Prof. Davis told us about a very important new detector of solar neutrinos which he is developing: it is a *Li* compound from which it is possible to extract chemically a volatile compound of *Be*, which can be introduced into a counter. This is the beginning of a promising development, since the reaction $\nu_e\,(Li^7, e^-)Be^7$ is capable of detecting *pep* neutrinos.

Now the more remote future of solar neutrino astronomy, in my opinion, is connected with the development of huge liquid or solid (noble gas?) electronic detectors, capable of giving some information on the energy and the direction of the detected neutrino. But I will not elaborate on that.

4) Exotics

If neutrinos will be missing at the level expected for the $H^1(p,e^+\nu)H^2$ or $H^1(pe^-,\nu)H^2$ reactions one has to invent something more or less extraordinary. Consequently exotics is useful since it makes us ready for the worse. The danger arises if you believe really in extraordinary things even before you are forced into exotic by hard facts. There were several exotic suggestions at our conference.

In this work Lande suggested: the neutrinos may not be here now, but in the past they were, because the Sun maybe is pulsating. He than discussed the relation between the neutrino history of the Sun and the thermal history of the earth ("neutrino archeology").

Bahcall, Cabibbo and Yahil said: if neutrinos are missing, maybe they decay on their way from the Sun to the earth. It is a simple explanation, if explanations are needed. Such a speculation, as it turned out, has been already useful, inasmuch as it stimulated an experiment: if the neutrino with mass $\neq 0$ decays into another particle ν' neutrino-like with zero mass + a photon, one can try to detect the photons near a working reactor. During the discussion Reines told us that he has performed such experiment. For the particular decay $\tilde{\nu}_e \to \nu' + \gamma$ it was found that the $\tilde{\nu}_e$ decay path is larger than 10^5 astronomic units. Again an inequality! One can ask: why do you need such experiment? my opinion is that any correct experimental measurement is always a very respectable thing. The exotics is useful.

Let me say a few words about the problem of neutrino oscillations, about which there was quite a lot of discussions at our conference. Oscillations were proposed and studied in Dubna, Moscow and Leningrad because they give a very sensitive method for investigating the question about possible lepton charge violations and the neutrino mass problem. The relevance of the oscillations to the interpretation of future solar neutrino experiments was immediately recognized, but I wish to emphasize that the oscillations were not invented "a posteriori" to solve the "missing neutrino puzzle".

It is argued that lepton charge nonconservation and a finite value of the neutrino mass may lead to oscillations of the type $\nu_e \rightleftarrows \nu_\mu$, similar to the $K^0 \rightleftarrows \tilde{K}^0$ oscillations in kaon physics. Other types of oscillations ($\nu_e \rightleftarrows \tilde{\nu}_e$, etc.) can be ruled out if in nature exist only 4 neutrino states.

Since the problem of neutrino oscillations was discussed in detail in my report at the 1970 Kiev Conference, I will not elaborate furthermore and only state some results:

1. The presence of oscillations will decrease by a factor 2 the number of neutrino detectable in a solar experiment (since half of neutrinos are sterile). Only under special exotic conditions or in very sofisticated and remote experiments can the "decrease factor" may become >2.

2. The existence or absence of oscillations could be established in various ways, the simplest method being the comparison of the measured and expected capture rates of solar neutrinos from the *pp* or *pep* reactions. As far as the problem of the neutrino mass is con-concerned, the oscillations are observable if the mass difference of the two Majorana neutrinos which enter the theory is larger than 10^{-6} eV. This method is several millions of times more sensitive than the ordinary one of measuring the neutrino mass (sensitive to mass values larger than ~10 eV, in the most favourable case of the tritium decay). The physical reasons why the method is so sensitive are a) the possibility of measuring an amplitude (and not a squared amplitude) and b) the huge distances which characterise the solar system.

Lepton Charge Conservation

1) Double β decay

There was a very interesting review of the subject by Fiorini who presented also a beautiful experiment, done under the Mount-Blanc, searching for the process $Ge^{76} \to e^- + e^- + Se^{76}$. A Ge(Li) crystal (~ 70 cm^3, ~ 400 gr) was used both as a source and as a detector. The pulse amplitude spectrum is measured with high resolution and one looks in the region around 2.045 MeV, which is the expected sum of the energies of the two electrons in the process looked for. No peak appeared at 2.045 MeV, where the background count was only 2 $(keV)^{-1}$ in 1000 hours. In other energy regions you see lots of peaks due to very minute impurities of natural redioactive elements, so you are confident that the experimental arrangement is working properly. The experiment is absolutely convincing and gives as a result another inequality for the double β decay of Ge^{76}:

$T_{1/2} > 4.5 \cdot 10^{21}$ years (68% confedence level).

The lepton charge violating amplitude is at most one percent of the lepton charge conserving amplitude. Similar results were obtained previously by different techniques in the search of neutrinoless double β decay of Ca^{48}, Se^{82}, Te^{130}.

2) Search for the $\mu^+ \to e^+ + \gamma$, $\mu^+ \to e^+ + e^+ + e^-$ processes

Concerning the muon charge violation, I am going to report now on the work of the group of Korenchenko, who at the Dubna synchrocyclotron looked for the mentioned processes. They use a cylindrical spark chamber magnetic (9200 - 4500 Oersted) spectrometer to analyse the muon decay products (see the Table below).

As for the $\mu \to e\gamma$ process, the accuracy of the result is comparable with the one obtained previously by different methods. But it may be of interest to you that Korenchenko is planning now a search for the $\mu \to e\gamma$ process, where a branching ratio 10^{-10} should be measurable after the accelerator reconstruction is completed. As for the $\mu \to 3e$ process- the result

Searched for process	Number of π^+ stopped in target	Number of photographs	Registration efficiency	$\Gamma_{process}/\Gamma_{\mu \to all}$ (90 % confidence)
$\mu^+ \to e^+\gamma$	6.10^9	$2.5.10^5$	1.35%	$<2.9.10^{-8}$
$\mu^+ \to e^+ + e^+ + e^-$	$2.9.10^{10}$	$6.0.10^5$	3.0 %	$<3.2.10^{-9}$

is ~ 40 times better than the best previous one. It is seen that the muon-charge violating amplitude can hardly be greater than 1 % of the normal amplitude. The results are comparable in accuracy with those obtained in the double β-decay investigations, but of course not only the processes but also the lepton charges, which are investigated, are different.

3) Multiplicative lepton charge?

I would like to mention something new I heard at our conference on the question as to whether one of the lepton charges is a multiplicative number. As you know, several proposals for experiments on this point were made long ago and I will not mention them here.

Now the IHEP-ITEP collaboration (Arbuzov et al.) proposed recently for the NAL program to search for the reaction $\tilde{\nu}_\mu + e^- \to \mu^- + \tilde{\nu}_e$, which, if observed in a large bubble chamber, would directly prove the existence of a multiplicative lepton number. At Batavia the $\tilde{\nu}_\mu$ energy is more than sufficient and the proposal, in my opinion, is the best for the solution of the multiplicative lepton charge problem.

Talking of the multiplicative lepton charge, I would like to mention also a comment made by Filippov for the benefit of experimentalists. He will talk later about his model of a "four dimensional symmetry with multiplicative lepton charge" (whatever that means), but has already presented in the discussion the predictions of his model, which are:

1) $\sigma(\tilde{\nu}_\mu + e^- \to \tilde{\nu}_e + \mu^-) = \frac{1}{2}\sigma_{V-A}(\tilde{\nu}_e + e^- \to \tilde{\nu}_e + e^-)$

2) $\sigma(\nu_\mu + e^- \to \nu_e + \mu^-) = \frac{1}{2}\sigma_{V-A}(\nu_\mu + e^- \to \nu_e + \mu^-)$

3) $\sigma(\nu_e + e^- \to \nu_e + e^-) = \sigma(\nu_\mu + e^- \to \nu_\mu + e^-) = \frac{1}{2}\sigma_{V-A}(\nu_e + e^- \to \nu_e + e^-)$.

4) Theory

Prof. Marx gave an interesting review of the subject and I am unable now to go into it. I would like to mention, however, that in his talk he proposed a decrease with time of the strength of the CP violating interaction. Irrespective of the arguments given by Marx, the idea seems to me interesting; the possible change in time of constants was discussed before, but the CP violating constant seems to me an attractive candidate for the following reasons: if nature worked that way, which is of course highly improbable a priori, the big bang approach would easily give the asymmetry between matter and antimatter even if the barion number in the Universe is equal to zero (and without the need of inventing new particles, as it was done in the papers of Zakharov and Kuzmin).

Stable Heavy Leptons?

In the report of Gershtein on work done at Serpukhov by the Landsberg group there was discussed an experiment designed to detect heavy, stable ($\tau > 10^{-9}$ sec), charged leptons. These objects, with charge $\pm$ are supposed to be very similar to muons; they are not interacting, so they can be detected as muons usually are. The cross section for their pair production in collision of protons with nuclei is supposed to be due only to their electric charge, and can be calculated on the basis of the Lederman experiment on production of muon pairs by protons. Such charged heavy leptons were looked for but not seen at Serpukhov. A comparison with the theoretical expectation, normalized to the muon pair production data, permits to draw the following definite conclusion: there are no "stable" heavy leptons with masses in the interval 1-3.5 GeV. The reason why you get a definite statement is that a production rate of heavy leptons equal to the expected one (normalized to muon pair production) could have been measured easily if such object existed.

Neutrino Scattering

Now I will turn to the subject of neutrino scattering, that is neutrino lepton and neutrino nucleon scattering. I must say that the Weinberg's theory has a very progressive influence on the work. We are now seeing a sort of renaissance of the weak interaction physics and this, to a definite degree, is due to his theory. I heard that at the Tashkent's Conference Pais qualified Weinberg's work as "strategy". I like this definition. The old problem of neutral currents is now investigated experimentally on a very wide scale with the help of reactor and accelerator facilities, and most experiments are being interpreted in terms of Weinberg's theory. Certain experiments, for example the search for $\nu_\mu - e^-$ scattering (at least with electronics methods) would probably not be considered without the new theoretical encouragement. Now such experiment is one of the first on the list at pion factories and is being performed now in large bubble chambers.

1) Antineutrino-electron scattering (reactor + electronics)

The experiment, being conducted by Reines and collaborators with the help of a large reactor, occupies a central place in our conference. Reines has been working on the problem for more than ten years. The investigation is very difficult and, as I said before, the experimental arrangement is a monopoly of Reines. Why the experiment is so difficult? An elastic collision between $\tilde{\nu}_e$ and e^- at reactor energy is an event without a very characteristic signature, and is imitated easily by background, for example, by a Compton electron generated by γ's from radioactive impurities, etc. So the fight against background is the main problem and is made by clever and complicated methods, which I cannot describe now. The detector itself is a ~ 8 kg plastic scintillator, which sounds very simple but it is not. In order that you maybe appreciate the difficulties, I remind you that the counting rate for the events of interest, that is for events which could be electron recoils with energy > 3.5 MeV in the reaction $\tilde{\nu}_e + e^- \rightarrow \tilde{\nu}_e + e^-$, is about one a day! This is the rate with the reactor on as well as with the reactor off, that is here again it was not possible to see what one has looking for.

The upper limit of the cross section σ_{exp} for the process $\tilde{\nu}_e + e^- \rightarrow \tilde{\nu}_e + e^-$ with fission $\tilde{\nu}_e$ is found to be

$$\sigma_{exp} < 1.7\ \sigma_{V-A} \qquad \text{(70 \% conf. level)}$$

Here σ_{V-A} is the expected cross section according to the V-A prediction (only charged currents) and the V-A spectrum of electron recoils was assumed when calculating the fraction of electrons with energy > 3.5 MeV. Now in the Weinberg's theory the presence of neutral currents, to an extent depending upon the parameter e^2/g^2, changes the V-A prediction; the results of Reines give already some constraints on the Weinberg's parameter. This was also discussed in the report of Baltay.

We heard from Reines that he has been recently improving the experimental arrangement, doubling the mass of plastic scintillator without any increase of the background. He feels confident that within a year he could "see" the scattering events if $\sigma_{exp} > 1/3\ \sigma_{V-A}$. Let us wish him success.

2) Neutrino Electron and Neutrino Nucleon Scattering. Neutral currents (Accelerator + Bubble Chamber)

We heard yesterday in the talk of Pullia (the data were obtained at CERN in the "heavy" bubble chamber Gargamelle exposed to ν_μ and $\tilde{\nu}_\mu$) and in the talk of Baltay (a critical and instructive review of all the data) about the state of the search for neutral currents in $\nu_\mu - e^-$ and $\nu_\mu - N$ scattering and also in other processes. Work is being done on a very wide front; sufficient to say that only at CERN the number of neutrino events obtained with Gargamelle is at least an order of magnitude greater than all the world pregargamelle statistics; the analysis of the data, however, for the time being is very preliminary and partial. The Weinberg strategy dominates. Baltay emphasized that the energy spectra of the final state electron or hadrons in processes due to neutral currents strongly depend upon the parameter of Weinberg theory e^2/g^2. This means that the detection efficiences in the experiments (and consequently the upper limits obtained) depend on e^2/g^2. The results of such analysis of Baltay are illustrated in self-explanatories figures in his report, and I will only say a few words of summary on the neutral current (I will call symmetrical the neutral currents of the type $e\bar{e}$, $p\bar{p}$, $\nu\bar{\nu}$ ..., etc., and asymmetrical the neutral currents of the type $e\bar{\mu}$, $n\bar{\lambda}$..., etc.).

1. There is strong evidence against asymmetrical neutral lepton currents (for example the Dubna work on the absence of processes like $\mu \to e\gamma$, $\mu \to 3e$, etc.).

2. There is strong evidence against asymmetrical neutral hadron currents (of course this comes out from the absence of certain kaon decays such as $K^0_L \to 2\mu$, $K^+ \to \pi^+ + e^+ + e^-$, etc).

3. There is no experimental evidence in favour of symmetrical neutral lepton currents (e.g. $\nu_\mu + e^- \to \nu_\mu + e^-$) nor in favour of neutral symmetrical hadron-lepton currents (e.g. $\nu_\mu + p \to \nu_\mu + p$, $\nu_\mu + p \to \nu_\mu + n + \pi^+$, etc; such processes can hardly have a cross section larger than 1/10 of the cross section corresponding to charged currents).

The predictions of the Weinberg model (requiring neutral currents, at least symmetrical ones), however, are very close to be tested. The game is only starting, but within less than a year we should have an answer. I should notice, however, that even if the Weinberg model with neutral currents will be excluded, the Weinberg's strategy will stay: I am told that B.Lee has shown that this is so in a model with heavy leptons.

In conclusion I wish to express my warmest gratitude to the Hungarian Academy of Science, to the Hungarian Physical Society and to Prof. Marx for the wonderful hospitality.

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