THE DISCRETE SYMMETRIES P, T AND C

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1. The Concept of Parity P - In 1924, in analyzing the structure of the spectrum of iron, Otto Laporte found that there are two kinds of terms, which he called "primed" and "unprimed". Transitions are always from primed to unprimed terms or vice versa, and never between primed terms or between unprimed terms. This selection rule was then found to apply to the atomic spectra of other elements as well, and was given the name of "Laporte's rule," or the "Laporte-Russell rule." After the development of quantum mechanics, it was interpreted as related to invariance under the operator

\[ i: \quad x' = -x, \quad y' = -y, \quad z' = -z, \quad (1) \]

which was called "Spiegelung" but designated by the symbol \( i \), by Weyl. For the eigenvalue of this operator Weyl used the name "signature." In the 1931 book of Wigner, the eigenvalue was called the "Spiegelungscharakter." I do not know precisely when the name "parity" was adopted. In 1935, Condon and Shortley used the term "parity operator."

Parity symmetry rapidly became a part of the language of atomic, molecular and nuclear physics in the 1930s. Level assignments, selection and intensity rules, and angular distributions were discussed with the concept of parity conservation assumed, explicitly or implicitly. When elementary particle physics began to develop, parity conservation was naturally carried over into the new field.

Before going into the subject of parity nonconservation, it is interesting to recall that while the wide use of group theory in physics is taken for granted today, the introduction of group theory in physics by Weyl and Wigner in the late 1920s was by no means welcome at the time. In the preface to the 1959 English translation of his book Wigner wrote:

When the original German version was first published, in 1931, there was a great reluctance among physicists toward accepting group theoretical arguments and the group theoretical point of view. It pleases the author that this reluctance has virtually vanished in the meantime.
and that, in fact, the younger generation does not understand the causes and the basis for this reluctance. Of the older generation it was probably M. von Laue who first recognized the significance of group theory as the natural tool with which to obtain a first orientation in problems of quantum mechanics. Von Laue's encouragement of both publisher and author contributed significantly to bringing this book into existence. I like to recall his question as to which results derived in the present volume I considered most important. My answer was that the explanation of Laporte's rule (the concept of parity) and the quantum theory of the vector addition model appeared to me most significant. Since that time, I have come to agree with his answer that the recognition that almost all rules of spectroscopy follow from the symmetry of the problem is the most remarkable result.

In the twenty odd years since this paragraph was written, larger and larger Lie groups have found their ways into the literature of physics. One cannot escape wondering whether a good and important development has been abused.

Now we come to the 1950s. In studying the decay of the $\tau$ meson, Dalitz analyzed its spin-parity possibilities through the introduction of the famous Dalitz plot. This was an especially useful method, and by January 1955 he reached the conclusion that "if the spin of the $\tau$ meson is less than 5, it cannot decay into two $\pi$ mesons." In other words, the spin-parity assignment of the $\tau$ and the $\theta$ were very likely different. This conclusion had, however, to be balanced against the experimental results about the masses and lifetime of the $\tau$ and the $\theta$. The atmosphere around that time can be seen from the following paragraph in a report entitled "Present Knowledge about the New Particles" that I gave at the International Conference on Theoretical Physics, held in September 1956 in Seattle:

However it will not do to jump to hasty conclusions. This is because experimentally the $K$ mesons seem all to have the same masses and the same lifetimes. The masses are known to an accuracy of say from 2 to 10 electron masses, or a fraction of a percent, and the lifetimes are known to an accuracy of say 20%. Since particles which have different spin and parity values, and which have strong interactions with the nucleons and pions are not expected to have identical masses and lifetimes, one is forced to keep the question open whether the inference mentioned above that the $\tau^+$ and $\theta^+$ are not the same particle is conclusive. Parenthetically, I might add
that the inference would certainly have been regarded as conclusive, and in fact more well founded than many inferences in physics, had it not been for the anomaly of mass and lifetime degeneracies.

It is interesting to note that the use of the word "anomaly" betrays the feeling around that time that the degeneracies should not have been there.

The dilemma was called the $\theta-\tau$ puzzle, which became clearly defined by early 1956. At the Rochester Conference that year, in April, I gave a rapporteur's talk on the new particles and devoted more than half of my time to this puzzle. Oppenheimer said at the end of the session, "The $\tau$-meson will have either domestic or foreign complications. It will not be simple on both fronts."

The puzzle was later solved by the discovery of parity nonconservation. Why was that not the obvious immediate solution? I think there were three reasons:

1. Geometrical symmetries were generally thought, automatically, to be absolute. The precision of space-time symmetries in atomic, molecular and nuclear physics only served to reinforce this a priori belief.

2. Parity selection rules worked well in nuclear as well as atomic physics. Hundreds of experiments have been successfully analyzed in terms of parity selection rules in nuclear level identification, nuclear reactions and $\beta$-decay. It was thus difficult to entertain parity violation in the face of such extensive experiences of the past.

3. The idea that parity is not conserved only in weak interactions was not yet born.

In late April and early May of 1956, T. D. Lee and I worked on the $\theta-\tau$ puzzle. In particular we worried about the definition of the dihedral angle in the experiment

$$\pi p \rightarrow \Lambda^0 \theta^0$$

$$\Lambda^0 \rightarrow \pi^- p$$

which had been reported at the Rochester Conference by the groups of R. P. Shutt, J. Steinberger and W. D. Walker. One day Lee and I hit upon the idea that perhaps parity is not conserved only in the weak interactions. That would produce an up-down asymmetry in (2). This idea led to a few weeks of intensive work, especially in $\beta$-decay. In June we submitted a paper to the Physical Review entitled "Is Parity Conserved in Weak Interactions?". It was published in October, but the title became "Question of Parity Conservation in Weak Interactions," because the editor ruled that the title must not contain a question mark.
We suggested several tests to find whether parity is conserved in the weak interactions. Two groups started on such experiments in 1956. One of them was formed by C. S. Wu of Columbia with E. Ambler, R. W. Hayward, D. D. Hoppes and R. P. Hudson of the Bureau of Standards. The other one consisted of V. L. Telegdi and J. I. Friedman of Chicago. When the Columbia-Bureau of Standards experiment showed, in early January 1957, definite parity nonconservation in $\beta$-decay, R. L. Garwin, L. M. Lederman and M. Weinrich rushed to completion in 48 hours another experiment on parity nonconservation. The results of these three experiments convinced all physicists that parity is not conserved in weak interactions.

2. The Concept of Time Reversal $T$ - Time reversal invariance in classical physics was a subject which has been well studied already in the 19th century. Modern understanding of this invariance started with Kramers' theorem that in any electric field, for an odd number of electrons, the energy eigenstates are at least doubly degenerate. To prove this theorem, Kramers considered an operator that involved the complex conjugate of the wave function of the electron system. Two years later, Wigner showed that this operator was the correct operator for time reversal in quantum mechanics.

Wigner's time reversal operator was not immediately appreciated by physicists. As late as 1941, when Pauli wrote his review article on field theory, this operator was not mentioned and Pauli seemed to favor another one not involving the complex conjugation operation (which was not correct.) Indeed the complex conjugation operation made the time reversal operator difficult to understand. It also made it difficult to use, so that throughout the 1930s and 1940s there were few papers on the subject.

Today we know that an important application of time reversal invariance is the determination of the relative phases of the matrix elements for transitions. The first use of this idea was by S. Lloyd who discussed the relative phases of matrix elements for electric $2^L$-pole and magnetic $2^{L-1}$-pole radiations.

Schwinger introduced another formulation of time reversal invariance. But his formulation is in fact equivalent to that of Wigner.

3. The Concept of Charge Conjugation $C$ - The concept of charge conjugation has an origin which is entirely different from that of parity and of time reversal. In fact, it has no counterpart in classical mechanics at all.

When Dirac wrote his paper on the Dirac equation he mentioned in the introduction that the negative energy states are problems: "The resulting theory is therefore still only an approximation,...". Two years later he came back to this problem in a paper called "A Theory of Electrons and Protons" in which he proposed that "all the states of negative energy are occupied except perhaps for a few...". These unoccupied negative energy states he called "holes" and he assumed that "the holes in the distribution of negative-energy electrons are the protons." He then raised the question "Can the present theory account for the
great dissymmetry between electrons and protons, which manifests itself through their different masses and the power of protons to combine to form heavier atomic nuclei?". Soon after this paper was published, Tamm, Dirac, Oppenheimer and Weyl reached the conclusion that the asymmetry hoped for was not there. Furthermore, if the hole were the proton the life time of a hydrogen atom would be $\sim 10^{-10}$ sec. which is clearly wrong. Oppenheimer proposed therefore that the proton and electron should be treated separately. The result was the view that

in the world as we know it, all, and not merely nearly all, of the negative-energy states for electrons are occupied.

A hole, if there were one, would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an anti-electron.

Thus was born the concept of the charge conjugate particle. I have likened the step taken by Dirac to initiate the "hole" idea to "the first introduction of the negative numbers." It was to lead to today's more sophisticated view of the nature of the "vacuum", which is a revolution in our concept of space-time. I had always admired Dirac's courage in proposing such a crazy idea as the sea of negative energy particles. When I talked to Dirac one day about this, however, he said the idea was not that crazy at the time, (in his opinion), because people were already familiar with holes in atomic shell structure. I think it may have appeared to him to be not so crazy, because he believed that the most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalise the mathematical formalism that forms the existing basis of theoretical physics, and after each success in this direction, to try to interpret the new mathematical features in terms of physical entities... 

To his fellow physicists at that time, his idea was quite unpopular. [See Moyer, D.F. Am. J. Phys. 49 (1981) 1055.]

The next step in this development was taken by Furry, who proved a theorem, later called the Furry theorem, which in the language of Feynman diagrams, is the statement that for odd order electron-positron loops in quantum electrodynamics, the two diagrams with opposite directions for the electron arrow cancel each other. Furry emphasized in the abstract of his paper that the cancellation was "brought about by the distribution's symmetry between the electrons and positrons."

At about the same time, Majorana, and later Kramers started the formal treatment of conjugation symmetry.

These three papers published in 1937 were very interesting since in addition to charge conjugation they touched on various additional concepts that became interesting or important later on: Majorana's paper introduced the Majorana theory
of the neutrino. Kramer's paper concluded with the following sentences

As a result we expect that a correction must be applied to the energy values of the stationary states of the hydrogen atom, as given by the Dirac theory of 1928. In a later paper we will discuss more closely the possibility of actually computing this correction.

(Italics original).

It seemed, however, that although Kramers started on the idea of the renormalization program already in 1937, he did not bring it to a successful conclusion.

In the post World War II era, Furry's theorem was generalized to various types of meson-nucleon couplings and Pais and Jost showed that these are related to charge conjugation invariance and to charge symmetry. Further applications of charge conjugation invariance were made by Michel and by Lee and Yang.

The experiments of 1956-1957 showing that parity conservation is not observed in the weak interactions also showed that charge conjugation invariance is not observed in weak interactions.

4. CPT Theorem - In Schwinger's papers about field theory, there is implicit realization of what was later called the CPT theorem which states that in any Lorentz-invariant local field theory, the operator CPT leaves the theory invariant, even though the C,P and T operators individually may not do so. This theorem was partially proved by Luders and more completely by Pauli, and became of great practical importance in the mid 1950s.

In 1957 Jost pointed out the relationship between the CPT theorem and microcausality.

From the conceptual viewpoint, it is interesting to observe that quantum mechanics necessitated the use of complex numbers as an essential element in our description of the physical universe, and quantum field theory necessitated the use of analytical functions. Out of these developments came the CPT theorem. We of course do not know at this moment whether and what more sophisticated developments lie ahead in our understanding of the CPT theorem.

5. Violation of CP Invariance - After the discovery of P and C nonconservation, in order to save as much symmetry as possible, there were proposals to have CP strictly conserved. For a number of years, this proposal was in agreement with all experimental results. But in 1964 Christenson, Cronin, Fitch and Turlay found that CP conservation also was not strictly valid. Because of the CPT theorem, it is believed that time reversal invariance is also not strictly valid.

6. Comments - The investigation of the violation of the discrete symmetries have continued in many directions up to this day. Much has been learned about the phenomena of P, C and CP nonconservation. Theoretically, the two most important conceptual developments resulting from these investigations are first, the reaffirmation of an earlier 2 component theory of the neutrino, and second the very remarkable analysis of Kobayashi and Maskawa in 1973 that to accommodate
CP nonconservation, four quarks are not sufficient. Technically, the violation of P conservation made possible the production of polarized beams of particles which facilitated many experimental studies.

But the fundamental reason for the violation of the discrete symmetries remains unknown today. In fact, there does not even seem to be any suggestion of a possible rationale for these violations. Such a rationale, I believe, must exist since at the fundamental level, we have learned that the theoretical structure of the physical world is never without reason.

FOOTNOTES

1. LAPORTE, O., Zeit. f. Phys. 23 (1924) 135.
7. YANG, C. N., Rev. Mod. Phys. 29 (1957) 231.
10. See YANG, Chen Ning, Selected Papers 1945-1980 with Commentary (Freeman, in press).
14. See the forthcoming biography of H. A. Kramers by M. Dresden to be published by Springer-Verlag.
15. PAULI, W., Rev. Mod. Phys. 13 (1941) 203.
17. SCHWINGER, J., Phys. Rev. 82 (1951) 914.
29. LEE, T. D., OEHME, R. and YANG, C. N., Phys. Rev. 106 (1957) 340. This paper was written as a result of a letter from Oehme dated August 7, 1956. See Commentary on §57a in ref. 10. See also Ioffe, B. L., Okun, L. B., Rudik, A. P. JETP 32 (1957) 396.
30. SCHWINGER, J., Phys. Rev. 91 (1953) 713; 94 (1954) 1362. See especially equations (54) and (209) and discussions of these equations in the latter paper.
32. PAULI, W., in Niels Bohr and the Development of Physics, (Pergamon, 1955).
34. It is interesting that H. Weyl wrote in November 1930, in the preface to the second German edition of his The Theory of Groups and Quantum Mechanics

The fundamental problem of the proton and the electron has been discussed in its relation to the symmetry properties of the quantum laws with respect to the interchange of right and left, past and future, and positive and negative electricity. At present no solution of the problem seems in sight; I fear that the clouds hanging over this part of the subject will roll together to form a new crisis in quantum physics.

[See H. P. Robertson's translation, (Dover, 1950).] He was thinking of P, T and C, but I am not sure what crisis he was referring to.
DISCUSSION

L. MICHEL.- Thank you very much, Professor Yang.

Well, I'm sure, this talk of Professor Yang will start a lovely discussion among the members of the round table and also among the audience. So I will use the chairman privilege, may be to speak to historians because, things which are so clear for us and of course which were so clear for Wigner in the beginning of the 30's were not at all clear for the physicists. For instance, I could give a list of people who violated parity without knowing it. Among the physicists we spoke about this morning and I have great respect for, was for example Toushek, in his paper on double $\beta$-decay. I quoted yesterday Enatsu who had the most economical vector meson, intermediate boson before 1950; I could quote several other people. I even could quote Pauli whom made a mistake about some argument in parity and this happen in a letter. He answered me saying "Yes but Michel you made a mistake on charge conjugation". That was true, of course, and the only excuse I could make is that Professor Kemmer did it before me, in his famous paper on charge independance. Well I would not like to speak on time-reversal; I have also to accuse myself of a sin against time-reversal in 1951. You see, it was before I went to Princeton and knew Wigner but if I had to make the list of papers who violated time-reversal, I would need several hours. So I think that we should ask for questions to Professor Wigner about parity and time reversal. You understood time reversal long before any physicist. You wrote on it in 1932 and I remember the controversies in 1951 for instance. It just happens that now that $\mathcal{P}$ and $\mathcal{T}$ invariances are two approximate laws of nature. Would you like to give us your rememberance or comments?

E. WIGNER.- I must admit that I was really greatly surprised by the violation of reflexion symmetry. I was never surprised by the violation of charge symmetry. I knew that most of the electrons on this earth are negatively charged and most of the protons are positively charged but that there is an asymmetry with respect to reflexion was a sort of a shock to me. Perhaps, I mention something which bothers me very much: Doctor Cox sent me an article on $\beta$-decay and his article clearly showed the lack of validity of space symmetry.

L. MICHEL.- When was that?

E. WIGNER.- Much before: 1932 or 33 and I wrote back to him that your experimental results seem to contradict reflexion symmetry and I would look at it more carrefully and he withdrew the article and it embarasses me ever since because I think the article was correct. But these things happen.

C.N. YANG.- May I say something about this?
The Cox experiment was analysed in great details in articles by Lee Grodzins. One was in "Adventures in Experimental Physics" edited by Mađi in which he reached the following conclusion: that the magnitude of the effect that Cox found (it's about helicity of $\beta$-decay) was roughly correct but his sign was wrong. Grodzins then added the statement that he believed that the experiment was right, but in the data analysis, Cox gave it the wrong sign.

E. AMALDI.- As pointed out by Yang already in 1928 and 1930 a few experiments had provided evidence for a longitudinal polarization of the electrons. These papers have been amply discussed in recent years and the conclusion has been reached that the relation of these results to the conservation of parity was not recognized or understood by any contemporary physicists, including the authorsthemsevles. The references to these papers are given at the end of my report on "Beta decay opens the way to weak interactions" /99/ /100/.

L. MICHEL.- I can give another anecdot about "could be" parity violation experiments. It just happened in 1955. Bouchiat and I, we computed correlations in the Möller scattering electron-electron or in the Bhattachara scattering, and then Halban, who is no longer with us, came and said to me: "Oh I would like to make this experiment, it's very interesting; is it very important?". Well I told him, you know QED is verified up to the 6th decimal (about that at that time) so if you want to make this experiment
within 10% or even 1% accuracy that would be a great thing, but may be it will not teach us many things. Anyway they started the experiment with $^{32}P$ sources but you know $^{32}P$ has only a two week lifetime. And after having bought 3 sources, they run out of patience, may be of fund and of time and they haven't published. When the bomb of parity violation occured, they came to me and said "Well, what can we do?" I said: "Well just do the same experiment again". They did a similar a similar experiment.

What about time-reversal Professor Wigner? You spoke of parity but you were also accused to be the first one to establish time-reversal for us in quantum mechanics after this beautiful paper of Kramers, that you quoted and that we read. Have you anything to comment about time reversal? I would say that time reversal is just one of these examples of the quotation I read in Review of Modern Physics where Wigner understood physics but the physicists didn't understand Wigner. In that case for about 20 years.

E. WIGNER. - Frankly, I was fully convinced that both time reversal invariance and reflection symmetry are valid. It was a great shock to me when the lack of validity of these was proved. Of course I was fully aware of the fact that the entropy increases, but I gave a very different explanation for that, based on initial conditions, and I think that that explanation is valid and it isn't the absence of time reversal invariance that causes the entropy increase. But I must say I have a great respect for those who were bold enough to anticipate that these invariances are not valid. I don't know whether it is conceivable that the lack of validity of these invariances also depends on the initial conditions. Surely the fact that all electrons in this table are negatively charged is a result of the initial conditions. But it is not at all clear that the aforementioned lack of symmetry can also be reduced to the lack of symmetry of our world. It is possible to think that the whole existence of the weak interaction is due to some initial condition of the world, but I can't believe it and therefore I am as puzzled as before by the lack of validity of these invariances. If we believe in the simplicity and beauty of all laws of nature, these invariances should be valid. Would you contradict me?

C.N. YANG.- I think, everyone's initial orientation is to prefer more symmetry rather than less. The point that this table is full of electrons and not full of positrons, there are new theories, which remain to be proved, that in some sense understand this. I think the question of symmetry and the manifestation of nature which is not so very symmetrical, the marriage of the two terms of broken symmetries, is a most interesting idea. But the details of this idea still remain to be clarified. I think we will have a very interesting time in the future.

L. MICHEL. - You spoke about the CPT symmetry and this one is not yet broken. Everybody believes in CPT symmetry. Nevertheless you have a problem: why is there only matter around us? If you don't want to break CPT symmetry you have to think of several steps, if you want to start from a charge-symmetric Big Bang. And many people wanted that. It's Sakharov who first showed how is preserve CPT invariance, start from a C symmetric Big Bang and have now more matter than antimatter. He did it in 1967, so his paper is outside the scope of this conference but it's still history. It just happened that the term that Sakharov introduced gave to the proton a lifetime of $10^{50}$ years. Thanks to gauge theory, this value has been reduced, and could be experimentally tested now. In most great unification schemes proton should decay. So now we question baryonic charge conservation but we still do not know the answer. Recently Fleche and Souriau, from the corpus of quasar observations, have proposed convincingly a model of our universe which is symmetric between matter and antimatter, but that antimatter is very far: it has to be further than 10 billion years.

E. WIGNER. - I would very much like to make one more remark. We very well know that the initial conditions do not show any symmetry, they are in some sense as irregular as possible. Dr. Anderson is there and there is no Dr. Anderson on that side. The question therefore arises: will the separation of initial conditions from the laws of nature (in my opinion Newton's greatest accomplishment) prove to be absolutely valid or is the interaction of the initial conditions with the laws of nature responsible for the
absence of some symmetry in the latter. We know that, according to Ernst Mach, all known laws of physics are approximate and, if so, this is true also of Newton's separation of initial conditions and laws of nature. You know that Dirac made the suggestion that the ratio of electromagnetic and gravitational forces depends on the density of the universe, and since that decreases, the law of nature postulating the time invariance of this ratio is not valid. Hence it is really possible that some lacks of invariance are due to the lack of symmetry of the world around us.

I thought it would be good to call attention to the possibility that the lack of reflection invariances of weak interactions is due to the asymmetry of the state of our universe. I thought I should call attention to this possibility even though I cannot really believe it.

Y. YAMAGUSHI.- Let me know the answer posed by Professor Yang. Who was the godfather of "parity"?

E. WIGNER.- I have no idea. But it is not a very great invention, this word.

E.C.G. SUDARSHAN.- I would like to add a comment to Professor Yang's presentation: there is one context in which maximal parity violation increased the harmony in physics. As long as the free particle is considered as the basic unit the massive spin $\frac{1}{2}$ particles and massless spin $\frac{1}{2}$ particles have quite different realization of the Poincaré group. The massive ones have irreducible representations into two spin states, but massless ones have only one. The work in 1956 put the maximal parity violation in correspondence with the two component neutrinos. However Marshak and I found by an analysis of weak interaction experimental data that the chiral components alone were involved even for massive particle fields and we presented it in 1957 at the Padua-Venice Conference. The chiral decoupled in the anticommutation rules for the spin $\frac{1}{2}$ fields. Thus the proper point of view is to include the particle masses in the dynamical (interaction This emphasis that we made about the chiral components proved correct for by nonleptonic decays; and is absolutely essential for the standard model with $SU(3)\times SU(2)\times U(1)$ and in grand unified theories. So I wish to emphasize the importance of seeing the chiral components and chirality in the development of weak interactions and particle physics in general.

V. Telegdi.- 1) Regarding the Cox experiment, the outcome of which I do not consider trustworthy, it is interesting to note the year: 1928. The spin was brand new, and Mott's paper was not yet in existence. Cox had the idea to perform an analog of Malus' celebrated experiment in optics, which contributed so much to establish the "spin" of light. In fact, Malus introduced the word "polarization" into optics.

2) When the nonconservation of parity was established, we proposed to study the decay of polarized neutrons. These were available at Argonne, and the great expert on them was Dr. R. Ringo. When we discussed the matter with him, he said that after Robson's experiments with (unpolarized) neutrons he had proposed what we wanted, but that the most senior theorist at Argonne had talked him out of it, saying that in virtue of parity conservation no new observable effects could arise!!

Y. NE'EMAN.- 1) With respect to initial conditions (Professor Wigner's remark) - one wonders why those of the Universe are so symmetrical.

2) About Symmetry, and the dislike of Group Theory mentioned in Professor Yang's transparency from Herman Weyl's book ("the group pest"). Symmetry is in the nature of Science, since it corresponds to generalization, removal of "particular cases". For example "all directions should be similar". So Group Theory should have been "in" from the beginning. However, there is always a dislike of new mathematics in each generation, and so Group Theory tended to be rejected in the beginning.
L. MICHEL. - Well, I would like to make a comment to Professor Wigner. I agree that it was a great thing of Newton to distinguish the initial conditions from the laws of nature. But the problems of physics change and the origin of the solar system is another problem of physics that Laplace began to worry about. So what you call initial conditions becomes later a physical problem, and I would say now that the Big Bang, you can consider it as an initial condition, but for most of us its history is a problem of physics.

E. WIGNER. - But no theory of the Big Bang explains the number of people who are in the first row here. The Big Bang is too complicated and certainly it didn't have any symmetry. And as a result if we don't believe in the separability of the initial conditions and laws of nature, no real symmetry will prevail. And that is entirely possible.

L. MICHEL. - I would like to ask Professor Yang: I remember vividly when I was inSeattle hearing Professor Yang gave this lecture on parity violation and Professor Wigner was in the room and asked him questions. As far as I remember you spoke namely on parity and I had already read your preprint. And I remember very well the details of the preprint. But Professor Wigner asked you a question: "How did you choose to violate parity?" And you didn't understand the question and he asked the question again in his a little special way and you said: "You see, I had a problem to solve and I wanted to find an issue. And you are in a room with different doors and you try the different ones and finally..." Professor Wigner told you "Now I know seven ways to violate parity, which one have you chosen?" And you didn't answer. It showed that your thoughts were not really complete at that time.

C.N. YANG. - I remember the Seattle conference very well. I discussed parity non-conservation, and I also discussed the possibility of parity doublets. I also remember that Professor Wigner asked me general questions. I don't remember the story that Wigner had said at that time that there were many ways to violate parity. But I did remember the following: I did say that the situation is very puzzling and I would liken our situation to that of a man in a dark room. We know that there is a way out of this dark room, but we do not know in which direction to look so we have to explore all possibilities. And I will be frank with the present audience: at that time I was not betting on parity non-conservation; Lee was not betting on parity non-conservation. I don't think anybody was really betting on parity non-conservation. I don't know what Teledgi was thinking, but Miss Wu was thinking that even if the result did not give parity non-conservation, it was a good experiment. It should be done because $\beta$-decay did not previously yield any information about right-left symmetry.

And I was told by a very distinguished Russian physicist that Landau did not believe in parity non conservation. In fact in an October meeting in 1956 in Russia he was very strong in saying that this was absolute non-sense. But before the experiment was done, apparently Landau changed his mind and he felt that there might be parity non-conservation. Why is it that most people did not want it? I have thought about this and I think there is only one conclusion: it is that we all like more symmetry than less.

J. TIFONNO. - I would like to put a question to Professor Yang about Fermi's opinion on space reflection because he wrote an expression which people thought was wrong with $\gamma_+^\mu$ in one term (nucleon current) and $\gamma_+^5\gamma_+^\mu$ in the order ($e-\nu$ current). We know now, since a long time, that this is a scalar convenient choice of the $\nu$-phase in reflection. But at that time, people thought that it was a pseudoscalar. I heard once that Fermi answered to this criticism saying that he did not believe that the law of space reflection invariance should apply to all physics. I want to know if this is correct or not.
C. N. YANG.- It is not correct. At least I didn't have that impression. I had much contact as a graduate student and later as a young instructor with Fermi at Chicago and I knew just by discussion with him that he was especially interested in parity conservation I don't know particularly why.

In 1950 after you and I wrote our paper about different possible phase factors of spin 1/2 particles under reflection, there was a conference in Chicago. I think it was in 1951, and Fermi was intensely interested in our paper. So he arranged a special session to discuss it. And he specifically wanted to discuss the question: what are the possible experimental manifestations of what we proposed? That paper that you and I wrote in 1950 turned out to be very useful for the parity work in 1956, because C and C' of couplings were directly taken from the 1950 paper. It came in automatically and immediately.

J. TIOMNO. - In this case I just like to make an observation about the fact that really it was a bold step to give this one of Lee and Yang because at the time everybody would swear for parity conservation. And I just remember that in my Princeton PhD thesis on theories of neutrino and double β-decay, under Professor Wigner, which examined possible projection operators of Dirac fields, there is a footnote stating that I did not include the projection operators with \( \psi(\gamma_5)\psi \), because they were obviously wrong, as they would violate space reflection invariance. I am sure that the fact that Wigner did not react as everybody to this was that, he did not find even interesting which were the unsatisfactory observable effects in such a theory of β-decay.

E. AMALDI. - In the early 30's in Rome, the person that was really very much interested in Group Theory was Majorana, who considered Weyl book the best and deeper book on quantum mechanics. Once he mentioned to have started to write a book on Group Theory, but after he disappeared nobody found any manuscript that could be considered as a draft or a part of such a book.

L. MICHEL. - I would like to ask a last question to Professor Wigner. We are speaking of concepts, but there is a concept we shall not touch which is super-selection rules. In the alphabetic order Wick, Wightman, Wigner wrote a paper on super-selection [Phys. Rev. 88 (1952) 10]. They also wrote in footnote 9 of this paper that they were ready to believe that discrete symmetries couldn't be exact. This changes from what you said. And you gave the example in this footnote: P and C could be violated but \( \text{PC} \) conserved [exact quotation added after the conference: "that C is an exact symmetry is moreover still far from proved. The disturbing possibility remains that C and P are both only approximate and CP is the only exact symmetry law..."]

E. WIGNER. - What should I answer?

L. MICHEL. - Have you comment to make on this footnote of your paper with Wick and Wightman?

E. WIGNER. - You know, I don't remember that footnote.

C. N. YANG. - you said that C and P could be violated.

L. MICHEL. - You choose this example.

E. WIGNER. - Yes, and they were violated.

L. MICHEL. - So Professor Wigner, with Wick and Wightman, you said it in 1952 in a footnote.