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A Conversation with Chin Long Chiang

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Abstract. Chin Long Chiang, Professor in the Graduate School, University of California, Berkeley, was born on November 12, 1914, in Ningbo, Zhejiang Province, China. He received his B.A. degree in economics in 1940 from National Tsing Hua University in China; his M.A. degree in 1948 and his Ph.D. degree in 1953, both in statistics from University of California, Berkeley. Dr. Chiang was on the U.C. Berkeley faculty for 36 years and has served as Chairman of the Program in Biostatistics, of the Division of Measurement Sciences and of the Faculty of the School of Public Health, and as Co-chairman of the Group of Biostatistics. When he retired in l987, the University honored him with "The Berkeley Citation" award for his "distinguished achievement." He was recalled to active duty in 1996. Dr. Chiang has been invited as a visiting professor at the following universities: Harvard; Yale; Pittsburgh; North Carolina; Emory; Michigan; Minnesota; Texas; Vanderbilt; and Washington at Seattle. He has given courses at Peking University, Beijing Medical University and Tongji Medical University, all in China, and at Tunghai University in Taiwan. In addition to his many scientific articles, he has published four books, two of which are about stochastic processes. Three of his books have been translated into Chinese and one into Japanese. Professor Chiang is a Fellow of the American Statistical Association, of the Institute of Mathematical Statistics and of the Royal Statistical Society of London. He has served as a special consultant to several national and international agencies. Professor Chiang is residing with his wife in Berkeley, California. They have two sons and one daughter, and two grandsons.

This conversation with Professor Chiang was conducted by telephone and e-mail from his office in the School of Public Health, University of California, Berkeley, in the weeks of October 26 and of November 2, 1998. Portions of this conversation have previously been published in the *International Chinese Statistical Association Bulletin*, July 1998.

EARLY DAYS

Li: Professor Chiang, I have known you for a long time. I never asked you about your background. Maybe you could tell me how did you get interested in statistics?

Chiang: I was always interested in mathematics and physics since I was very young. When I was at Tsing Hua University in Beijing as a freshman in 1936, my major area was physics. Following the July 7, 1937, incident that started the full-scale Japanese invasion of China, the university moved to Changshia, Hunan Province, and joined Peking University and Nankai University to form a Temporary University, which moved again to Kunming, Yunan Province, in February 1938. The situation in the university was very chaotic. Some students went to the army; some changed their majors. Without any good reason, I changed my major to economics. Courses in economics did not interest me at all, except a course in "statistics." I decided then to study statistics as soon as there was a chance.

When I entered University of California, Berkeley, in the fall semester of 1946, I was registered in the Economics Department. But I changed my major to statistics during the second (maybe the third) week of the semester. At that time there was only a

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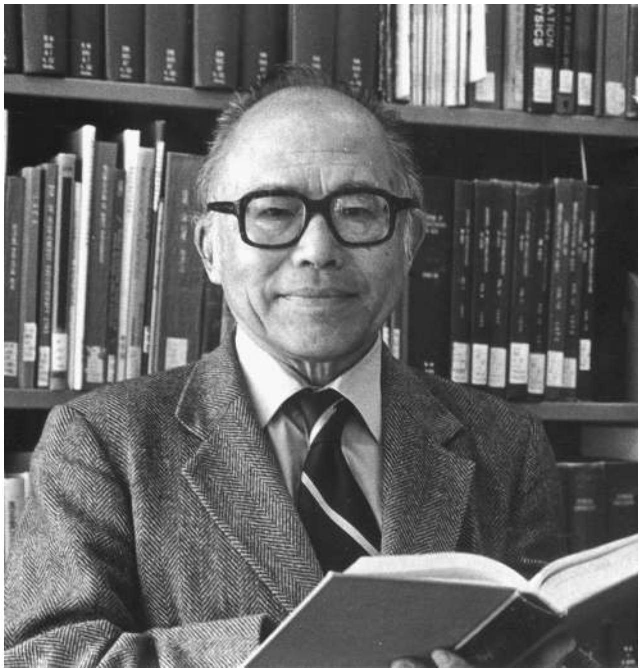

FIG. 1. *Professor Chin Long Chiang.*

Statistical Laboratory in the Mathematics Department, and no statistics department, on the Berkeley campus. The director of the Statistical Laboratory was Professor Jerzy Neyman, one of the founders of modern statistics. He became my advisor ever since. I worked in the Statistical Laboratory as a research assistant for a few years.

FROM LIFE TABLE TO SURVIVAL ANALYSIS

Li: You obtained your Ph.D. in statistics. What brought you to biostatistics and the life table?

Chiang: My career in biostatistics started in 1951. At the recommendation of Neyman, I went to the Biostatistics Program in the School of Public Health as a teaching assistant. But I gave courses! One of the courses I was assigned to teach was the life table. That was the first time I ever heard something called the "life table." Because of the lack of my knowledge in that area, the life table course was difficult for me to teach. I had to memorize all the formulas which were not meaningful to me at all. Since all I knew was mathematics, I reconstructed the topic "life table" from a mathematical point of view. The result was the life table method of analysis. I published my work in 1960–61 in three articles under the common major title "A stochastic study of the life table and its applications." They were later included in my 1968 book: *Introduction to Stochastic Processes in Biostatistics*, published by J. Wiley.

In addition to deriving probability distributions of the life table functions, I also have proposed a method of "life table construction," that is, a formula of conversion of an age-specific death rate to the probability of dying during the same age interval, such as (5, 10). The formula was published in *Vital Statistics—Special Report* (Chiang, 1961), and derived from an analytical viewpoint in *The Journal of American Statistical Association* (Chiang, 1972).

Li: Why is the conversion from rates to probabilities so important?

Chiang: Because the rate is what we observe in a population and the probability is what we need in the theoretical development. Without a formula of conversion, we cannot find the probability and hence cannot do our theoretical work correctly.

Li: So a conversion formula is needed not only in mortality analysis, but in some other fields of research as well. Is my inference from your answer correct?

Chiang: Yes, your inference is correct. In theoretical demography, for example, a conversion is needed from a birth rate to a probability of birth. I believe that was the problem encountered by Alfred Lotka about 60 years ago when he developed the intrinsic rate of natural increase. Lotka was a true mathematician and his intrinsic rate of natural increase was the foundation of modern demography. Almost every demographer knows it and many have computed it. But there is something wrong with the intrinsic rate!

In his derivation of the formulas for the intrinsic rate for the current human population (Dublin, Lotka and Spiegelman, 1949), Lotka used the birth rate instead of the probability of birth for each age interval. As a result, the exact meaning of the intrinsic rate was unclear; certainly it was not the reproductive rate of the population as Lotka had intended. Probably Lotka did not have a formula of conversion of a birth rate to a probability of birth at his disposal. And a probability of birth over a five-year age interval is not useful anyway, since a woman may have more than one birth during the interval. These could be the reasons why Lotka kept the birth rates in his formulas. That was a mistake.

Li: What should one do then?

Chiang: The thing to do is to abandon the age interval. I used the parity of a woman (the number of children she has) instead of the age interval, and derived a formula of conversion to obtain the probability that a woman of given parity will have a (female) birth during her reproductive period. And then using Lotka's concept of generation I developed a formula of "true rate" of growth for the current population. The "true rate" is the the rate of growth of a (hypothetical) population if the women had the same parity-specific birth rates as those prevailing in the current population under consideration. Since the parity-specific rates are observable in the current population, the "true rate" can be computed.

The true rates of growth have been computed for a number of populations. The rate for the 1987 U.S. white population was $R = -0.0037$. This means that the U.S. white population size was decreasing in 1987, and was losing 3.7 females for every 1,000 women per year. I reported my work at the 47th Session of the International Statistical Institute (ISI) in Paris in 1989 and published it in 1991 (Chiang, 1991a).

Li: You had correspondence with another mathematician in the field. Had you not?

Chiang: Yes, I had. That was with E. B. Wilson of Harvard University.

While there are formulas for most life table functions in my work, I was not the first person who derived the formula of the variance of expectation of life. The honor should go to Wilson. Back in 1938, Wilson already had published an article entitled "The standard deviation of sampling for life expectancy" (Wilson, 1938), except that his formula contained an error. Although he used a different method from mine, his formula should be the same as my formula. In my correspondence with him in 1958, I used his method and arrived at my formula. I suggested that he publish the corrected formula himself. He declined: "I am out of this line of work. . . ." In 1966 I published the correct formula with his method, as my tribute to Wilson (Chiang, 1966).

Li: I understand that you did not specify any function in your life table work. Have you published any survival functions in general?

Chiang: Yes, I have proposed two survival distributions: a "staging distribution" in 1979 (Chiang, 1979) and an "unnamed distribution" in 1989 (Chiang and Conforti, 1989). Let me give you some rationale for each of the two distributions.

First, the "*staging distribution*"—development of chronic illnesses is characterized by stages: from a mild stage through intermediate stages to severe stages to death. The disease process often is irreversible, but a patient may die while being in any one of the stages. In the natural progression of cancer, for example, there are stages of the disease determined by the size of tumor and metastasis of cancer. Diabetes, cardiovascular diseases and many other chronic diseases all progress in stages. Such phenomena when studied carefully lead to a conceptual model, and when the conceptual model is expressed in the probability and statistical language, we have the staging distribution. In developing the formulas for the distribution of the lifetime of a patient, I relied on some identities in a "useful lemma" found in 1964 (Chiang, 1964). But to find a simple formula for the expectation of the lifetime that can be explained intuitively, I derived three more identities.

Although there are transitions from one stage to the next, the staging process is quite different from Markov processes. First, the stages are ordered in the staging distribution, while the states in any Markov process are not ordered; and second, the disease process in the staging distribution is irreversible, but the transitions among states in any Markov process are reversible. Thus the distinction between the staging distribution and Markov processes is very clear. In the description of the development of HIV, for example, the underlying process is the staging process, not a Markov process.

The staging distribution applies wherever the concept "stage" can be defined and the end result need not be death. In the reproductive process in a family, for example, birth orders may be regarded as "stages" and the process ends when a couple decides not to have more children.

Second, the "*unnamed distribution*"—this distribution was developed on my belief that there are two forces continuously acting on an individual to influence his survival and death. One force causes the mortality intensity function to increase, while the other causes the mortality intensity function to decrease. Their action and interaction decide the survival distribution.

As a concrete example, consider an individual who is continuously exposed to a low level of radiation and other toxic material in the environment. During a small time element there is a probability that the individual will absorb a unit of toxic material, and a probability that the biological reaction inside the human body will cause a unit of toxic material in the body to be discharged. Over a period of time, there is an expected amount of toxic material in the body of the individual. This amount leads to the mortality intensity function. The distribution was found useful in a study of time to tumor. Several other authors have since found it useful in their work. There are other examples of this kind. Emigration and immigration determine a population size; survival and death influence the expectation of life; and action and reaction are the underlying forces of one of Newton's laws.

Li: You also have made an important contribution to the theory of competing risks besides the life table. Who was the first person to use the term "competing risks."

Chiang: To my knowledge, the term "competing risks" was suggested by Neyman. But the origin of the concept of competing risks goes back to the time when Daniel Bernoulli read his memoir on the mortality from smallpox and the effect of inoculation in the French Academy of Science in 1760 (Bernoulli, 1760).

I got the idea of competing risks from my study of the causes of death in mortality analysis. While a person is exposed to various causes of death, he dies from only one cause. Thus there must be some "competition" among various causes for the life of the person. That type of thinking led me to study competing risks. I described the theory of competing risks in some detail in Chapter 11 in my 1968 Wiley book.

Li: A popular method of studying competing risks is one using the concept of potential lifetime and a multivariate approach. Would you comment on that?

Chiang: The multivariate method is not appropriate to use in survival analysis. The method may be briefly described as follows. Consider a situation where an individual is subject to r risks of death, denoted by $R_1, R_2, \ldots, R_r$. For each risk R_i, there is a potential lifetime (net lifetime) X_i, which is his (or her) lifetime if risk R_i were the only risk acting, for $i = 1, 2, \ldots, r$. What is observed is

$$Y = \min(X_1, X_2, \ldots, X_r). \tag{1}$$

The fallacy of the multivariate survival method becomes quite clear when we try to understand the meaning of the joint density function. Suppose that there are two risks under study: R_1 stands for cancer and R_2 for other risks of death. The bivariate density function is

$$\begin{aligned} &f(x_1, x_2)\,dx_1\,dx_2 \\ &\quad = \Pr\{x_1 < X_1 \le x_1 + dx_1 \text{ and} \\ &\qquad\qquad x_2 < X_2 \le x_2 + dx_2\}. \end{aligned} \tag{2}$$

For $x_1 = 40$ years and $x_2 = 50$ years, for example, formula (2) is the probability that the individual will die from cancer at age 40 AND will die from other causes at age 50. Since an individual cannot die at the two different times from two different causes, the density function in (2) has no meaning when used in survival analysis. And the multivariate survival analysis has no meaning. I made this point in 1991 (Chiang, 1991b).

The basic formula underlying the multivariate survival analysis is the corresponding joint density function similar to (2), not formula (1) above. Most authors who use formula (1) as the starting point of their work might not see the fallacy of the method they are using. In our approach, we assumed a force of mortality for each risk, and arrived at desired formulas without difficulties.

RESEARCH IN STOCHASTIC PROCESSES

Li: You mentioned your book *Introduction to Stochastic Processes in Biostatistics*. How did you become interested in stochastic processes and their applications?

Chiang: While still working on the competing risks, it occurred to me that if deaths are classified by causes, people who are living also should be classified into different categories. After all, people in different health conditions are subject to different risks of death and should be in different categories. That led me to study the finite Markov processes; each health category is a state in the Markov process. The mathematics in studying people moving among different health categories is similar to studying transitions of states in the finite Markov processes. My first publication in that area, entitled "A stochastic model of competing risks of illness and competing risks of death" in *Stochastic Models in Medicine and Biology* (Chiang, 1964), indicates a connection between biostatistics and stochastic processes.

During the years that I was teaching courses in stochastic processes at University of California, Berkeley, and elsewhere, I had the opportunities to do more work in that area. Almost all the results that I have obtained in stochastic processes are in my two books: *Introduction to Stochastic Processes in Biostatistics* (Chiang, 1968) and *An Introduction to Stochastic Processes and Their Applications* (Chiang, 1980).

Li: What are your main contributions to stochastic processes?

Chiang: My main contributions to stochastic processes are the following:

1. *In continuous time Markov processes*—two explicit solutions for the Kolmogorov differential equations. The first solution was published in 1964 (Chiang, 1964) and the second in 1968 (Chiang, 1968). Both are for the forward and the backward differential equations. Two solutions for the Kolmogorov differential equations when the characteristic equation has multiple eigenvalues (Chiang, 1980).
2. *In discrete time Markov chains*—two explicit formulas for the nth step transition probabilities $p_{ij}(n)$ and a formula for the limiting probabilities of $p_{ij}(n)$ as $n \to \infty$ (Chiang, 1980).
3. *An equality in stochastic processes* I introduced in 1974 (Chiang, 1974). Incidentally this equality has been used to derive an explicit solution

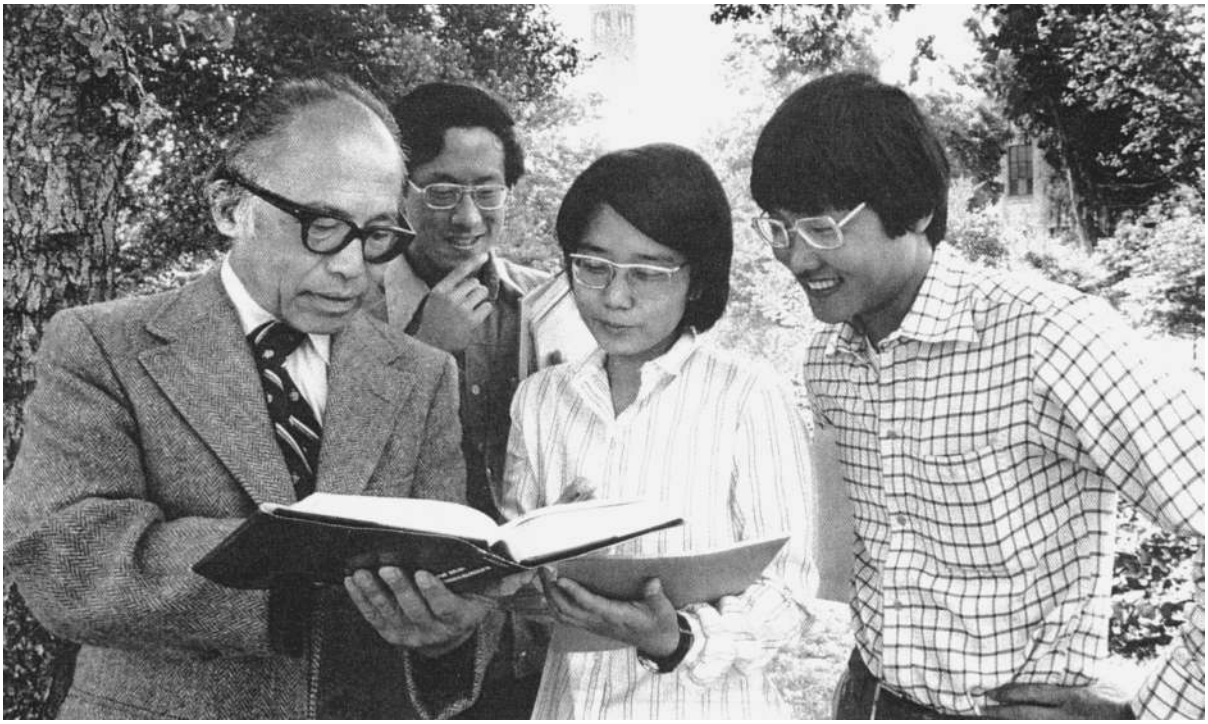

FIG. 2. *Professor Chiang with biostatistics students on Berkeley campus in 1980.*

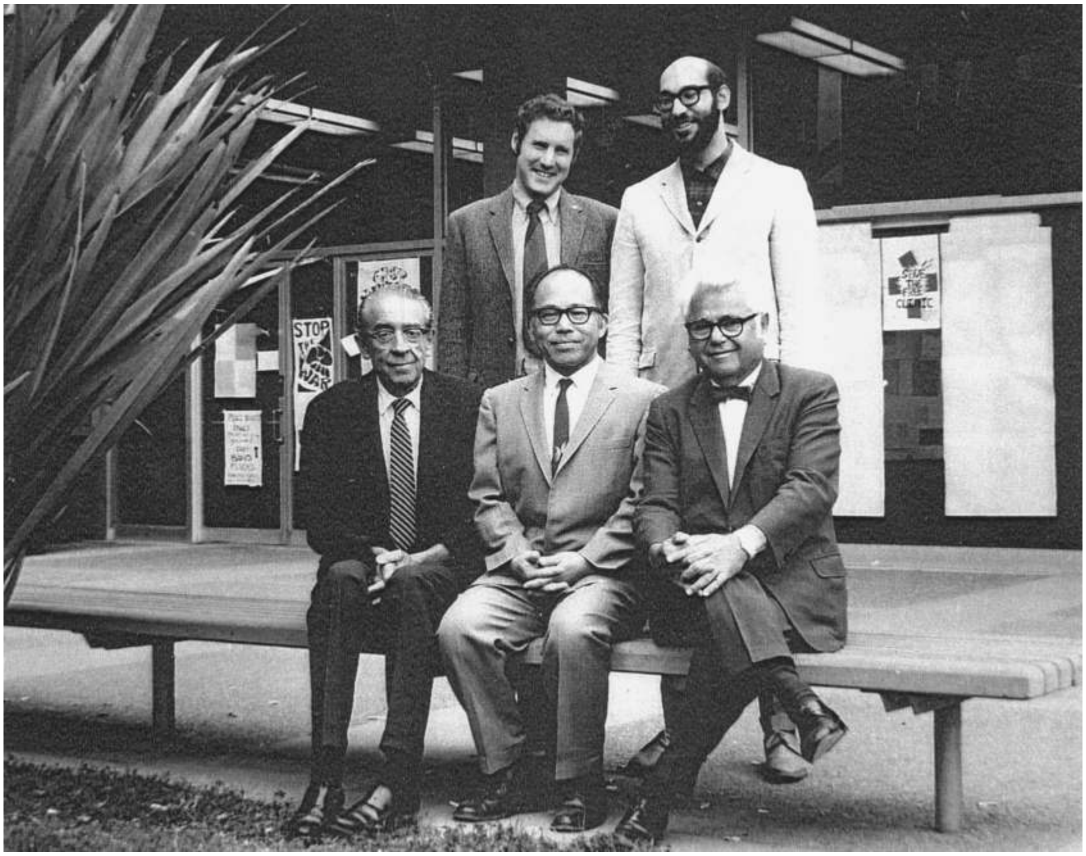

FIG. 3. *Professor Chiang with his colleagues in the Division of Biostatistics, University of California, Berkeley, undated.*

for the simple Kermack–McKendrick epidemic model (Yang and Chiang, 1971).

4. *In two-state processes*—formulas for the nth passage probabilities and the nth recurrent probabilities, and for the density functions of the nth passage time and of the nth recurrent time (Chiang, 1980).
5. *The general birth process* (Chiang, 1980).
6. *A staging process*—a new process, derived from my staging survival model (Chiang, 1980).

I would like to add that I am only responding to your question, but not claiming priorities. It is entirely possible that someone out there might have published any of the above items before I did. In any case, the priority should go to whomever published the result first.

Li: Professor Kai Lai Chung from Stanford University has done extensive research in the stochastic processes area, especially on Markov processes and Markov chains. It is my impression that you and Professor Chung went to the same high school. Did you have any interactions with him?

Chiang: You are absolutely correct. Kai Lai Chung and I went to the same high school and the same university. He was the pride of our university. Chung should be ranked the equal of William Feller and J. L. Doob in the development of probability theory and Markov chains. I suppose that theoretical people like Chung, Feller and Doob devote their effort to the theoretical development of the subject, and thus they leave explicit solutions for applied people to work out. Theory is the foundation of any science. Markov processes are no exception.

My interest in Markov processes and Markov chains has been, and still is, in the applications to practical problems. When I read Feller's necessary and sufficient conditions for the existence of solutions of the Kolmogorov differential equations, I wanted to know where was the solution! In a way, Feller's theory and practical applications prompted me to work on the solutions of the differential equations. As there is a certain distance between theory and applications, I never had any interaction with Professor Chung. I like to work on my own. More fun, that way.

Li: You must have some personal interactions with another well-known mathematician on the Berkeley campus, Professor S. S. Chern.

Chiang: Professor S. S. Chern is the top mathematician in the world. He is also a gentleman and scholar. I have known him for many years. He once gave me a reprint of one of his publications. In 1987 when I was teaching at Peking University, he invited me to give a report on my work at Nankai University. That was about the extent of the interaction I had with Professor Chern in mathematics. His mathematics is a high-level conceptual mathematics, while my mathematics is only applied mathematics.

I received my mathematics training at University of California when I was working for my Ph.D. degree. But I learned mathematics mostly from my working on problems. For example, in deriving solutions for the Kolmogorov differential equations when there are multiple eigenvalues, I learned much linear algebra and differential equations. When I found my mathematics not adequate to solve a problem, which occurred very often, I used my intuition and guessed a solution, or a formula. If my guess was wrong, no harm was done. If my guess was right, I got my solution. The following are a few examples:

1. A useful lemma appeared first in my 1964 University of Wisconsin paper, later in my Wiley and Krieger books. I had checked with many mathematicians over the years, nobody seemed to know the existence of the lemma.
2. The first formula for the nth step transition probability in Markov chains, appeared in my 1980 Krieger book, was my conjecture. I still cannot derive it, but I proved it by induction in my new book.
3. The general birth process in my 1980 Krieger book and in my new book was a result of my hunch. I thought that there should be a general formula representing all the birth processes, such as Poisson processes, Pólya processes, Yule processes, etcetera. Since I could not find one anywhere in the published works, I derived one myself. Many problems were difficult when I was working on them; they became trivial after I have found the solutions.

MEMORIES FROM EUROPEAN MEETINGS

Li: I understand that you attended statistics meetings in Europe often. How did that happen?

Chiang: Yes, I attended statistics meetings in Europe often. I attended the ISI Biannual meetings in London, Amsterdam and Paris. I also attended a few European Meetings of Statisticians, and special meetings. During the years that I was working on topics in stochastic processes, I often had some results to report.

It all began in January 1964; at the invitation of Peter Armitage, I went to England on a Fulbright Fellowship to give 13 weekly lectures on the theory of life table, competing risks and some stochastic processes. People in Britain had used life tables

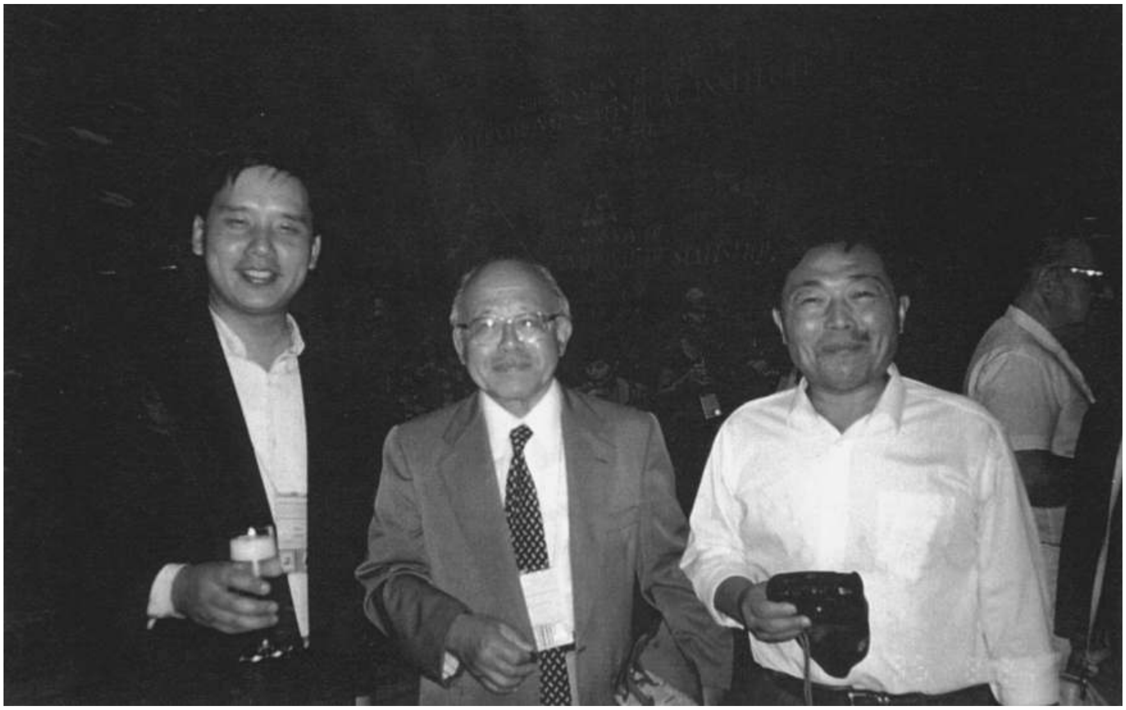

FIG. 4. *Professor Chiang with two of his previous Ph.D. students at the ISI 50th Session in Beijing, China, September 1995.*

for centuries and might also remember the origin of competing risks. Many of them seemed to have a personal interest or a curiosity to know the theoretical side of these topics. Armitage's skillful arrangement greatly helped the success of that lecture series. Subsequently, the University College in London and the King's College in New Castle also invited me. Finally, the Royal Statistical Society of London extended its invitation for me to talk on historical development of the life table at their annual meeting in the spring.

A few years later I had an appointment as consultant with the World Health Organization (WHO) in Geneva. The appointment lasted for about 10 years. The WHO even published a book for me. I also went to Europe at my own initiative. So I was on the continent quite often. I attended statistics meetings whenever I happened to be there. I suppose when your name is on somebody's list, you get the announcements and invitations to meetings.

For example, I was invited to present a paper on Markov chains at the Second Vilnius Conference on Probability Theory and Mathematical Statistics, in June–July 1977, in Vilnius, Lithuania, organized by the mathematicians in the then Soviet Union. I went to the then Soviet Union on a tour arranged in the U.S. and had visited quite a few cities, including Moscow, Leningrad (St. Petersburg), Kiev and Yalta, before the conference. During the period of the conference, I learned a couple of Russian words. So I wrote down the title of my talk in Russian on the blackboard before I presented my paper. The audience did not know that these were the only Russian words I knew; I was careful not to pronounce them. But I believe that the title in Russian language added some color to my presentation.

At one of the receptions, someone came to me and handed me an envelope. When I opened it, there was a large amount of money inside! I asked the man: Who gave the money to me, and why? He said it was from the Organizing Committee and he did not know why. Before I left the town, I invited the committee member who initiated "the money-giving episode" to an afternoon tea to express my thanks. We had a very good conversation. I believe people are nice when you get to know them, regardless of national origin.

In Wrocław, Poland, I attended the 14th European Meeting of Statisticians in August–September 1981, at the Technical University of Wrocław. I presented a paper and chaired a session at the meeting. The host institute organized a tour of the old castles in the outskirts of the city for the visitors. That was a treat.

While in Warsaw, at the invitation of the Institute of Mathematics, Polish Academy of Science, I gave a lecture on Markov processes. The Academy arranged for a Dr. Kupść to show me around the town in his automobile. So I have seen quite a lot of Warsaw: from the New City to the Old City, from the Palace in the Lazienki Park in the city to the Wilanow Palace to the southeast of the city, from Fryderyk Chopin Statue in the park to the house of Madame Marja Skłodowska Curie in the Old City. Warsaw was a beautiful city, rich in history.

In the Old City Square, there was a big plastic box for people to donate money to help rebuilt the old city that was destroyed during World War II. As I looked at the money in different currencies inside the box, I was very much moved by the generosity of the people who came from different parts of the world to visit Warsaw. Generosity recognizes no national boundaries.

At that time the Solidarity movement was in the upswing. The Solidarity Union claimed 10,000,000 membership throughout the country. But the movement was under a tremendous amount of pressure from the government to restrict its activities. One day I went to the Headquarters of the Solidarity Union for a visit. There were quite a few people buying souvenirs. "Is that the way the Solidarity Union raises their money?" I wondered. The movement very much impressed me. And I had developed a deep sympathy for the Polish people. In the morning when I was ready to go to the Academy to give my speech, I could feel the Solidarity movement in the air, and in my mind. That disturbed me. I wanted to do something to get that awful feeling out of my system. But what could I do? Finally, I put the necklace and other items that I had bought the day before to take home and all my zlotys (Polish currency) in a big envelope, and asked the cleaning lady to take the envelope to the Headquarters of the Solidarity Union. Only after that was I able to concentrate my thoughts on my speech. On December 13th of that year the Polish government imposed martial law on the nation.

STORIES FROM CONSULTATIONS

Li: As a statistician in public health, you must have many interesting problems from your consultations. Would you share some with me?

Chiang: Yes. There are quite a few. I shall describe four of them for you.

First, *Mosquito infection rate*—mosquitoes are the vectors of a number of viral diseases. The proportion of mosquitoes P in a field infected with virus is called the *infection rate*, which bears a direct relationship to the prevalence of the diseases in the human population. To estimate the infection rate, a team led by an epidemiologist had collected a large number of female mosquitoes from the field and made an effort to determine the number infected in the sample. But the infection rate was low and the cost in determining an infection was high. Even for a moderately large sample, $N = 500$, for example, it was impractical to make a separate virus isolation for each individual mosquito. In statistical language, we needed to estimate the population proportion P without knowing the sample proportion.

As it turned out, the problem was quite simple. All one needed to do was to divide the N mosquitoes into n groups, or pools, with an equal number (m) of mosquitoes in each pool, so that $N = mn$. Let X be the number of positive pools. The ratio X/n is an estimate of the probability $1-(1-P)^m$ that a pool is a positive pool, immediately leading to the estimate $\hat{P} = 1-[1-X/n]^{1/m}$. For example, for $N = 500$, $m = 25$ and $n = 20$, only $n = 20$ determinations of infection were needed, which was well within their reach. If $X = 10$ pools are found positive, then $\hat{P} = 1-[1-0.5]^{1/25} = 0.0273$, or the estimate of the infection rate in the field would be 27.3 per 1,000. It was now a simple matter to write down the likelihood function to obtain the maximum likelihood estimate $\hat{P}$. In our publication a maximum likelihood estimate of P was also given for the case when two pool sizes were used (Chiang and Reeves, 1962). I was informed in the 1970s that the pooling method was used in "group testing." Recently I have seen in publications that the pooling method was proposed to estimate the prevalence rate of HIV in the human population. All these indicate that the pooling method is a useful one.

Second, *Complement fixation test*—the test starts with seven test tubes containing an equal amount of diluent in each tube. They are called a "run" of seven tubes. First, a same amount of blood as the diluent is added to the first tube, so that the blood concentration in the first tube is 1 to 2, or $(1/2)^1$. Next, one-half of the amount of the liquid in the first tube is poured into the second tube, so that the blood concentration in the second tube is $(1/2)^2$, and so on. Finally, a half of the amount of the liquid in the sixth tube is poured into the seventh tube, so that the blood concentration in the seventh tube is $(1/2)^7$. A half of the liquid in the seventh tube is poured into a "waste" tube. As a result, there is the same amount of liquid in the seven tubes as there was to begin with, but with a geometric sequence of blood concentrations, from $(1/2)^1$ in the first tube to $(1/2)^7$ in the seventh tube.

The entire run of the seven tubes is tested for the presence of virus. The last tube with a positive reaction is called the titer of the blood. For example, if the first three tubes had positive reaction, the titer of the blood would be $(1/2)^3 = 1/8$. The entire procedure is called titration. As the test is specific for several particular viruses, it had been used in laboratories for the presence of complement, or antibody, as a means of diagnosis of viral diseases.

However, the results of the test vary; it often has different values of titer for the same blood specimen. In the 1950s, the Virus Laboratory in the California Department of Health decided to measure the consistency, or the reproducibility, of the test. They collected a large number of blood specimens from different people and performed a number (N) of runs with each blood specimen.

Suppose n_i of N runs were found to have titer $(1/2)^i$, for $i = 1, 2, \ldots, 7$, with $n_1 + n_2 + \cdots + n_7 = N$. What is the reproducibility of the test? One of my former students was assigned the job. She recognized that the mean of $(1/2, 1/4, 1/8, \ldots, 1/124)$ has no meaning, nor is the standard deviation a meaningful measure of reproducibility. So she came to me. After she described the problem, I suggested a probability to measure the reproducibility. Specifically, I recommended the probability (R) that two runs picked at random from the N runs will have the same titer as a measure of the reproducibility. In formula,

$$R = \frac{\sum_{i=1}^{7} (n_i/N) \binom{n_i}{2}}{\binom{N}{2}}.$$

They accepted my suggestion. My student did the computations, her boss reported his findings and everybody was happy, including me.

Third, *Blackjack*—one of my colleagues wanted to go to Las Vegas to play blackjack. She thought that because blackjack was a game of chance I should have a winning formula for her. By coincidence, the latest issue of the *Journal of The American Statistical Association* (JASA) had just arrived. The leading article was entitled "The optimum strategy in blackjack" (McDermott, Baldwin, Cantey and Maisel, 1956). The abstract of the article began with:

> "This article discusses the card game blackjack as played in the casinos of Las Vegas. The basic rules for the game are described in detail...."

I thought I could not have found a more suitable article for my friend. So my friend copied down the "basic rules" from the article and, with a great expectation, she was cheerfully on her way.

The following week my friend came back to work, but she seemed to avoid me. I became suspicious, until another colleague pulled me aside and whispered sternly: "Your optimum strategy was disastrous! She lost a lot of money! What was worse, she recommended your strategy to her friends. They lost money too!..." I did not know what to say, what to do. I felt very bad. My friend had trusted me, and I let her down. Those were the darkest days in my life as a statistician. Three years and three months later, in the December 1959 issue of *JASA* (Thorp, 1959), a reader published corrections on nine(!) mistakes in the original article. The authors concurred. At that time, I did not care whether the "rules" in the article would work with the mistakes corrected. So far as I was concerned, I was the person to be blamed for my friend's misfortune. I never should have recommended those "rules" to her without checking them out myself first.

Fourth, *Sperm bank*—in Los Angeles, California, there was a sperm bank for collecting sperm of Nobel prize winners, geniuses and other distinguished people. While it has never been publicly announced what the bank was going to do with this precious commodity, the message was clear: "Only geniuses produce geniuses!" Then there were some people in northern California who set up another sperm bank in the city of Oakland, of all places, to collect sperm of ordinary people with the intention of challenging the superior position of the sperm bank in the south. But they were depressed as their sperm bank had such an inferior reputation that even ordinary people hesitated to make their donations.

One day I received a telephone call from someone who identified himself as a friend of the Oakland sperm bank. He wanted to know if there was any statistical method he could use to show that the sperm in their bank were just as promising in producing geniuses as those in the Los Angeles bank. While I was not a contributor to either bank, my sympathy was with the Oakland group. So I asked him: "Do you know that ordinary people are more likely to produce Nobel prize winners?" "No, but how do you know?" He was pleasantly surprised by my question. I asked him again: "How many parents of Nobel prize winners were Nobel prize winners, and how many children of Nobel prize winners were Nobel prize winners?" "None!... Oh, I see your point, thank you very much!" The caller hung up, before I could say: "with the exception of the Curie family."

Of course, the purpose of asking him these questions was more to lift his spirit than anything else.

But I would be in a stronger position to challenge the Los Angeles sperm bank with a statistical test, if it were not for Madam Marja Skłodowska Curie!

STILL A BIOSTATISTICIAN

Li: You have developed the theory of stochastic processes and applied it to life sciences and biostatistics research. I consider these as parts of the biostatistics activities described in your 1985 *Biometrics* paper, "What is biostatistics?" What is your current view on the same question you tried to answer a decade ago?

Chiang: I still think that "... biostatistics is a discipline that is concerned with the development and application of statistical theory and methods for the study of phenomena arising in the life sciences," as stated in that paper. My work in stochastic processes really is not biostatistics; the theory of stochastic processes is not a part of biostatistics. In studying recovery from one disease and relapse of another, I found myself working in the area of finite Markov processes. Although I might have solved some problems in biostatistics with stochastic processes, these solutions apply also to other fields of research. Instead of changing my view of biostatistics, I would rather say that my work in biostatistics caused me to drift away from biostatistics proper to stochastic processes. To be accurate, I suppose I am wearing two hats, one from biostatistics and the other from stochastic processes. I get problems from one and solve them, if I can, with the other.

Li: You wrote nine articles in the recently published *Encyclopedia of Biostatistics*. Based on this fact alone, I do not think you are in a very strong position to say that you are not a biostatistician.

Let me change to a nonstatistical subject. I am practicing journalism on you without knowing anything about journalism. You must know much more about journalism than I do because there is a journalist in your family. Is that right?

Chiang: First, you are doing an excellent job as a journalist. Your performance definitely qualifies you as a journalist. In order to be a good journalist, one must know what questions to ask, and how to write. You surely know both very well. However, I would not therefore say that journalism is a part of biostatistics.

Yes, I have a journalist in my family. Every morning I read the *San Francisco Chronicle*. I enjoy reading the morning paper more when there is an article in the front page written by my daughter. That is all I know about journalism.

You are quite right that I cannot say that I am not a biostatistician. Although I might have used stochastic processes to work on problems in biostatistics. I am still a biostatistician.

It is interesting that you should have mentioned the *Encyclopedia of Biostatistics*. That was not entirely my own doing; there was a story behind it. One day in March 1997, long after the deadlines for submission of manuscripts to the editors had passed, I received a telephone call from one of the Editors-in-Chief. They had just discovered that they had not yet invited people to write 12 of the most important topics. He asked me for help. I should have answered: "Of course, I will be happy to write one article for you." But I committed to write nine! As a result, during nearly two months in that spring, I worked day and night to fulfill my commitment. Now I am happy that I was able to help my friends when they needed my help.

Li: You always like to help other people. You helped me to translate my Chinese academic documents into English when I needed them for graduate school admission in 1985. I still have copies of them.

Chiang: Thank you for saying that "you always like to help other people." Usually it takes only a little effort on my part to help others. So, why not? Of course I had the most opportunities to help my students. Also, like many teaching at the universities in the U.S., I also had opportunities of helping people in China to come as visiting scholars. Some of them are doing very well. But I rarely turned down a reasonable request for my help regardless of who requested. I remember one day in December 1994, a member of ICSA (International Chinese Statistical Association) asked me to prepare documentation to nominate him for Fellow of the American Statistical Association. Since I did not know much about him, even less of his work, I had to spend a great deal of time to read and digest all the material he sent me. I prepared as good a documentation as I could. Subsequently, he was elected. The chair of the ASA Committee on Fellows congratulated me. That made me very happy.

DEVELOPMENT OF BIOSTATISTICS—A PERSONAL ACCOUNT

Li: You have been working in biostatistics for a long time. As you look back now, who and what events have played an important role in the development of biostatistics.

Chiang: There were quite a few people who played important roles in the growth of biostatistics from a nearly virgin field to a scientific discipline in the past 50 years. The credit must go to some of the universities, the federal government and the collective efforts of some biostatisticians and statisticians. Soon after World War II, people in several universities in the country recognized the need for biostatistics in scientific research. They established biostatistics departments inside the schools of public health. To attract talented young men and women to the field, the biostatistics departments provided financial assistance to students with training grants from the federal government, for a period of about 20 years. These departments used the established statistical methods in developing biostatistics courses in their curricula.

There were also activities in the federal government. The Division of Statistical Methods in the U.S. Public Health Service was established just before 1950 (I believe), and later became one of the best-known statistical groups outside the university setting. In 1960, the National Health Survey and the National Office of Vital Statistics were combined to become the National Center for Health Statistics. There was one issue of *Statistical Science* (Volume 12, Number 2, May 1997) devoted to interviews with the well-known biostatisticians at the NIH (National Institutes of Health). A personal note: with an NIH Fellowship I spent my 1958–59 sabbatical year in Washington, D.C., and delivered 10 weekly lectures in the then National Office of Vital Statistics.

One of the most important events in the development of biostatistics was the "Graduate Summer Session in Health Statistics." This program was created through the collective effort of the leading biostatisticians from three universities: including Felix Moore and Richard D. Remington from Michigan, Colin White from Yale and Bernard G. Greenberg from North Carolina. Possibly William G. Cochran from Harvard University also gave his advice. Due to their vision and effort, plans were made to have a series of summer sessions to be held in different universities throughout the country on a rotating basis, with the financial support from the federal government.

According to the plan, each host university would conduct six-week summer sessions in two consecutive years, each such session having eight or nine courses. Each course would carry three semester units recognized by the participating universities. And there were to be weekly seminars. Beginning with the first two summer sessions held in 1957 and 1958 at the University of Michigan, the program lasted for over 20 years. In addition to Michigan, the participating universities included Minnesota, North Carolina, Yale, Pittsburgh, Emory, Washington at Seattle, California at Berkeley, Texas at Houston, Vanderbilt, Harvard, Stanford and California at Los Angeles. I believe the influence of the summer program on the growth of biostatistics was tremendous.

The following brief description of the sessions held at the University of California will give you an impression of the summer program. We were informed more than one year in advance that the 1971–72 summer sessions would be held on the Berkeley campus. That gave us sufficient time to do our planning, to apply for a federal grant and to publicize our program. We invited the best statisticians and biostatisticians available to give courses in their specialties. In addition to health statistics and two levels of statistics, the courses included epidemiology, rates and proportions, population genetics, sampling methods, sequential clinical trials and nonparametric statistics. There were over 120 students in each session, coming from different parts of the U.S., and some from foreign countries. The California State Department of Health, located across the street from the campus, sent their employees to attend courses. The Dean of the Graduate Division, Dr. S. Elberg, opened the first session with a welcome speech. Professor Neyman, of course, was one of the seminar speakers. A trip to Yosemite was arranged by the staff for relaxation and recreation. After the final examinations, the faculty, students and staff got together in a Chinese restaurant to celebrate the ending of a successful summer session.

RETIREMENT YEARS

Li: Nowadays, you must enjoy your retirement life: gardening, photographing and traveling in addition to writing papers and books and helping other people.

Chiang: I am still teaching and giving lectures. I gave a course in stochastic processes at Tunghai University, Taiwan, in 1994. While in Taiwan, I gave seminar lectures at National Tsing Hua University, at the Academia Sinica–Institute of Statistical Science, at National Taiwan University and at other academic institutes. I gave a course at University of Texas, Houston, in 1995. While in Houston, I gave three seminar lectures, one of which resulted in publication of two papers jointly with my friends in Houston. I presented a paper and chaired a session at the 3rd ICSA International Statistical Conference, I gave a report at the 50th Session of the International Statistical Institute and gave two lectures at Peking University—all in Beijing in the early fall

FIG. 5. *Professor Chiang received the "Berkeley Citation" from Provost Doris Calloway at his retirement party April 24, 1987.*

of 1995. I also gave a lecture series on the theory of life table and competing risks in Barcelona, Spain, in the fall of 1996.

And of course I am still teaching at University of California, Berkeley. In order to prevent me from saying "No" to their invitation to be recalled to duty, the University gave me a new title: "Professor in the Graduate School," with an honorarium yet! I am thinking of buying a new computer with that money. So I do not yet have much leisure time.

Li: Do you have any hobbies?

Chiang: Photography is my hobby. Wherever I go, I have my camera with me. From the West Lake in Hangzhou to Notre Dame Cathedral in Paris, from Temple of Heaven in Beijing to Red Square in Moscow, to Yosemite in California, to the fjords in Norway and to Gibraltar at the southern tip of Spain, I take pictures. Now I have well over 60 albums. Statistically speaking, more than five percent of the pictures I took were good. I think that is significant.

Another hobby is playing "bridge." But some of my bridge partners are getting old, like myself, and some of them simply passed away. So I do not play bridge any more. I used to play "Go" when I was in China, and still like to play. But there are no "partners" or "adversaries" to play with. I am not much of a gardener. But I like to take pictures of the flowers.

I also played flute (xiao). I remember when I was a freshman at Tsing Hua University, I heard that one could get the best "xiao" in Yubin, in Guizhou province. But I did not know any one in Yubin. So I wrote a letter to the mayor of the city. After about a month, I received a pair of the most elegant "xiao" in the world, with the mayor's compliment! Apparently, the mayor was a very generous man. When he received a letter from a person named Chin Long Chiang from the National Tsing Hua University in Beijing, the mayor must have thought that I was someone of importance in the university. When the shop got the order from the mayor's office for a pair of "xiao," they gave the mayor the best. I wrote the mayor a gracious letter to express my appreciation.

A PIECE OF ADVICE

Li: What will be your advice for young statisticians?

Chiang: Mathematics! Study mathematics, take more courses in mathematics.

Li: Thank you very much, Professor Chiang.

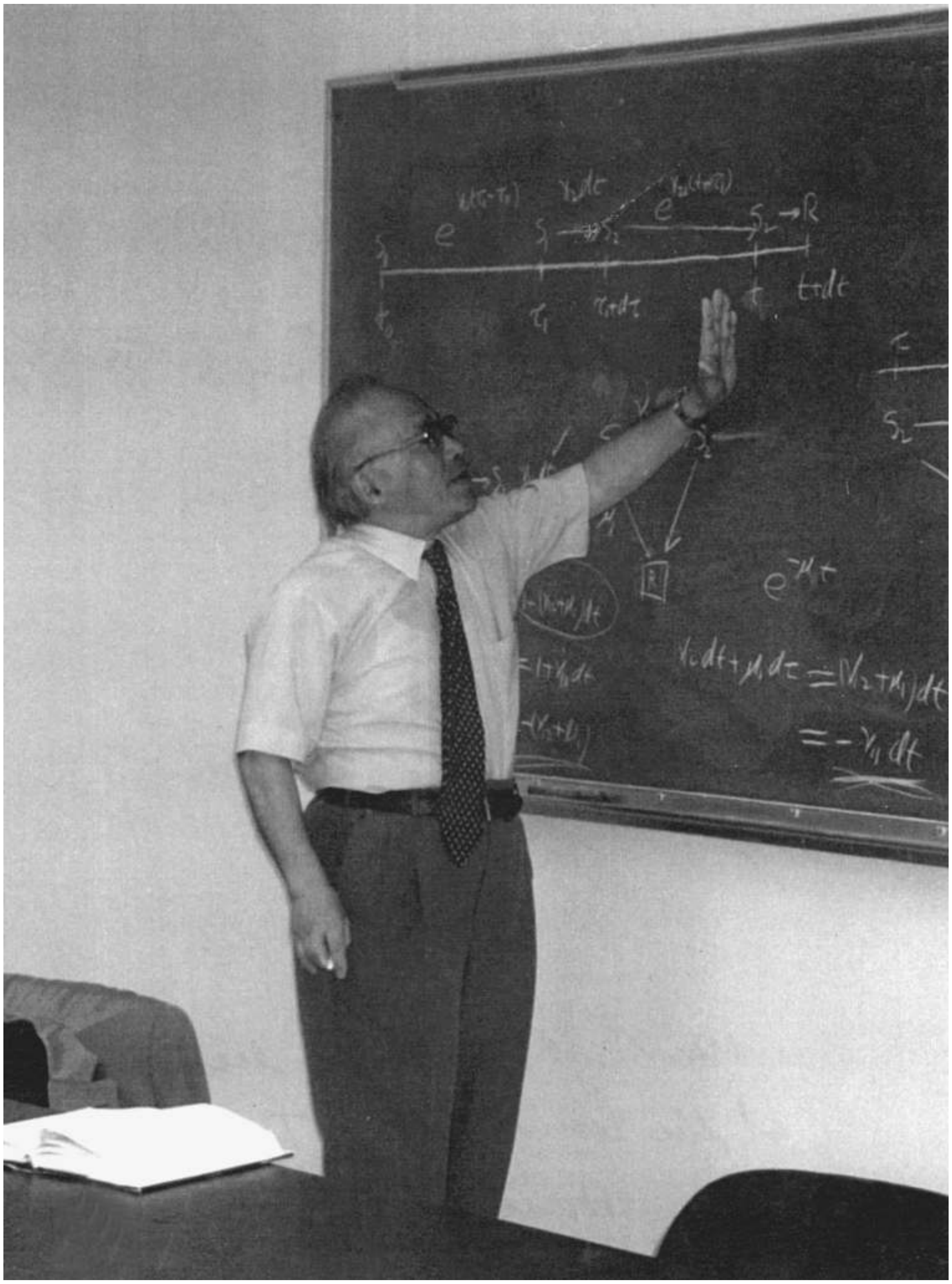

FIG. 6. *Professor Chiang giving a lecture at the University of Texas, Houston, March 1995.*

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