HOW I BECAME A PHILOSOPHER WITHOUT TRYING

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Dear President Inamori, Esteemed Members of The Inamori Foundation, Distinguished Guests, Dear Ladies and Gentleman.

May I begin by thanking all of you for coming here to listen to us, the Laureates of the Kyoto Prizes for 1992. You have come here to celebrate with us what undoubtedly must be, for all three of us, one of the greatest and happiest occasions of our life.

I understand that each of us is expected to give two lectures, of which today's lecture is the first. While the second lecture is supposed to tell something of interest to the specialists in our field of work, today's lecture, as I understand it, should give you an idea of our life and how it happened that we devoted our interests and our ambitions to the special field in which we did our main work.

Now I think that it will be best if I warn you from the very beginning that, in my particular case, this pattern for today's lecture does not fit at all, unfortunately. I am standing here before you as a philosopher; and I am known as a philosopher in Great Britain, and in other countries. And it is in the field of “philosophical thought of the 20th century” that I have been awarded in your country the great Kyoto Prize. But never during the long course of my life did I think: “I want to become a philosopher” or even “I want to study philosophy.” Nor did I ever in my life look at myself with satisfaction as a philosopher. If other people—even some professional philosophers—do classify me under this heading, and rank me as a philosopher, then this happens without my having planned it, or my having intended it.

Of course, at a certain stage in my life, I had to decide upon the profession I wanted to adopt; and after some wavering, I decided that I wanted to become a schoolteacher. At first I thought that I wanted to be a teacher in primary schools; later I wanted to be a teacher in secondary schools of mathematics, physics, chemistry, and biology. These were my aims; and with these aims in mind I left school prematurely in 1918 when I was 16 and became a so-called “extraordinary” student at the University of

Vienna in mathematics and physics. Three years later I became an “ordinary” student, and another seven years later, in 1928, I received both my doctor's degree in philosophy and my teacher's certificate in mathematics, physics, chemistry, and biology.

However, I wrote my doctor's dissertation in the field of psychology, under Karl Buehler, a child-psychologist. There were two reasons for this: first because I was interested in education, and therefore in psychology; and secondly because I had some ideas of my own this field—quite enough for a doctorate I thought. By contrast, I rated the fields of mathematics and of physics so highly that I thought I should never be able to have a really original idea of my own in these two marvellous and difficult fields. But I should have made these my fields of research, had I dared to do so, and had I not looked at myself as a future schoolteacher.

Perhaps I had better mention at once that some years later I did have some original ideas in both these fields: within mathematics, it was mainly in the axiomatization of probability theory, and within a field called lattice theory; for example, I gave the first proof of the theorem that if you have a semi (meet) lattice with a relative measure defined on it, then it becomes distributive upondefining the join—a theorem of interest in probability theory, in lattice theory, and in quantum physics. And in physics, I had a number of original ideas in the field of quantum mechanics. This was a new theory, invented by Werner Heisenberg just one year before I was awarded my doctorate. This invention of Heisenberg's was very important. But it seemed soon superseded by Erwin Schroedinger's invention of wave mechanics: it looked very different but led to almost equivalent results, and it was a more intuitive approach. I met both Heisenberg and Schroedinger in 1935, and I remained a friend of Schroedinger's until his death in 1961; and I am still a friend of his family.

Let me stop here for a moment. You will see that I worked in too many fields to gain the kind of specialised knowledge that a creative leader in any of these fields would need. But I had studied intensely for many years all these subjects without thinking for a moment about such leadership with regard to myself. I studied only because of the burning interest and excitement which these various subjects had aroused in me. And also because I hoped one day to infuse my pupils with this interest and excitement. I also never dreamt, before Hitler's rise, of a career as a university teacher. So from my point of view, I was not “wasting my powers by fragmenting and dissipating them over too many subject matters." But if I had contemplated a career as a discoverer, or as a research worker in any of these fields, or as a leader in the advance of science, then, of course, my way of working in perhaps ten different fields would have been sheer madness. However, I had no such ambition; not, at least, until very much later—until the publication of my book *The Logic of Scientific Discovery*, in German, towards the end of 1934. (It is of some interest that Hitler had become the leader of Germany one year earlier).

Since I am speaking about the fragmentation and dissipation of my powers in so many fields, I ought to add to all this still another confession. I come from a musical family. My grandparents on my mother's side had been co-founders of an organization in Vienna known as Gesellschaft der Musikfreunde. Around the middle of the 19th century, it was instrumental in the building of what remained for a long time the most famous of all the concert halls in the world, the Musikvereinssaal in Vienna, designed mainly with Beethoven's Symphonies in mind. And when I was 16 I began to compose, taking Johann Sebastian Bach as my ideal. Most of these compositions are lost, but one, a fugue for organ, had its first public performance in July of this year in the Escorial Palace near Madrid, Spain, about 70 years after it was written.

Now I must draw your attention again to the fact that I am standing here as a philosopher, and that of all my studies, I have not yet mentioned to you the study of philosophy!

I think that the first philosophic question which I asked was when I was 7 or 8 years old. I asked my father to explain to me the concept of infinity, and the infinity of space. I said that I found it impossible to conceive infinity, and that I wanted an explanation. My father advised me to ask one of his two brothers, and from him I received indeed an excellent explanation of what is technically called potential or Aristotelian infinity, in contrast to actual or Cantorian infinity. It was an explanation that satisfied me almost completely, although Cantorian infinity has left me somewhat puzzled until the present moment.

There were many philosophical problems to come. When I was 10, the problem "what is life" fascinated me and I proposed to myself and one of my school comrades that life is a process of oxidation like the flame of a candle - a solution first envisaged, as I found out later, by the early Greek philosopher Heraclitus of Ephesus. Philosophic problems were occupying me even before I started reading philosophical books, of which there were plenty in my father's library. Of his philosophical books the first I attempted, though quite unsuccessfully, was Kant's *Critique of Pure Reason*. I could not understand a word: I had no idea what problems it was about. But soon I learned that there was another book in my father's library that explained Kant's Critique—a book by Schopenhauer, called *The World as Will and Representation* (or "as Will and Idea"), and this, if I remember correctly, was the first philosophical book (at least the first very large philosophical book) that I read, and indeed, studied.

I found Schopenhauer difficult, yet I managed to learn a lot from him; and I went on to read both Schopenhauer and Kant. Once I could read Kant, I liked him better than Schopenhauer. But Kant left me with many unsolved problems that troubled me for many years. Yet the idea of becoming a philosopher myself -either in the sense of concentrating my reading mainly on such books as Kant's and Schopenhauer's, or in the sense of making philosophy my profession and writing such books myself—never occurred to me; and if it had occurred to me, I certainly should have rejected it. For although some philosophical problems interested me greatly, I did not think myself capable of solving any of them. A system of philosophy like Schopenhauer's appeared to me fascinating but also incredibly ambitious; and I did not believe that his metaphysical theories were true. Also, I wanted to do something useful, like teaching. Besides, I found problems in physics more attractive, and Darwinism more exciting and far more convincing. This explains why I never made up my mind to study philosophy, although I read a lot of philosophy. And I loved and admired best, apart from Immanuel Kant, one group of Greek philosophers: the so-called Pre-socratic philosophers, especially Heraclitus, Xenophanes and Parmenides. I also loved and admired Socrates, the Athenian citizen who was put to death by a jury of 500 of his Athenian fellow citizens. His defence speech, which his pupil Plato published under the name *The Apology of Socrates*, is the most beautiful philosophic work known to me.

Because I felt that it would be useful for me as a schoolteacher if I could master a craft like cabinet-making, I added that to all my other activities; and I actually obtained the official certificate of the Austrian State announcing that I was a trained cabinet-maker.

It was while working at a cabinet-maker's bench that I arrived at what I may describe as my first conscious solution of a philosophical problem. It was the problem of the origin of our Western system of classical music—tonality, harmony and counterpoint. I did not write it down, and I never talked about it to anyone; but 50 years later, in 1969, I described it briefly in a short chapter of an autobiographical book entitled *Unended Quest*, which was published some years later in 1974. It was also published in Japanese by Iwanami Shoten. To my great surprise, I was told this year, in May 1992, that my very old theory of 1919 or 1920 of the origin of Western music was almost identical with a theory published at the same time by the famous German sociologist Max Weber. I knew nothing about Weber in those days; and until this year I had never heard of his theory of the emergence of Western music. Even today I have not yet seen Max Weber's book which, I have been assured by an expert, is very similar to the relevant chapter in my autobiography.

I have told this story because it is, I am afraid, characteristic of my whole life. Neither my modest results in Greek scholarship (which led to new historical theories about Plato and the Pre-socratics) and in the sciences, nor those in philosophy (in epistemology and in the now so-called philosophy of science, or in social and political philosophy, such as in the theory of the origin of language and in the theory of democracy) were, as a rule, derived by that process which is usually described as "research." Instead, it was a process that could perhaps be described as follows. First, I become acquainted with a subject, maybe superficially to start with, then more deeply because I am fascinated by some problem. Then—in some cases soon, in other cases after some years, perhaps even after some new problems in other fields have begun to interest me more—a problem or an idea may crystallize itself and lead to intense work; that means, I begin to think intensely about the problem, I try to clarify it, simplify it, often in the light of a new idea, and the problem may considerably change its character. A tentative solution may turn up; and upon further intense work, this solution may considerably change, in interaction with the changing problem.

Sometimes all of this happens only in my mind, and the proposed solution is never written down; or it is written down 50 years later, as in the one case I described before. At other times, I may write down notes at various stages of the process, or I may draw up a diagram. Sometimes I reach my main result quickly, but I have never published any new result quickly. Quite often I do not publish my result at all, but only tell my friends or pupils about it. Or I send my result to a friend in a sequence of letters, for his criticism. And sometimes I forget my result for years—or, worst of all, for ever. You will, I hope, realise that I do not recommend this method of working. On the contrary, I wish to discourage all my listeners from adopting so hazardous a method.

Now that I have warned you that this is not a method that I should advise anyone to imitate, I confess that I personally lived very happily with it all my life. But my late wife, who worked with me and for me, and who wonderfully helped me in my work, suffered terribly from my working method. It is a method which cannot guarantee that hard work will yield results. I certainly have worked very hard and so did my wife. Take for example my two-volume work, *The Open Society and Its Enemies*. It has just been re-edited in a new and improved German translation, in order to celebrate my 90th birthday, and so I had a look through it. The two volumes, with some new appendices, amount to just over 1,000 pages; and they contain so much material, and so many thoughts and arguments, that I was surprised that I have ever been able to write them all down. But the fact is that I wrote the book 22 times, always trying to clarify and to simplify it, and my wife typed and re-typed the whole manuscript 5 times (on a decrepit old typewriter). It then took 2 years and 3 months before it was published in 1945. No, I cannot recommend this method. Nor could I repeat it.

At the time when my first published book came out in 1934 (its English title is *The Logic of Scientific Discovery*) I was a schoolteacher. When I had first written the book, the finished manuscript was more than twice as long as the one finally published in 1934, but the publisher told me exactly what size would be acceptable to him. I had developed the fundamental idea of this book in the winter of 1919-1920; that is, I had worked on the idea for 14-15 years; at first with not the slightest idea of publishing it. Its story is as follows.

In June and July 1919 I had become deeply critical of the theories of Karl Marx, and I had began to study them thoroughly in order to come to a rational judgement whether they were true or false. I considered it my duty to undertake this important study. I was only 17, and I of course never expected that anyone would listen to my findings, or be interested in whatever I might say about this subject. So I undertook the study merely to satisfy myself about the truth of a theory that I regarded as a grave danger for mankind. It took me 25 years until the publication of my book *The Open*

Society and Its Enemies, which contained the outcome of these investigations. And although this book has been in print steadily since it was published in 1945, it played only a small part in undermining Marxism and the Soviet Empire; a much smaller part than the famous books of my late friend Friederich von Hayek, for example, his book *The Road to Serfdom*. Hayek died this year in March.

Now very early in my critical studies of Marxism, during the winter of 1919-1920, I had arrived at some problems which, also after many years, led to the publication of three other of my books, in the following order: *The Logic of Scientific Discovery* was my first published book in 1934; and 25 years later, it appeared in an English translation. (It was not my first completed book: this remained unpublished from 1933 to 1979; that is, for 46 years.) *The Poverty of Historicism* was published next, by Friederich von Hayek, who then edited the *Journal Economica* as a series of articles in 1944 and 1945; and it was only 10 years later published as a book, first in an Italian translation, and later in English and then in many other languages; in Japan, 3 editions were published of this book in 1961, and 2 more in 1965 and 1966. In the 1960s, several other Japanese translations appeared. I am not so well informed about some later publication in Japan. All these three books were the late results of some of my early work in the winter of 1919-1920 when I was 17; all of them can be described as books on the theory of knowledge or epistemology or philosophy of science (although the second, called *The Poverty of Historicism*, was a book on the philosophy of history, and also of
historical science).

I shall now briefly explain how I came to write these books.

As I mentioned before, I had started to study Marxism in a critical spirit of finding out whether what Marxism asserted was true of false.

Now apart from other things, Marx, Engels, and Lenin asserted that the Marxist system was a science: that it had the status and the authority of natural science; and they had Newton's theory of gravity in mind. Now this assertion was of great importance in those days, many years before the modern attacks on the status and authority of the natural sciences. For in those days, the assertion that some theory had the status of science meant that it was true; and it meant even more: it meant that its truth could be demonstrated. In other words, science had in those days a tremendous prestige in the

West—a prestige like nothing else. And the claim that Marxism is a science—or let us say more clearly, a scientific theory—was therefore of great importance. For it meant, in those days, that Marxism was true, and beyond all criticism except, perhaps, by experts in this field or science.

I decided, in the Autumn of 1919, to investigate the claim that Marxism is a science, separately from the claim, whether or not it is true, that socialism or communism are, indeed, bound to come as the next historical epoch or world period (which I, by that time, had begun to disbelieve, since the Marxian arguments seemed to me highly Questionable).

So I decided to work, first of all, on the following problem: is it true or is it false that Marxism is a science, like Newton's gravitational theory (which I greatly admired)? And this problem pleased me, for I looked at myself as a future teacher of physics, and any physics teacher, I felt, ought to know what are the criteria that give physics, or chemistry, the status of a science—or still better, what makes any allegedly natural science a genuine science. Or in very different words: why do I respect astronomy but despise astrology? So I was very pleased to have been able to replace a problem about Marxism (which I disliked) by a more general problem that included physics (which I loved—especially Newton's cosmology).

It was this problem that made of me a philosopher of science, without any intention on my part of becoming a philosopher of science.

I was only 17, and although I was a member of the Mathematical Institute of the University of Vienna, I had no idea of how to tackle a problem like this. So I began just by thinking about it. I was sure that this must be an old problem, and that all the great professors at the university—at any rate all the physicists - must be familiar with it, and must know the solution. But in the mathematical seminar, only utterly different problems were discussed, and there was no opportunity to raise a problem like mine. And when I tried to mention it to some of my fellow students, they were (with one exception) not interested.

I first started with the following attempted characterization: An assertion or a statement belongs to science if it can be proved; or, what amounts to the same, if it is a demonstrably true proposition. However, I knew even before the start, that this characterization must be unsatisfactory and must be replaced by something better. For I knew that in geometry, for example, we have the famous axioms and postulates and definitions of Euclid. (In those days, postulates and axioms were rarely distinguished any longer.) And these are characterized as being not demonstrable, although they clearly belong to geometry—which is very much a science! Moreover, they are the very foundations of geometry: from them all other propositions of geometry are derived, as theorems, from axioms and postulates and definitions.

I soon found that, in order merely to clarify my problem and before even beginning to work on a solution, I had to distinguish between mathematics and so-called empirical or natural sciences, like physics, chemistry, biology, geology and also geography.

So I tried to study, in the Institute of Mathematics, what was called there “axiomatics,” that is, the general theory of axiomatic systems; of these the greatest expert was David Hilbert. And when, 10 years later, I had to write a mathematical thesis for my teacher's examination, I chose to write it on the subject of axiomatics.

I soon arrived at the distinction between a formal system on the one hand and, on the other, a theory whose purpose is to refer to reality—or, more clearly, to describe or explain something real, such as do Newton's or Einstein's theories of gravitation.

So I was first led to a comparison between pure mathematics and the theory of gravitation, and then between Newton's theory and Einstein's theory. I studied intensely these theories and the claims made for them by different physicists. I conducted all these studies merely because of my burning interest in the problems, and without being in the least aware that here or there I was also breaking new ground.

At the time, I had no prospects whatsoever of obtaining a teaching position, not even in a primary school. I had been too young by a year to fight as a soldier of the Imperial Austrian Army in the First World War; and all the available positions for teachers were, quite properly, reserved for the soldiers that came back from the war, or from the prison camps.

But I obtained work—at first unpaid—in institutions for children, mostly for neglected children. Later, I was lucky enough to have the opportunity of giving private lessons on quite different subjects, varying from mathematics to psychology and philosophy, to all kinds of students at the University of Vienna. Among them were a few American students; and since in those days the value of the U.S. dollar was high and that of the Austrian currency was very low, this from of teaching was quite satisfactory for both parties. And I had excellent opportunities to learn how to teach (and also, how not to teach).

This kind of life was by no means exceptional in Vienna at that time. On the contrary, the economic situation made it not infrequent. And when, in 1923, the City of Vienna announced that it would in future employ, even in its primary schools, only teachers who had passed successfully through its newly founded Pedagogic Institute, I and others like myself applied for permission to become members of this new Institute, which was affiliated to the University of Vienna.

In those days, students in Vienna had to finance themselves. But at least we did not have to pay any fees to the Institute; and we had a good prospect of getting a (badly paid) teaching job at the end.

I was a student at the Pedagogic Institute for two most interesting years; we were taught both at the newly prepared institute and at the university; and as some of my fellow students at the institute found the going very hard, it so happened that I myself became, unofficially and unknown to the director of the institute, a teacher at the institute—obviously, an unpaid teacher—giving courses to my fellow students. At times when officially the classrooms were empty, I began to give lectures and seminars to students who had difficulties in following some of the lectures at the university, which they, as members of the institute, were bound to attend. As we had lots of intermediate examinations (called “Colloquia”) to pass at the university, my courses became special preparations for these university examinations. Only one of our university teachers, the psychologist Professor Karl Buehler, knew about my teaching activities, since I had to ask his permission to use his laboratory for my teaching purposes; and he later said in a letter to me that the group of students I had prepared were the best he had ever examined.

Among my unofficial courses at the Pedagogic Institute were courses in the Latin language for those who had not had Latin at their secondary schools; for the university in those days demanded from all students a certain, though not very high, proficiency in this language. This language teaching to single adult students taught me much about human language; it was an experience essential for the views on language which much later became part of my (very sketchy) theory on the origin of human language, which even now is still unpublished.

After leaving the Pedagogic Institute it took me another 5years before I was appointed, first as a primary school teacher and, after another year, as a secondary school teacher. During this period I wrote many papers—they filled a whole wardrobe by 1930—on subjects that today would be said to belong to the philosophy of science. None of these papers of mine were ever submitted for
publication.

In 1930, shortly before starting as a primary school teacher, I met professor Herbert Feigl, an ex-Austrian of the same age as myself, who was now professor of philosophy in the United States, and a member of the so-called Vienna Circle of philosophers. After listening to my theories for a night, he said that I should write them down in a book. And so I stopped writing papers and started writing a book, which led to the publication of my *The Logic of Scientific Discovery* in the autumn of 1934.

This book contained a theory of scientific knowledge and its growth, a theory of probability, which I later much improved, and a critical interpretation of quantum mechanics. (Of this, several important points have been rediscovered since by others.) My book was an immediate success. Of course, this success was confined to the small circle of those who were, in spite of Hitler's dictatorship in Germany, his devastation of the German universities, and the threat of war, still capable of thinking about such abstract notions as those treated in my book.

In spite of all this, I received excellent reviews not only from the main European countries, but even from America, and very soon invitations to lecture came from several Polish, English and even German universities; at the same time I also received intimidating threats from some of the national-socialist teachers of the school at which I was then teaching, and also from my very powerful school inspector.

So I decided to accept the lecturing invitations from England, and to try to emigrate to England. The lectures in England were also a success and through an old friend from Vienna, B. P. Wiesner, I met a philosophical biologist, Professor J.H. Woodger, the founder and leader of the Theoretical Biology Club, to which some famous biologists belonged, including some Nobel Prize Laureates. Woodger became the first biologist who consciously tried to apply the methodology I had advocated in my book. He advised me in 1936 to try for a post in the British Empire; and he showed me an advertisement for a professorship and for a lectureship at the Canterbury University College in Christchurch, New Zealand. I applied for both of them.

On Christmas Eve of 1936 I received in Vienna a telegram from New Zealand, in which I was offered the lectureship. I accepted, and my wife and I left Vienna two months later, first for London, and soon on to New Zealand. There existed no air traffic yet, and we spent five very interesting weeks at sea before we arrived. When we arrived, I took up my appointment as a lecturer in philosophy at Canterbury University College, then a part of the University of New Zealand. It is now the University of Canterbury.

So I had evolved from a schoolteacher to a professional philosopher, teaching in a university, without having ever chosen philosophy as my subject of study - in fact, without ever having tried to become a philosopher. Admittedly, guided by Professor Woodger, I had made an application for both the professorship and the lectureship in Christchurch. But I was then trying for a position, not trying to become a philosopher. For I had previously shown my competence as a philosopher through the success of my first book, and of my lectures.

So how did it happen? The answer is: Although I had never decided to study philosophy, the problems I had taken up as my own had forced me to study many things, and philosophy was among them. So I must say that I owe everything to my beloved problems. I really fell in love with my first problem: how to find a criterion of the empirical-scientific character of a theory. And after I obtained a solution, I fell in love with my various other problems, among them historical problems about ancient Greece—from Homer and Xenophanes and Parmenides and Plato to modern times: to Kant, Hegel and Marx, and to Khrushchev and Gorbachev.

I should certainly not encourage anyone to take my own ways of studying as an example for his own; rather, I should warn them against it. But I can recommend every serious student, and especially every serious science student, to look out for a beautiful problem that he can really love, and to which he is prepared to dedicate his life. This attitude will make it easy for him to try again to find a solution, and to be critical of his own efforts which, in most cases, will have to be often renewed before they can be successful; and even if they appear to be successful, they should be most seriously questioned by himself, for they will usually be open to improvement. Einstein somewhere tells us that through the years from 1905 to 1915, when he was trying to generalize his so-called theory of relativity (which thereby became a geometrical theory of gravitational forces), he rejected through all these years about every few minutes a hopeful idea for a solution. And my friend and former pupil Guenter Waechtershaeuser, whose marvellous theoretical work I have been happy to have been allowed to follow closely for the last 10 years, has been constantly working to improve critically every single of the countless theoretical ideas that have gone into his work—a method that has led him to an unprecedented harvest of new results in the field of evolutionary biochemistry. So the constant consciousness of our fallibility, the constant criticism of ourselves combined with unlimited devotion to our main problem and its many problems—children and other subsidiary problems—this is what I can recommend to you with full conviction, from the bottom of my heart.

So I wish to end with the advice: however happy you may be with a solution, never think of it as final: There are great solutions, but a final solution does not exist. All our solutions are fallible.

This principle has often been mistaken for a form of relativism; but it is the very opposite of relativism. We seek for truth, and truth is absolute and objective, and so is falsity. But every solution of a problem opens the way to a still deeper problem.

May my advice be a signpost on your way to a creative and happy life!

Ladies and Gentleman, I thank you for your attention and for your patience.