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50 YEARS OF Y-T RELATIONS

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0. Foreword

I understand that the purpose of this meeting is not only to commemorate the 40 years of Ward-Takahashi (W-T) relations, but also, of course,to honor Yasushi Takahashi (Y.T.) who has made an important contribution to the understanding of those relations. Thus my paper will be concerned with the latter part of the purpose. That is, I am going to talk about Y.T. himself by recollecting almost "50 years" of my association with him, or what I call my "Y-T relations".

1. Prehistory

Having said so, however, I feel obliged to say at least a few words about W-T relations, just as every other speaker at this meeting. But, to tell the truth, in none of my research papers have I ever written the term "W-T relations". That is, I myself have never worked on the relations, except in the prehistoric times when the relation $Z_1 = Z_2$ was still called "Ward identity". In the beginning of my research career (1949-1951) I was interested in the renormalization theory of general field systems, hence naturally in such identities. Let me therefore start by telling you a few prehistoric episodes which might not be known to those living in the modern times.

In his famous paper of 1949 [1], Dyson gave a general formulation of the renormalization method, and introduced for the first time the concept of multiplicative renormalization. Here, for example, the renormalized charge e is related to the bare charge e_0 in terms of three renormalization constants Z_1, Z_2 and Z_3 as $e = Z_1{}^{-1} Z_2 Z_3{}^{1/2} e_0$. Having confirmed $Z_1 = Z_2$ in the e^2-approximation, he conjectured that this might be true in general. In the following year this was proved by Ward [2]. Strangely enough, when viewed from our present-day standpoint, neither of the two authors related the validity of the identity to gauge invariance of QED. To the best of my knowledge it was Rohrlich who first noticed its possible connection with gauge invariance [3]: the remark was made in a footnote to his paper on the scalar QED. Here 3- and 4-vertices appear, and their renormalization constants ought obviously to be related to each other by gauge invariance. Perhaps this may be the reason why the idea of gauge invariance occured to him in this problem of the spinor QED.

The use of $Z_1 = Z_2$ leads to $e = {Z_3}^{1/2} e_0$. As a consequence e becomes a universal constant in the sense that it does not depend, first, on species of charged particles, and second, on dynamical situations in which the action of e takes place. That is to say, a common replacement $e_0 \rightarrow e$ may be made, wherever e_0 appears in any Feynman diagrams. It seems, however, that the necessity of this was not strongly felt by the researchers of the early period, including Tomonaga and his collaborators. In fact, these people's main concern was to make finite a transition amplitude as a whole, that is, without decomposing it into parts such as propagators, vertices and external lines. In such calculations the divergence of Z_1 or of Z_2 does not show up by itself, and their cancellation takes place automatically, provided the treatment of diverging expressions is properly made.

Consider, for example, the case of charge renormalization. According to the recipe of Dyson, the effect of vacuum polarization Z_3 should be split into two ${Z_3}^{1/2}$'s, one to renormalize the e_0 of the photon source and the other to renormalize that of the photon sink. In Tomonaga group's calculations this is not systematically made, however: the whole Z_3 is rendered either to the source or to the sink. Thus, in the e^2-approximation the self-charge δe, defined by $\delta e \equiv e - e_0$, differs from the correct value by a factor 2.

Some years ago I asked one of Tomonaga's collaborators about this problem, and his reply was that what is required of renormalization is only to make finite all amplitudes individually. From this I got the impression that for him it did not matter whether the renormalized charges appearing in a given amplitude take the same or different values. Incidentally, such an attitude may have not been independent of the general atmosphere prevailing at that time. In our student days we were taught by teachers and by textbooks as well that the problem of self-mass and self-charge is an academic one which cannot be checked experimentally. As far as I know, Sakata was the first to emphasize observability of self-masses.

2. Dawn of the History

After this brief prelude I now come to my main theme: I shall begin by telling you how Y.T. started working on W-T relations. Details of the story have already been described ,however, by Y.T. himself in an article written (in Japanese) in 1988 [4]. So what I have to do is simply to quote some lines from there.

The story dates back to the year 1955. Y.T. then was at Iowa: as a research associate he spent two years (1955-57) there, working with J.M. Jauch and F. Rohrlich. The problem Jauch suggested to Y.T. was to tidy up one of Källén's papers [5], claiming that at least one of the renormalization constants Z_1,Z_2 and Z_3 in QED should be diverging. Jauch found the paper untidy, that is, the arguments and calculations were so lengthy and complicated that it was not easy to see how and why such a conclusion could be reached. With this problem Y.T. struggled in vain for two years. Although unsuccessful in attaining his original purpose, he got a by-product whose implication he did not, according to him, fully realize at that time. Jauch and especially Y.Nambu, however, took a strong interest in this result, and persuaded him to publish it. So he wrote a short paper, and sent it to Nuovo Cimento for publication. And this is the paper [6] whose 40th birthday we are celebrating today.

At the time when Y.T. started the work, several authors had in fact been referring to a generalized form of Ward identity. Among such papers Y.T. knew, as he writes, those by Green, Fradkin, Landau, Källén,... (*cf.* Jackiw's talk for historical details [7]). From there, however, it was not clear at all whether the formula was a mere conjecture or the one rigorously proved. It turned out in any case that Y.T. was the first to publish a theoretically satisfactory derivation. Incidentally, about this time people in some other quarter were also trying the derivation, but hindered by what they called "Yennie trouble". Y.T. says that he was quite free from such a trouble, simply because he was not aware of its existence.

Recalling those days Y.T. writes that although certainly derived the relation, he did not understand the meaning of what he did. That is to say, he simply played with some basic equations of QED and combined them in such a way as to obtain the required relation. But, he had no idea at all as to why such a relation is theoretically possible, or what the basic principle is, which underlies the validity of this relation.

On the other hand, Nishijima in the meantime (1960-61) published a series of papers [8] on Green's functions, time-ordered products, etc., and thereby clarified all those problems that had been worrying Y.T. To the best of my knowledge, it was in one of those papers that Nishijima introduced, for the first time, the term "Ward-Takahashi Identities". Later Y.T. himself made an extensive study on, and further generalizations of, W-T relations [9]. It was only after such researches that Y.T. came to confidently say his last word in the problem. That is, the basic principle that makes the W-T relations possible is nothing but the canonical structure of quantum field theory, as expressed by $i\hbar\delta A(t) = [A(t), G(t)]$, where $A(t)$ stands for an arbitrary functional of canonical variables, and $G(t)$ for the generator of an arbitrary, infinitesimal canonical transformation. Thus this, I believe, is his present understanding of the problem.

So far I have been talking about the dawn of the history. As for further developments, i.e., the history itself, many papers have been read at this meeting, and I myself need not enter there.

3. Y-T Phenomenology

In this section I now come closer to Y.T. Let me first show you how his physical structure developed since December 12, 1924 when he was born in Osaka, Japan. A few months after this date he looked like these [A1,2 in Appendix]. Then my photo collection forces me to jump to the years 1948-53 when he was an undergraduate and then graduate student at Nagoya University: here he is with two gentlemen who worked together at that time – H.Umezawa (H.U.) in the middle and myself [A3]. In the years 1953-57 he was on the American continent as a post-doc or research associate, staying in Rochester (1953-54), Ottawa (1954-55) and then Iowa City (1955-57). For example, on September 26, 1956 when I met him in Chicago, he looked like this [A4].

After submitting *the* paper to Nuovo Cimento in 1957, he went to Dublin, Ireland, where he stayed until 1968 as scholar, as assistant professor and finally as full professor. During this period I was in London, met him often, and in fact wrote a number of papers together. These are some pictures I took in Dublin [A5,6]. Here he looks, I should say, most sharp and intelligent, and moreover – surprisingly enough – slim and smart! In 1968 he came back to the American continent to take up the post of professor at University of Alberta, and since then he has been here in Edmonton, except for some years spent abroad as visiting professor [A7,8]. And at present his appearance is something like this [A9,10].

As already mentioned, Y.T. came to Nagoya as a physics undergraduate in 1948, and I did so one year earlier. So we should have met each other around this time. I do not remember, however, whether I had actually talked with him before 1950. In the spring of this year he, as a 3rd-year student, joined Prof. Sakata's laboratory to write his graduation thesis. As I had already been there, we soon started working together and also with H.U. who was at that time research assistant to Prof. Sakata.

In the following few years both Y.T. and I learned from H.U. how to make researches, or more precisely, H.U.'s way of doing so. In fact, we were trying to catch up with him by accepting all he says and even by imitating all he does. In 1953 Y.T. left Nagoya for Rochester and has been abroad ever since. As I shall tell you later, this was, in my opinion, very good for him, for the experience acquired in the subsequent years abroad enabled him to become free from the H.U. physics and to establish his own.

What, then, is the H.U. physics?, what characterizes the Y.T. physics?, how do they differ?,······: to answer these questions will be my task in the following sections.

4. Methodology I: 3 stages in understanding

The basic method I usually employ in discussing the character of a physicist is the following *3-stage theory* of understanding, or of thinking in general.

According to Antonio Gramsci (1891-1937), a great Italian thinker, there are three stages in, or three degrees of, understanding [10]: in order to reach a real understanding one should go through the

> "Passagio del sapere al comprendere, al sentire, e vice versa,
> del sentire al comprendere, al sapere".

Here, *sentire* = *feel*, *comprendere* = *comprehend* and *sapere* = *know*, corresponding to what I shall hereafter call the 1st, 2nd and 3rd stages, respectively. Although Gramsci advocated this in reference to social problems, I think it applies as well to other problems in life – especially to understanding physical theories, or more generally, to the way in which theoretical physicists relate themselves with physical theories. Incidentally, it may be of interest to compare the above three stages with those of the Kantian or Hegelian philosophy: Sinnlichkeit - Verstand - Vernunft or Anschauung - Analyse - Synthese. In this connection I should say furthermore that in order to deepen one's understanding it is necessary to repeat such downward (3rd $\rightarrow$ 1st) and/or upward (1st $\rightarrow$ 3rd) passages. So, let me call hereafter the transition from a stage to its neighboring one a *Gramsci passage*.

Now, the above three stages may be adapted to theory-understanding in theoretical physics as follows. Suppose that for a certain problem (say, superconductivity) a theory, to be referred to as theory A (say BCS theory), is known. Then, at the 1st stage, by practice, i.e., by applying theory A to various related phenomena (say, Meissner effect, isotope effect, Josephson current,$\cdots$) one learns and enriches the basic knowledge and practical techniques. In this way one gets well acquainted with the subject. The purpose here is to see by oneself whether theory A well agrees with all experimental data concerned. In so doing one is allowed to invoke all possible ways of reasoning such as analogy, intuition, guessing,$\cdots$ in addition to logical reasoning: here their mutual connection may, for the time being, be left out. Thus, the 1st stage is the *stage of practitioner.*

At the 2nd stage one reconsiders the whole problem in a purely theoretical manner, i.e., on the basis solely of theory A and by means only of logical reasoning: here all other ways of reasoning mentioned above should be excluded. The purpose here is to see by oneself whether theory A can stand by itself, or be right as a theory. Thus, the 2nd stage is the *stage of theorist.*

Lastly, at the 3rd stage one tries to see how theory A is related to, and founded upon , general principles of physics, such as causality, locality, invariance (under symmetry transformations), unification, etc. The purpose here is to see by oneself how and why theory A is made possible. Thus the 3rd stage is the *stage of natural philosopher.*

In studying theories intuitive people may start from the 1st stage and then go up, whereas logical or math-minded people may start from somewhere near the 2nd stage and then go up or down. Broadly speaking, the upward reasoning is mostly inductive, and the downward one is mostly deductive. What Gramsci emphasized is, however, that in order to reach a deep understanding one should make Gramsci passages in both directions: 3rd→2nd→1st and 1st→2nd→3rd. And for further deepening it is necessary, as I stressed above, to repeat such passages as many times as possible.

It is to be remarked here that to remain at the 1st stage, that is, to familiarize oneself with a given subject by acquiring some amount of experience and by mastering practical techniques does not necessarily mean, or is often far from, a real understanding of the subject. The great philosophers are thus warning us. Hegel says "Das Bekannte überhaupt ist darum, weil es bekannt ist, nicht erkannt" (One cannot say that one understands something well, simply because one has got used to it) [11], and according to Kant "Gedanken ohne Inhalt sind leer, Anschauungen ohne Begriffe sind blind" (Conception without perception is empty, whereas perception without conception is blind) [12]. It may be true indeed that remaining only at one of the three stages, one sees merely the individual trees separately and not the entire structure of the woods.

5. Methodology II: 2-dimensions of theorists' working

Let us consider a 2-dimensional space in which individual theorists work. Here, the vertical or y-axis represents one's degrees of theory-understanding: thus $y = 1, 2$ and 3 correspond, respectively, to the 1st, 2nd and 3rd stages mentioned above. The horizontal or x-axis, on the other hand, represents how intensive one's working is at given y, and/or how large the scope is of his activity, interest, etc. Broadly speaking, the direction of y-axis implies 'logical climbing', whereas that of x-axis corresponds to 'intuitive wandering'. The working domain of any theorist may then be characterized by a 2-dimensional area such that $|x - x_0| \leq \Delta x$ and $|y - y_0| \leq \Delta y$.

I am now in a position to assert the following. Theorists are generally classified into two types, depending on the shapes of their working domains: the *Yoko* (or *horizontal*) *type* if $\Delta x \gg \Delta y$, and the *Tate* (or *vertical*) *type* if $\Delta x \ll \Delta y$. The two types have contrasting features in various respects. Theorists of Yoko type rely mostly on intuition, whereas those of Tate type rely on logic. The former are always eager to make Δx as large as possible, and in so doing they do not mind making, so to speak, logical jumps, thus the results often lacking theoretical rigor. On the other hand, the latter's main concern lies in making their formalism more and more complete as theories, but their Δx's are, relatively speaking, not so large as those of the former. Two typical examples are, if chosen from among Japanese theorists, Yukawa for Yoko type and Tomonaga for Tate type.

Before going into analyzing Y.T. who is originally from Japan, I may perhaps spend a few words about Japanese theorists in general. As far as I can see, the majority of them is content with remaining at $y \approx 1$, and with trying only to increase Δx. It even appears to me that they are apt to ignore the existence of, or the necessity of going up to, higher values of y. That is to say, they are mostly of Yoko type.

In my opinion, however, such is not a tendency only in the recent times, but deeply rooted in their historical background, i.e., their own culture. Here, I have no time to go into details, and so give just one example to illustrate my point. This is about a Japanese 'mathematician' Kowa Seki (1642?-1708) and his *wasan* school. He was contemporary with Newton and Leibniz and found by himself differential and integral calculus. Further, his school knew, for example, Euler functions before Euler and the Laplace expansion (of determinants) before Laplace. They always busied themselves with the activity in the x-direction, and never tried to climb up in the y-direction, i.e., to base their knowledge upon something more fundamental. Consequently, they did not reach a scientific system of mathematics. In this connection I should further say that such a difference existing between East and West is not restricted only to science, but also seen in other aspects of culture. In fact, many of Japan's conflicts with other countries come, I believe, from just the same source. Unfortunately, what Kipling wrote many years ago seems to be true even at the present time: "Oh, East is East and West is West, and never the twain shall meet..." [13].

So much for my methodology. I should now turn to its application, that is, analyzing Y.T. as a researcher.

6. Y-T Analyses: style of physics

In observing and analyzing Y.T. I find it convenient to set up a frame of reference, and to place at its origin H.U. who is also a very familiar figure to you here. This is because by comparing with H.U. the picture I draw of Y.T. will become, hopefully, more direct and concrete.

As I told you already, in 1950 Y.T. joined H.U.'s research group where I had been since the previous year. At that time the immediate goal of both Y.T. and myself was to reach the stage of the H.U. physics as soon as possible. This means that the style of the then Y.T. physics was more or less the same as H.U.'s, which thus fixes Y.T.'s initial condition. As you will see in a moment, what distinguishes Y.T. from H.U. is, however, that the change from this initial condition which had occured to Y.T. afterward was very large as compared with that of H.U.

So at any rate, I have first to explain to you how the H.U. physics was at that time. Now, in terms of our methodology H.U. by nature was of Yoko type, and remained around $y = 1$ throughout his research career, whether he liked to do so or not. As a matter of fact, he shared with other Yoko-type researchers many of the features characteristic of that type which I have described above. He was concerned more with generalizing what he has in hand than with completing it as a theory. In other words, he was more interested in enriching the contents of a work than giving to it a logically coherent structure. Now, generalization needs inductive arguments, which in his case were often made, I might say, a little too hastily.

Perhaps, the following episode may illustrate the situation. In the years 1950-52 we three were trying to formulate the scattering theory in terms of Heisenberg field operators [14]. But, even at the stage where the formalism itself was incomplete, we hurried to its application to realistic phenomena, including multiple production of mesons. I cannot deny, of course, that such an attitude of ours was due partly to the *Sakata realism* that had been dominating our laboratory. So, at any rate our formalism did not reach theoretical completeness such as attained later by the LSZ-paper [15]. I can therefore say that such was H.U's and hence Y.T.'s style of physics in their early period 1950-53 at Nagoya University.

As mentioned above, the difference between the two lies in their ways of time evolution. While H.U. was, so to speak, precocious, Y.T. was late blooming. The former established himself as researcher in his early twenties, and remained basically the same throughout his career. It was rather, I should say, a tragedy for him that although acutely feeling by himself the necessity, and thus trying hard, to get out of the initial mold, he did not or could not succeed in doing so, probably up to his own satisfaction. Y.T., on the other hand, was more flexible in structure, and was able in fact to gradually transform himself as his experience is enriched in the course of time.

It was, I believe, at his Dublin period that a kind of phase transition took place in his style, thereby enabling him to get out of the H.U. physics and to establish his own. As he often tells me, this was due to the strong influence of Profs. J.L. Synge and C. Lanczos of the Dublin Institute for Advanced Studies (D.I.A.S.). Both were great masters of classical physics, educated in the good, old European tradition and - in Y.T.'s own words [16] - "invited him to the realm of fundamental physics by asking awkward questions".

In fact, I myself have witnessed one of such awkward questions. Incidentally, around 1960 I was working in London and often visited D.I.A.S. by his invitation. At the then institute people used to gather in a library room for tea on Monday, Wednesday and Friday mornings: the tea was naturally followed by discussions, which often lasted into lunch time. On one of such occasions, I remember, Y.T. started discussions by saying "Let $\Psi(x)$ be a field operator". As soon as he utterd this, Synge's question interrupted him: "Is it something like a matrix?". Y.T. answered "Yes". Synge continued, of course: "Then, what are the meanings of row- and column-indices?". With this, totally unexpected question Y.T. got stuck, having no words to say. Here Synge was essentially asking about the representation problem of field operators. Nowadays, everyone knows that this is the first question to ask in quantum field theory, but in those days most people were so naive or simple-minded that they had never felt the necessity of asking such questions. At any rate, while Y.T. was considering the problem only from the 1st stage, Synge's question was being asked from the 2nd stage. I am sure that Y.T. at D.I.A.S. must have had a great deal of similar experience with the two professors, and this in effect lifted Y.T. up to their own level, that is, the 2nd or higher stages.

Lastly, I should not forget to add that Dublin is *the* very special place for him in another respect as well: this is where he first met his lifelong partner – Elizabeth.

7. Y-T Analyses II: research attitude

After Dublin Y.T. came to Edmonton in 1968 and has lived there ever since, thus having been outside of Japan for more than 40 years. During this period his physics has completely been remolded to the non-Japanese or Tate type. I may summarize his attitude toward physics research at present as follows.

The basis of the Y.T. physics is quantum field theory: he has always been concerned with its formal aspect, generalization and application. And, what matters most to him concerning physical theories lies in the logical structure thereof. When writing his own papers or reading others', he wants to make clear above all the process of how certain conclusions can be logically deriverd from a given premise. In other words, what he pursues is, as it were, logically irreducible Feynman diagrams (LIFD) such that incoming (outgoing) lines correspond to the premise (conclusions), and propagators and vertices to the logical steps taken in the arguments.

And from this also comes, as I understand, his dislike of untidy arguments or untidy calculations: for him untidiness means those in which the LIFD cannot be seen clearly. Thus, in his case a research work will not be completed until the arguments and calculations thereof are logically and hence mathematically tidied up. I remember Prof. Synge very often saying during discussions "tidy up (or polish up) calculations", and certainly Y.T. must have inherited this habit of saying.

To illustrate what I mean by tidy, let us take a few examples. Källén's paper on QED quoted above [5], in spite of the equations being very sophisticated, was not tidy, or not tidy enough for young Y.T.(as well as most of us now) to understand the paper. I may perhaps say that Källén at the time of writing this paper was merely a QED phenomenalist, working at $y \approx 1$. I said in the above that Tomonaga was of Tate type – an exceptional case among the Japanese theorists of his generation. Yet when compared with Schwinger, his formulation, for example, of the Tomonaga-Schwinger theory is less tidy, and hence less convenient for practical use, than Schwinger's.

Let me now revert to the case of Y.T. I have always had the impression that a considerable difference exists between his papers and H.U.'s. In order to understand a paper by Y.T. I have only to follow the sequence of equations just by inspection. On the other hand, to do the same for a paper by H.U. I have to check by myself all the equations one after another: otherwise the LIFD would not be clear. Such a difference is obviously reducible to the difference in tidiness of their papers: the former is much tidier than the latter. Furthermore, this difference in tidines may in turn be reduced to the difference in their ways of making researches. Y.T.'s argumentation in most of his papers is downward in the y-direction. That is to say, he starts from something definite and solid theoretically, and derives deductively something else which is equally definite and solid. On the contrary, H.U.'s argumentation is upward in most cases. He starts from something special and then tries to obtain something general inductively. Inherent in such generalization, however, is certain arbitrariness, which does not always make the results unique and definite. Under such circumstances it should rather be difficult for anybody to describe the results in a tidy form.

In recent years Y.T.'s description is becoming tidier and tidier. What I particularly like in him is that his arguments are brief and concise, containing nothing inessential, so that I can easily see LIFD's there. In summary, Y.T. is a theorist of Tate type.

8. The Present

Theorem (H. Morinaga) [17]:

> One's life is symmetric with respect to age a, where the central point a is given by $a = 40{\sim}45$ years of age.

Stated otherwise, what one is doing at the age of $a + x$ is the same as, or similar to, what he or she was doing at the age of $a - x$. Prof. Morinaga can give many examples that beautifully conform to this theorem, but has never taught me how to prove it. At any rate, when young, one has interest in many things, but in due course he or she has to choose a profession and becomes a specialist in a certain limited field. This specialization reaches the maximum at the age of a, and afterward the movement proceeds in the opposite direction.

I now apply the theorem, of course, to Y.T. himself. In his case it seems appropriate to take $a = 45$: at present $a + x = 73$, hence $a - x = 17$. Now at the age of 17 young Y.T. was eagerly learning general physics at high school, and at present old Y.T. is eagerly writing textbooks on general physics, covering such fields as classical mechanics, electromagnetism, relativity, statistical mechanics as well as classical and quantum field theories. The number of textbooks he has so far written amounts to 14, and two more will soon be coming. His versatility here is really astonishing and may well compare with Landau and Lifshitz. These books, excepting one, are written in Japanese and sell very well in Japan: in fact, some have been best-sellers for many years.

If asked about the reasons for his success in this genre, I could immediately enumerate the following. (1) His Japanese is of a unique style: simple, brisk, direct – nevertheless very persuasive – and occasionally humorous. This makes the readers quite relaxed even when they are learning difficult matters. (2) For what he considers important he spends many pages, but for others passes fairly quickly. The balancing of these two is exquisite. (3) Those taken for granted by other authors are often examined carefully and in detail, thereby leading to nontrivial or important consequences. (4) Where his understanding is not clear, he prefers saying so honestly, rather than covering this up with hand-waving arguments. And last but not least, (5) as in the case of research his main aim here lies in making the logical structure clear.

Some people around me are often wondering why Y.T. has been so much keen on writing textbooks. To me, however, the reason is obvious: it is simply a consequence of the Morinaga theorem. That is, textbook writing provides him with a media through which to express his own, overall view on physics in general, a kind of thing which could not easily be attained by research-paper writing.

Very recently he has started writing even novels (in Japanese) – mostly love stories of short or medium length, that are based, as far as I can see, upon his rich experience in this field. One of the stories has been translated into Chinese and favourably received in the Chinese community as well. Among the literary techniques he employs the following is especially noteworthy. He likes to express something abstract (such as love) by means of something concrete (such as a sculpture) : in this way the reader is given an impression such that a subjective and ephemeral feeling is transfigured to, and thus fixed in the reality as, an objective and everlasting existence.

So much for the Present of Y.T. As for the Future I can say only the following, since my time has already been used up. Let me hope that ten years later all of us, including of course Y.T. himself, will be gathering here again to celebrate 50 years of W-T relations, and I in particular will be talking about 60 years of Y-T relations. And with this I would like to close my talk – a tribute I can pay to Yasushi Takahashi on this occasion.

Thank you very much for your attention.

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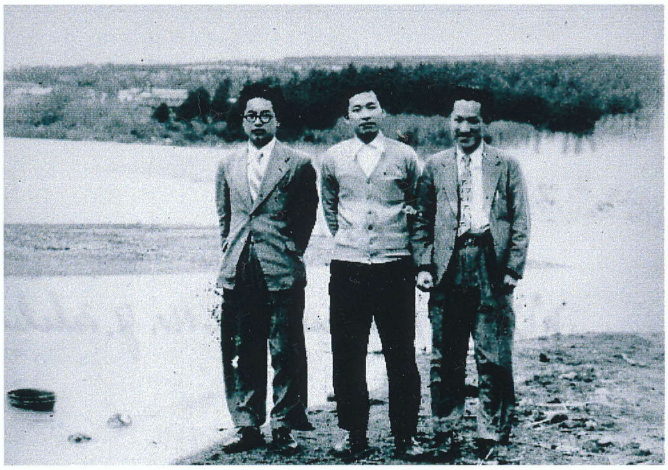
Appendix : Evolution of the Physical Structure

A1 1925

A2 ~1925

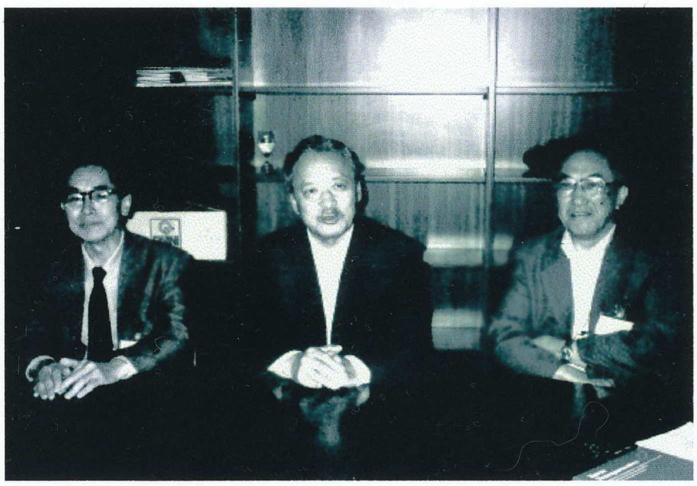


A3 May 2, 1951 (Hakone)

A4 Sept. 26, 1956 (Chicago)

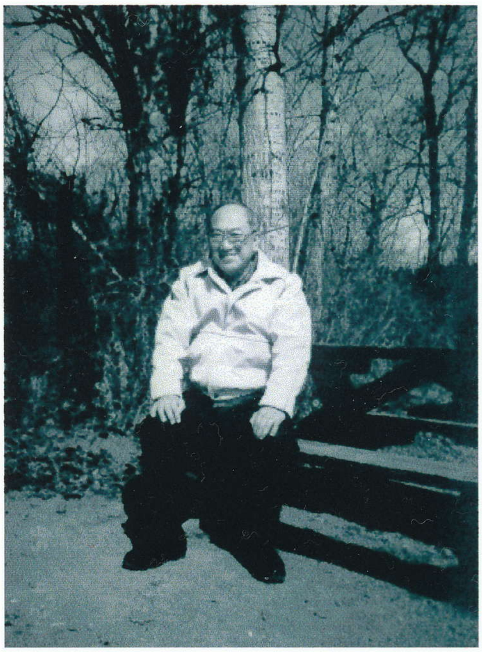
A5 Feb. 18, 1959 (Dublin)

A6 Feb. 18, 1959 (Dublin)



A7 May 31, 1992 (Perugia)

A8 May 31, 1992 (Perugia)



A9 April. 20, 1995 (Edmonton)

A10 Nov. 23, 1996 (Tokyo)