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A CAT HAS NINE LIVES

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YOUTH

When I was 13 years old I knew that I was going to be a botanist. I lived at that time in Berlin with my sister and brother-in-law, the poet Gerson Stern. One day he presented me with a book by R. H. Francé, *Die Welt der Pflanze: Eine populare Botanik* (8). The book hit me like a ton of bricks. I had studied in a humanistic high school, "Humanistisches Gymnasium," a school in which at that time we were taught mainly Latin, Greek, and with the exception of German, no modern language and no biology. The book, therefore, opened up a new world before me.

Up to that time, a plant was for me simply a "thing," and now it was revealed to be a complex entity provided with a great number of astonishing structures and mechanisms which were all "*zweckmaessig,*" i.e. adaptations enabling the plant to exist in very different environments. Two of the book's chapters aroused my particular curiosity. One was called "Invisible technicians," dealing with what we know since Haberlandt's classical book (8a) as physiological plant anatomy. Another was on seeds, their dispersal and their germination. Francé called the chapter "The plant as mother." In Metz, where I was born as Walter Schwarz and where I lived until my thirteenth year, we had a garden that was cared for by our factotum Johann, who every year had sown some flower seeds. I had never paid any attention to this. The idea that a plant could be a mother with children, as described so romantically by Francé, therefore seemed to me not fully believable. I started to germinate some bean seeds in pots and was deeply awed when the hypocotyl of the seedlings broke out of the soil. I confess that even today germinating seeds still affect me the same way. At that time I could not analyze my feelings, but I now know what struck me then. My awe apparently emerged from my collective subconscious (J. C. Jung). It was the same spiritual experience which made germination for many ancient civilizations and religions the symbol of death and resurrection and of the yearly recurring rebirth of nature in spring after its "death" in winter.

Shortly after I had received Francé's book, my brother-in-law presented me with a children's microscope. It looked like one of the first microscopes invented. It had a substage mirror but no condenser, the three objectives had to be screwed on since it had no revolving nosepiece, and the focusing had to be done by elevating or lowering the tube by hand. I immediately collected water from any puddle or pond I could find. Observing my first *Euglena* flagellating through the microscope's field of vision and my first *Volvox* majestically rotating was a revelation. My reaction was twofold. My curiosity to see (and know) more was awakened, and at the same time I was overwhelmed by the elegance and pure aesthetic beauty of these organisms.

In reviewing my scientific life now at the age of 80, I see that this primeval reaction to nature at age 13 has remained the mainspring of my scientific work—continuously stimulated by curiosity and, perhaps even more important, wonder, admiration, and love for the beauty of nature and its creatures. From then on animals and plants were for me far more than objects of study. In trying to find out what made these creatures tick I was bound to them by the common bond of life, a feeling which elevated them from study objects to brother living beings. I am convinced that most good biologists, even if they claim objective detachment, react, mostly unconsciously, in a similar way to their objects of research.

While I was still in my thirteenth year, my brother-in-law gave me another booster shot when he presented me with *Die Lebensgeheimmiss der Pflanze* (*The Secrets of Plant Life*) by Adolph Wagner (26). The title page of the book depicted two growing bean plants, one climbing up a pole, and the other, having climbed a broken pole, now reaching from its pole to that of the other bean plant. When writing this paper, I thought of this book, which I had not seen for more than 50 years. I had forgotten its title and only remembered the author's name Wagner, the fact that the book had been published before World

War I, and most of all, the title page picture. I asked friends in Germany to find the book, and they located it on the basis of my description of the climbing bean plants. I think the picture imprinted itself so deeply on my mind because, for the first time, I became aware that a plant has sensors enabling it to orient itself in space according to its needs. When I first received the book, I read it in a few days, and grew a bean plant in a pot to see if Wagner told the truth. This was my introduction to plant physiology.

Having devoured France's and Wagner's books and looked down the microscope, my fate to become a botanist was sealed. From then on I called myself (naturally only secretly in my mind) in good Latin style, "discipulus scientiae amabilis."

As stated earlier, I was born in Alsace-Lorraine, the two border provinces between France and Germany. After the Prussian-French war of 1870–71, the Germans had annexed both. In 1918, the provinces became French again, and in 1940 returned to the Germans, only to become French yet again in 1945. In contrast to Alsace where the people spoke a German dialect similar to the "Schwytzer Deutsch" of the Swiss, the language of the indigenous population of Lorraine was French. Since in Metz after the occupation of 1871 German was the official but French the unofficial language, I was bilingual from early youth. Everybody spoke at least some German, often intermixed with French. You could, for instance, hear a sentence like this one: "Maman, komm à la fenetre. Jean ne croit pas dass Du schielst" ("Mom come to the window. Jean does not believe that you are crosseyed").

My bilinguality as well as my knowledge of Latin and Greek helped me later in life to acquire other languages such as Hebrew, Spanish, Italian, English, and Russian with comparative ease.

It is most regrettable that today in science we do not have one common language, in the way that Latin was the language of scholars and scientists up to the eighteenth century. Since a return to Latin is most unlikely, a modern scientist should be able to at least *read* English, French, and Russian. If, as in most cases, he cannot, the results are sometimes bizarre.

For instance, a modern textbook of ecology cites 1695 papers written in English, 15 written in German, 7 written in Russian, 3 written in French, and one paper written in Dutch. Students will certainly get a lopsided view of ecology because they will believe that nearly all the important papers in ecology were written in English. Lack of knowledge of literature written in foreign languages also leads to objective mistakes. The same textbook can serve as an example. In it the term "allelopathy" is ascribed to Muller (15), whereas the phenomenon was first described and the term first coined by Molisch in 1937 (14).

The Germans introduced into Alsace-Lorraine a school system modeled along that which was prevalent in Prussia. The school had 12 grades numbered in Latin, as befitting a humanistic gymnasium that required seven years of Latin. One started at age five or six in the "nona" (ninth grade), went through Octova, Septima, etc, and finished in upper Prima with the "Abitur" which entitled one to enter a university.

Up to the beginning of World War I, school discipline was very strict. The teachers were tyrants, and we were often punished for small misdeeds with hard strokes of a cane on the inner hand surface. This changed completely after the beginning of the war. The young teachers joined the army and were replaced by old retired ones who could not master the unruly crowd. We learned very little, especially since we spent much time in the large underground air shelters of our school, a former French monastery.

Metz at the time of World War I was a strange place. The city was surrounded by a double ring of fortifications kept by the German army, and the front line was only about 20 km away at Pont à Mousson. From my window I could see the cannon fire there and hear the continuous rumbling of the guns at Verdun, about 50 km west of Metz. In the first days of the war I had my first taste of aerial warfare. French planes attacked Metz and used for the first (and I think the last) time, *Fliegerpfeile* ("aviator's arrows"), aerodynamically constructed metal arrows which the pilots released in bundles. One pierced the visor of the peaked cap of the gatekeeper of my parent's department store. We children fought for the possession of such a rare souvenir. Later French planes often bombarded the city, once hitting an ammunition train. The explosion kept us for hours in our cellar, where we sat huddled together, frightened to death.

The combination of inefficient teachers and the atmosphere of war not far off had a strong effect on me. I stopped doing my homework and spent most of my time playing trench warfare with a band of other rowdies in unused trenches. The result was that in one year I went down the ladder from the first to the last place in my class. The patience of my parents broke, and they sent me off to my sister and brother-in-law in Berlin.

In Metz I had already had my first experience with anti-Semitism and with Zionism. When I was seven, coming home from school, some children called me "*sale juif.*" I reacted promptly by jumping on them and a fight ensued.

Another anti-Semitic event made an even more profound impression upon me. My elder sister and my brother, who fell as a German soldier in World War I, belonged to the "Wandervogel." This German youth movement decided in 1916 to expel its Jewish members. This brought home to us that the social environment to which we thought we belonged apparently didn't want us. At about the same time, Jewish soldiers serving in the nearby German frontline visited us during the Jewish holidays, speaking to us about Herzl (the founder of modern Zionism) and the Zionist aim to build a Jewish homeland in Palestine. After my experience with anti-Semitism, the idea appealed to me, and I became, and still am, a Zionist.

When my parents moved in 1920 from Metz to Frankfurt/Main, I joined them. During the two and a half years of high school in Frankfurt I dedicated most of my time to the Zionist youth movement which was the Jewish-Zionist counterpart of the Wandervogel.

STUDENT DAYS

I entered Frankfurt's Johann Wolfgang Goethe University in 1923. I naturally took botany as my main subject but told my parents that I was studying chemistry (one of my secondary subjects) because at that time it was unthinkable that a good Jewish boy would take up such an outlandish profession as botany. I continued this pretense until the day I received my PhD.

Three years after I had received my PhD, my parents were still so worried about my professional future that my mother once visited Moebius, my professor of botany, and asked him: "Herr Geheimrat, has my son as a Jew really a chance as a botanist at a German university?" Moebius, knowing that at the time I was working on the physiological anatomy of the fruit stalks of heavy fruits, gave her a metaphoric answer citing a German proverb, "Es sind die schlechtesten Fruechte nicht, an denen die Wespen nagen," meaning, I was a good "fruit" and that she should not worry about my future.

My professor of zoology was Otto zur Strassen. He and Moebius were both heads of their respective departments and, as usual at that time in Germany, absolute bosses. Both carried the splendid title of "Geheimrat" (Privy Councillor) and had to be addressed as "Herr Geheimrat" even after years of professional contact. To call the professor by his personal name as is usual in American universities was unthinkable.

Moebius was a gentle man who treated his students like a benevolent father and took an interest in the personal life of each of them. He had an unusually broad and humanistic cultural background. During the Nazi period he also displayed unusual personal courage. In 1935 he dared to write in a letter to the mayor of Frankfurt: ". . . Presumably you have seen . . . the giant antisemetic placards on the fence of the Gontard house. We Frankfurters should be ashamed . . . that here Jew baiting is done in such a hateful and disgusting way while Frankfurt especially should be grateful to its Jewish citizens for a multitude of endowments . . . I remind you that Frankfurt would never have been a university town if Jews in particular had not donated the necessary means . . ." Somebody leaked the letter to the "Sturmer," the notorious anti-Semetic hate journal edited by Julius Streicher, which published it. As a consequence, Moebius's successor as head of the Botany department banned him from ever entering again the department which he had founded.

The botanical fields in which Moebius was most active were taxonomy, developmental and physiological anatomy, nature and origin of plant colors

(12), and the history of botany (13). I am proud to say that in his book (13) he twice mentions my own work. His interests stimulated my own. The elements of taxonomy that I learned from him came in very handy later in Palestine, when I worked in Aaron Aeronsohn's herbarium. The effect of environmental factors on anatomical structures had interested me throughout my botanical career. I was also much taken by Moebius's occupation with the history of botany. In his lectures he always tried to trace the origin of concepts or terms back to the men who had first conceived them. In my own lectures, I have always tried to follow Moebius in this regard because I feel that educationally it is important that students know that, with few exceptions, we in science are "standing on the shoulders of giants."

Moebius was not a good lecturer. We used to say that listening to him was like taking a sleeping pill we called "Moebiol." In the lab he was an excellent teacher. He had prepared a hand-written manual in which each consecutively numbered paragraph described what we had to do. We called it the "Fahrplan" (railway time table). We proceeded from number to number in our own time. I spent every free minute in the laboratory which was always open to us. I often worked there until the early morning hours. Moebius visited us every day and discussed our work with us, sometimes for hours. If he found that we were much interested in a certain matter, he encouraged us to enlarge upon it and to do some additional work. This very personal way of teaching was possible because only about eight students participated in the lab. It was ideal because one was completely free from any time limit and constraint. When he noticed that my special interest was in physiological anatomy, he permitted me to deviate from the "time table" and to do some special work.

In the department's botanical garden, I had observed that in the mature petioles of *Heracleum pubescens,* the central vascular bundles were arranged in a wave-like pattern whereas they were straight in the young petioles. When I showed this to Moebius he said, "Why don't you investigate the reason?" And so it came about that my first paper was published in 1926 (18).

From Moebius I received my basic training in general botany and from Fritz Overbeck, then the one and only assistant in the department, the basis of plant physiology. In his course I did my first scientific experiments on germination, a theme which later in my scientific career became very important to me. Under his influence I also carried out my first physiological investigations, one on the so-called "mitrogenetic rays" of Gurwitsch, which proved to be nonexistent (20); the other one on the etiology of variegation in *Coleus* (21). The phenomenon of variegation occupied my interest for many years.

Geheimrat zur Strassen, the zoologist, was a type very different from Moebius, an extrovert, aristocrat, and a brilliant lecturer. Not only students attended his lectures but half the intellectual elite of Frankfurt, especially women attracted by this impeccably dressed, charming man. I, as did all the others, listened spellbound to his lectures on evolution of the animals, where, according to him, every step was well known and documented. Only after the lectures, I asked myself if everything was really so easily explainable. I still have some doubts today about the *mechanism* of evolution, as proposed by Neo-Darwinism.

In zur Strassen's seminars we had to lecture on a zoological paper he had supplied. He then not only criticized what we said but also *how* we said it. We should never wander around the podium, but stand still facing the audience. We should never read from a prepared text, nor talk when writing on the blackboard with our backs turned to the audience. And above all, we should prepare our lectures carefully and, before giving them, practice them and time them. When listening today to some of my colleagues, I pity them that they did not have such teachers!

After four years at the University, I graduated with a PhD (19) at the age of 22, having passed an oral examination in botany, zoology, physics, and chemistry. In contrast to today's praxis, this was the only examination I had to go through during the four years of my university studies. I am sorry for the students of today who are plagued by so many consecutive examinations which, I suspect, obstruct the learning process more than they promote it.

My PhD examination was more a friendly conversation than an examination. Moebius and zur Strassen asked what I knew about the evolutionary process of animals and plants. When they saw that I had my own slightly unconventional opinions about the mechanism of the evolutionary process, they discussed these with me for over an hour.

ASSISTANT IN EUROPE

A few days after receiving my PhD, I married and worked for two terms as an assistant to Moebius. Then Ernst Pringsheim offered me a similar position in the department of plant physiology of the German University in Prague. I accepted and stayed there from 1927–1930. Prague at that time had two universities, one Czech and one German, and therefore also two departments of botany. There was practically no contact between them, an evil omen for things to come some ten years later after the takeover of Czechoslovakia by Hitler.

In Prague I worked on two problems, variegation and the physiological anatomy of the fruit stalks of heavy fruits. I found that mechanically the fruit stalks were much overdimensioned, and that with much less mechanical tissue there still would be no danger of the fruits falling off the tree. My investigation taught me to be very careful in relating structures to functions. Often certain structures seem to fulfill a certain function, yet in reality are only a necessary functionally neutral by-product of physiological events that are not directly related to the observed structure.

In my work on variegation I was lucky to find in the botanical garden a form of *Selaginella* in which the variegation was temperature dependent. In the white parts of the variegated leaves the originally normal plastids degenerate until they disappear completely. I found stomata in which one guard cell contained a normal green plastid whereas the plastid in the second guard cell degenerated. Both plastids derived from the apparently normal plastid of the stoma-mother cell. Here I was confronted with a basic problem of all developmental processes: physiologically unequal cell division.

My stay in Prague widened my botanical horizons considerably. I attended many of the lectures of the four professors and their assistants. In Prague I also became interested in floristics and plant sociology.

The three years I spent in Prague belong to my most pleasant memories. Prague was at that time, when Thomas Masaryk was the president, the cultural center of central Europe, abounding with writers like Franz Kafka, Karel Čapek, Jaroslav Hačeк (the good soldier Schweik), Franz Werfel, Stephan Zweig, Ernst Brod, and many famous musicians. I enjoyed the best of two cultures, the German and the Czech slavic. I decided that an educated person should know at least one slavic language and started to learn Russian. This opened for me a new cultural horizon.

While I was in Prague, an event occurred that was of the greatest importance for my future life. In 1930 Heinz Oppenheimer came to Prague in order to work in Pringsheim's department. After finishing his PhD under Molisch, he had immigrated to Palestine where he worked in the herbarium of Aaron Aaronsohn, the man who in 1906 had found one of the wild ancestors of cultivated wheat *(Triticum dicoccoides)* in Galilee, and over the years had collected a large herbarium of plants from Palestine, Syria, and Jordan, containing a number of new species. After his untimely death the family asked Oppenheimer to classify and publish the new species and a list of all the plants collected, as well as Aaronsohn's very interesting travel journals. At that time Oppenheimer had already published the *Florula Transjordanica* (16). In Prague we lived in the same house and became friends. After some time he asked me if I would be willing to come to Palestine and work with him on editing the *Florula Cisjordanica*. I promised him that I would seriously consider his offer. In the meantime, I was offered an assistantship in the botany department of the Technical University in Darmstadt. I accepted because the job offered the possibility of becoming a lecturer.

My first boss in Darmstadt was Friedrich Oehlkers. He was highly intelligent and had a special interest in modern philosophy. He was also a difficult personality and most excitable. Once when I disturbed him, he threw a chair at me, which for a professor was quite an extraordinary thing to do.

In Darmstadt I continued my work on the physiology of variegation (22). In the youngest cells of apical shoot mainstems I observed in vivo mitochondria and proplastids and found that mitochondria and plastids are physiologically and developmentally completely independent. This was an important new statement, because some authors then doubted the existence of mitochondria and others believed that plastids developed from mitochondria.

At the end of 1931, Oehlkers left Darmstadt and Bruno Huber became head of the department. The cool aloofness of Oehlkers toward his subordinates was replaced by the warm, affectionate, and friendly attitude of Huber. He encouraged me to write the thesis needed to become a lecturer, and in 1933 I gave my probation lecture before the faculty. He did all this, knowing that I was a Jew and knowing that the new Nazi regime was fanatically anti-Semitic. He later had considerable trouble with the regime because of what he had done for me.

In the department I was responsible for the laboratory exercises and the floristic excursions of the students of pharmacology and of the future biology teachers. When I met them for the first time I noticed that all of them carried the badge of the national socialist party. I immediately made my position clear, telling them that I was a Jew and a Zionist. From this point on an interesting relationship developed between us. The students knew that as a Zionist I wanted to go to Palestine. This made sense to them. I, on my part, was curious to know what attracted them so forcefully to Nazism. It came out that they strongly believed in the *socialist* part of the national-socialist movement. Hitler at that time had a strong competitor for leadership in the party, Gregor Strasser, who proclaimed socialism as one of the main aims of his party. He based his "socialism" on the ideas of Gottfried Feder, who in 1919 had published a booklet entitled, "The breaking of the bonds of interest slavery" (Die Brechung der Zinsknechtschaft). In this booklet he proposed a noncapitalistic social system in which everybody could borrow money without paying interest. This impressed my students very much. I had read Feder's pamphlet and tried to explain to them that the whole idea was an impossible bluff. I still hear myself telling them that Hitler used their naive idealism for his very different political purposes and that they were dupes. How right I was they must have seen later when, after he came to power, Hitler immediately got rid of Strasser, and Feder and his whole "socialist" program just vanished. Looking back after so many years I am still astonished that we could discuss those explosive matters freely and still remain on good personal terms.

Hitler's book, the discussions with my students, and the opinions expressed by the man in the street convinced me that Hitler would soon come to power, sealing the fate of the Jews. I had kept up my contact with Oppenheimer, and in October 1932, I signed a contract with the Aaronsohn family, which stipulated that I should come to Palestine in October 1933.

My nearly two years with Huber were, professionally speaking, quite productive. I had observed earlier that the leaves of a variegated form of *Coleus* rooted easily without forming buds. Their petioles thicken considerably and the white parts of the variegated leaves become green. I found that leaves of many other species can be induced to root in the same way as *Coleus*. The changes that take place when the petioles form a cambium and anatomically turn into stems, and their possible physiological reasons, became the theme of my *Habilitationsschrift* (23).

Under the influence of Huber I began to be interested in ecophysiology. Huber was the first to use a fast-weighing torsion balance for measuring transpiration in the field, and he also constructed a cumbersome instrument for the field measurement of photosynthesis. He turned my attention to Maximov's book on water relations (11), subtitled "A Study of the Physiological Basis of Drought Resistance," a topic which keeps me busy to this day. When in 1932 I told Huber of my intention to leave for Palestine, he encouraged me to take with me a torsion balance and his photosynthetic apparatus and to start ecophysiological work in the desert there. He also gave me the books by Volkens (25) and Stocker (24) on the physiological anatomy and water balance of desert plants. To this day, both are for me a kind of desert bible.

Otto Stocker, who much later became my personal friend, was the first ecophysiologist to work in the desert of the old world. I was thrilled by his book and decided to walk in his footsteps.

On April 1, 1933, the day Hitler declared a boycott against the Jews, the university sacked me. In the morning, the rector of the university summoned me. When I entered his office he said, after some mumbling and stuttering, "Herr Doktor, you were denounced to me as a confirmed Jew *(bewusster Jude)* and I should dismiss you on the spot. But I am personally willing to give you four weeks to leave the university." I had expected something of that kind and answered rather brashly, "Your magnificence (this was the official title of a rector), in times to come you may remember this day as the beginning of the downfall of Germany. As to the four week's grace that you want to give me—you can keep it. I am leaving this afternoon." With these words I stood up, turned my back on him, and walked out banging the door. I immediately sent a cable to the Aaronsohns telling them to expect me in Palestine in April instead of October. They agreed.

In the afternoon of that memorable day I packed my things in the department. Interestingly enough, my Nazi students told me how sorry they were at my leaving and then helped me to pack. The father of one, the owner of a transport company, moved all of my meager goods to Frankfurt and refused to be paid for his service. We left all of our furniture in our apartment, inviting everybody to take what they wanted. Three weeks later we arrived in Haifa. Thus began a new life.

PALESTINE

Oppenheimer and the Aaronsohns received us with open arms. They had prepared a flat in Jerusalem for us, and for the next four years I went every month for a few days to Zikhron Yaakov to work in the herbarium, returning with a bundle of herbarium species which I could not properly identify in Zikhron. In Jerusalem I could compare them with specimens in the herbarium of the department of the Hebrew University. I always tried to find the species that I had identified alive in nature and collected my own herbarium. By the time this taxonomic work ended and Oppenheimer and I had written the manuscript of our book, *The Florula Cisjordanica* (17), I had acquired a good working knowledge of the flora of Palestine, Syria, and Lebanon. This helped me greatly in my ecophysiological field work.

I think it would be good for science if all physiologists would have some knowledge of the flora surrounding them. This is very often not the case, and it shocked me when I heard a famous plant physiologist say that he was proud of not knowing the difference between a rose and a carnation.

A few days after our arrival in Jerusalem, Heinz Oppenheimer, who was the first head of the plant physiology and anatomy section in the botany department of the Hebrew University, brought me in contact with Alexander Eig, the head of the department. In 1933 the Hebrew University was still very young. It was officially opened in 1925 as a research institution and was only opened to students in 1928. Otto Warburg was its first professor of botany, followed by Eig, a taxonomist, phytosociologist, and phytogeographer. I became Heinz's successor when he left for the Agricultural Research Station in Rehovot, and gave my first lectures in plant physiology and anatomy in 1934.

In the meantime, much had happened to me. A few days after our arrival in Jerusalem I met, through Oppenheimer, Richard Richter. He was (and still is, at 90) a very colorful character. He is half Jewish and served during World War I in the German army as a fighter pilot. I asked him to cooperate with me in my desert research, and a few days later we went to the Judean desert to find an appropriate spot for our work. I had brought with me the torsion balance for measuring transpiration and Huber's instrument constructed for measuring photosynthesis.

On our first trip to the desert, on a day when the dry hot desert wind (Khamsin) was blowing and the temperature rose to 42°C, it soon became evident that under these conditions the photosynthesis apparatus refused to work. It was also so heavy that Richter, carrying it on his back in that heat, collapsed with a slight heart attack and we had some trouble returning to Jerusalem. We had no money for a car and had to drag ourselves and our equipment to the faraway bus station. From then on we had to restrict our work to the measuring of transpiration. Thus began my first ecophysiological work in the desert, the results of which were published in 1937 and 1938 (4–6).

The moment I had my first glimpse of the Judean wilderness, I fell in love forever with the desert. I was spellbound by its somber and sublime beauty. It moved me emotionally, spiritually, and intellectually: emotionally and spiritually because in it man in all his tragic loneliness is confronted with nature in the raw; only here could God have spoken to Moses, Jesus, and Mohammed; and intellectually, because its faunistic and floristic structure is, in comparison with a jungle or even a prairie, comparatively simple, comprehensible, and researchable.

In 1933 I also became acquainted with the Jordanian, Syrian, and Iraqian desert. The department of botany of the Hebrew University was invited by the Iraqian government to make a survey of the forests of Kurdestan. Eig asked me to participate in the expedition, together with my instruments, in order to get an idea of the water balance of the Kurdestanian forests. We traveled by car from Jerusalem to Amman and via Kasr el Asraq to Bagdad, then via Kirkuk to Suleimaniyeh, and from there on horseback up into the mountains of Kurdestan. I had never mounted a horse and felt very romantic riding one, an emotion enhanced by an episode which earned me the respect of our military escort. Before riding out from Suleimaniyeh, we had to choose our horses. The Iraqi soldiers wanted to give us the tamest ones because of our inexperience. Out of silly pride I asked for a "normal" horse. As we started out, my horsc, an old cavalry steed, immediately began to run and I was soon at the head of our long column. The soldiers apparently got the impression that I wanted to challenge them and raced after me. My horse must have been accustomed to races and galloped faster and faster, jumping over ditches and trenches. I held my arms around its neck, holding on for dear life. I was deadly afraid and tried to slow it down but did not find the right brakes. Miraculously, I was not thrown off and was the first to reach the forest. From then on I was the racing champion of our party.

For the next two months we traveled through the forests along the Iraqian-Iranian Turkish border in an arc of scenic mountains from Suleimaniyeh, Rawanduz, Amadiyeh, Zakho, to Dihok. Wherever we stayed for a few days, the Kurds built an airy hut from tree branches in which I measured evaporation, transpiration, and stomatal opening of the main forest tree, *Quercus brantii*. From this tree the Kurds collected the galls for tanning and ink production. They also gathered its leaves, which are covered by a layer of sugar, and put them into large vessels where hot water dissolves the sugar that later is used for making Turkish Delight (Lokoum). The sugar is produced by aphids which tap the phloem, use the protein, and excrete the surplus of sugar.

During our journey we also detected in the mountains far off the beaten track two heretofore unknown villages inhabited by Jewish peasants speaking Aramaic, i.e. the language spoken in Palestine at the time of Jesus. They claimed to be in Kurdestan from the time of the Babylonian exile (7th century B.C.). However this may be, it is certain that these Jewish peasants were there for at least more than 1500 years.

When we returned to Jerusalem, Eig offered me a part-time position as "external teacher" in the department which I accepted.

My narrative so far may have given the impression that my transition from Germany to Palestine was smooth and painless. This certainly was not the case.

Palestine in 1933 was very different from the Israel of today. Large parts of what is today good agricultural land was unproductive swamp, steppe, and desert. Living conditions were harsh. We often had no water in Jerusalem; we cooked on small petrol stoves (Primus); there was no central heating in winter and Jerusalem can be very cold; electricity often failed. Daily life was further complicated by the Arab-Jewish conflict. Since the British police were unable to protect us, we had to defend ourselves. In 1933, at the time of one of the recurrent outbreaks of violence of Arabs against Jews, I joined the Jewish self-defense organization, the Haganah. This meant that I had to do guard duty two or three nights a week in one of the Jewish suburbs of Jerusalem. In the beginning this seemed to be quite romantic, but when it went on year after year it became quite a physical and emotional burden. It was not easy to lecture and give labs after a night of guard duty without sleep. But we felt it our duty to carry on with our research and teaching as if times were normal.

These physical discomforts were only minor nuisances in comparison with the language problem. The transition from Germany to Palestine had robbed me of my mother tongue. From now on I could neither lecture nor publish anymore in German. I had to do both now in English and Hebrew. I felt like a man deprived of air. In school I did not learn English and my Hebrew language was most scanty. In some way I acquired the rudiments of both languages. I had to because in 1934, less than one year after leaving Germany, I already had to lecture in Hebrew on plant physiology and anatomy, for which there were no Hebrew textbooks. When I knew Hebrew better I remedied the lack of text-books when I published, together with my assistant Konis, two Hebrew books on plant physiology and one on general botany and translated, together with Konis and Michael Zohary, the famous popular book by Timiriasew on "the life of plants" from Russian into Hebrew.

The fact that Nazism had driven me out of Germany and deprived me of German as a means of scientific communication persuaded me to get rid of my two German names. When in 1935 I took out Palestinean citizenship, Walter Schwarz officially became Michael Evenari. I choose Michael because that is the name of one of the guarding angels, and Evenari is the Hebrew for Loewenstein, the maiden name of my mother. Schwarz (black) in Hebrew did not sound nice to me. To change names was not an easy decision. The botanist

Walter Schwarz, who had already published 17 scientific papers, had buried himself, and the new Michael Evenari had to start ostensibly as a newcomer to science.

Besides my own work on the water balance of desert plants, I also induced some of my students to do ecological and ecophysiological research. These included Alexendra Poljakoff, E. Shmueli, and Ephraim Konis. This was the work I had already planned to carry out in Palestine when I was still in Germany, but the special conditions of my new homeland turned me toward new lines of research.

When Jews started farming in Palestine they did so without any agricultural tradition behind them. This had its drawbacks and advantages; drawbacks because they lacked experience; advantages because they were not bound by old agricultural practices. Since the new Jewish farmers were not peasants and were often university graduates, they tried to farm scientifically. Thus one day David Zirkin, a member of Kibbutz Ain Kharod, came to me and asked if there was a scientific method to stimulate the rooting of cuttings. This was the beginning of a cooperation between Zirkin, my assistant Konis, and myself, which lasted for many years.

We were the first in the Middle East to use plant hormones for root formation on cuttings of grapevines, figs, etc for stimulating the union of stock and scion in grafts. We found new ways to force early flowering of *Iris* bulbs and to break the rest periods of *Gladiolus* corms. But most important for my future research was the fact that Zirkin turned my attention to the difficulties he was having in germinating apple and plum seeds. Thus started our research on germination inhibitors and germination physiology which occupied me and the plant physiology section of the Hebrew University's botany department for the next 25 years. The first paper was published by my student Gershon Mosheov in the first volume of the *Palestine* (now Israel) *Journal of Botany*, which was founded and paid for by the staff of the department. Mosheov found that a water extract of wheat grains first inhibits and then stimulates the germination of the grains. His two papers on germination inhibition were his only publications since this most able and promising student was killed in 1936 while on guard duty in a kibbutz which was attacked by Arabs.

I enjoyed the cooperation between science and agriculture. It stimulated my scientific curiosity and led to a number of unexpected results, and it made me feel that I contributed my share to the development of the country.

At this point I must say something which many of our colleagues may not accept. The intellectual and emotional satisfaction we get from our profession in the automated world of today is a great privilege for which we owe society a debt. One of our duties should be to apply our knowledge to the solution of practical problems. At the same time we should force society to give us a decisive role regarding the way our knowledge is going to be applied. In specific cases, we should have the moral courage to refuse to divulge our knowledge if we feel that its application would or could be catastrophic.

One of the many departmental excursions was of special importance for my future work, though at the time I did not realize it. In 1936, on the way from Amman to Aqaba, we visited Petra, the capital of the Nabateans, one of the seven wonders of the world. Besides the breathtaking beauty of the many tombs hewn into the red-rose Nubian sandstone, the many waterworks, channels, and cisterns aroused my curiosity. What was the water source of this desert city? Who were these Nabateans? I started to read out of curiosity about the culture and history of this forgotten nation, but the problem of their water source must have sunk into my subconscious and remained there until it popped up much later.

WORLD WAR II

In 1940 I volunteered for the British army. The unit I joined was called the "Palestine Light Anti-Aircraft Battery" and was composed only of Palestinian Jews, mostly members of kibbutzim, all members of the Haganah. Apart from wishing to show Hitler that Jews were not just victims of his persecution but could fight back, Rommel was nearing Egypt, and were he to occupy Palestine its Jewish population would be in danger. We were ready to turn our unit with all its weapons, together with the Haganah, into a guerilla fighting unit in order not to be slaughtered without resistance. Later we were also to fulfill another function: to seek out in Europe Jews who had survived the concentration camps and to smuggle them into Palestine. I thought that under the circumstances all this was much more important than to continue scientific work as if everything were normal.

This is not the place to tell of my experience during 5½ years in the British army. I only want to mention that even as a soldier I found time to botanize. During a period when I was stationed in Cyprus I systematically explored the flora of the island, collecting plant specimens everywhere. Floristically, Cyprus is very interesting because it contains a great number of endemic species. These I hunted specifically: on the igneous alpine top of the Troodos mountains above the beautiful forest of the Cyprus cedar (*Cedrus libani* var. *brevifolia*) and in the limestone mountain range stretching from the magnificent ruins of the Bellapais monastary to the eastern tip of the island at Rhizokarpas. I collected a whole herbarium which I sent to my colleagues at the Hebrew University.

Of my other experiences in Cyprus I mention only that there my knowledge of ancient Greek paid dividends in the material sense of the term. I had only to enter one of the many Greek-owned taverns and to cite some verses of the

Odyssey, the Iliad, or from a drama of Euripides or Sophocles to be treated to a free glass of cognac or wine.

I also botanized in Italy when the Jewish brigade, into which our unit was incorporated as a regiment of artillary, was sent from the Eastern desert to Italy.

One interesting event of our campaign in Italy occurred while I was in a military hospital in Rome. When I was able to leave the hospital for some hours I visited the Vatican, by chance on a day when Pope Pius XII gave an audience to allied soldiers. After a short speech he went round the first row of the audience where I, heavily bandaged, had been given a place. Everybody whom he passed knelt down and kissed the fisherman's ring. Being non-Catholic, the English officer next to me and I remained standing, bowing slightly. The Pope stopped and asked me in English, "From where are you my son?" On the spur of the moment—I think I just wanted to show off—I answered him back in Latin: "terrae sanctae civis sum Judaeus. Tibi gratias ago nomine populi Judaei quia salvabas vitam Judaeorum tam multorum" (I am a Jew from the holy land. I thank you in the name of the Jewish people for saving the lives of so many Jews). The pope looked at me, slightly taken aback and asked me in *Hebrew:* "Then you speak Hebrew. Let me bless you." He then extended his hands over my head and with spread fingers like the ancient priests of the Jerusalem Temple, gave me the priestly blessing in Hebrew, "The Lord bless thee and keep thee. The Lord make his face shine upon thee and be graceous unto thee. The Lord lift up his countenance upon thee and give you peace" (Numbers 6:24–26).

I thanked the pope for saving Jews because during our campaign in Italy we met with many Jewish families who were saved from the Germans, finding, by the pope's order, shelter in monasteries.

AFTERMATH OF WAR

When the armistice was signed in May of 1945, we found ourselves in Palmanova, south of Udine in Northern Italy. We asked our commander to permit us to prepare for our return to civilian life. Since the majority of the soldiers in our unit were farmers or agriculture students, we wanted to do agricultural work. I was asked to organize this venture, and we established ourselves in Fagagna, a small, romantically situated village in the Italian alps not far from the Austrian border. We lived with the peasants in their homes. The "students" worked in the morning in the fields, and in the afternoons I returned happily to my real profession, teaching them botany as the basis for agriculture. In my free time, I botanized in the mountains, collecting another herbarium. My most interesting trip was to the Triglav on the Italian-Austrian-Jugoslav border, where at a height of about 2800 meters, I had the incredible experience of seeing a whole field of Edelweiss, ordinarily a very rare alpine flower. Apparently because of the war, nobody ascended the mountain and the Edelweiss was able to expand unhampered by man.

I returned to Palestine after my demobilization in August 1945. I continued with my physiological-agricultural work but spent most of my time in the service of the Haganah because the country was in turmoil. The British did not permit the entry of the refugees from the concentration camps, so we had to bring them in secretly. In 1947/48, open war broke out when the state of Israel was established and the armies of six Arab states attacked us. We lost the access road to the Hebrew University on Mount Scopus. In spite of the fighting and the loss of our equipment and our books, we continued to teach in private houses, but our research came to an end. Since the finances of the Hebrew University were in a catastrophic state, I was sent to the United States and South America to collect money from the Friends of the Hebrew University for the university's upkeep. This work kept me and my wife Lieselotte, whom I had meanwhile married in New York, busy for the next ten months. I was then granted a year's leave of absence and spent this sabbatical at Caltech, which offered me a visiting professorship. The time I spent there was the happiest and most productive period in my whole scientific career. I can only repeat what Anton Lang wrote: "No one who has passed through Caltech has left quite the same person, and probably retains a trace of regret of having left" (10). At that time Beadle, Frits Went, and James Bonner were the leading biologists, together with Arthur Galston and George Laties. I soon became friends with Frits Went, but the man who had the greatest impact on my work was James Bonner. He advised me to investigate the factors affecting the germination of lettuce seeds, paying special attention to the effect of light and to germination inhibitors. I followed his advice and for the next 20 years germination of lettuce became the main theme of my work and that of the Hebrew University's physiology department.

I had now turned full circle back to my juvenile infatuation with the phenomena of germination. When late in the 1960s somebody at an international meeting talked about the "Jerusalem School of germination research" I was filled with secret pride.

I may be permitted here to intersperse a general remark. At the time I am speaking of, all the members of my department concentrated all their efforts on the elucidation of one problem, seed physiology. Today, this situation has changed completely. There is no unifying topic anymore. While each member may be quite excellent in his field, I doubt that this is really the most productive way for a university department to proceed in research. There is less mutual intellectual cross-fertilization, and there is also a material disadvantage—more and more expensive equipment is needed which cannot be bought since money for research is so scarce. This difficulty would be minimized if the department had a common topic of research.

When I started to write this story I did not realize that I let myself in for a soul-searching process. Therefore I have to confess here that I myself am at least partly to blame for what happened to my department. Years ago I had propagated the idea of having an institute of life sciences at the Hebrew University, incorporating botany, zoology, and biochemistry. The idea was good, but the way it was executed was disastrous for biology since it led to the deterioration and internal dissolution of zoology and botany. We no longer have one integrated research program, no longer one team working together, which still seems to me the best way to achieve optimal results.

When I left Caltech and returned to Jerusalem I found that the various university departments were housed in 55 different buildings all over town because we had lost access to our Mt. Scopus campus. Botanical taxonomy was located in a hut about 3 km from the physiology section, housed in a building of the former British police. Our students were continuously on the run from lecture to lecture. Our equipment was very poor. To break a Petri dish was a disaster because it was so difficult to replace.

ISRAEL

I became vice president of the Hebrew University in 1953, and my archaeological colleague and friend, Benjamin Mazar, became rector and president. Our first decision was to build a new campus in town. I served as vice president from 1953 to 1959. During this time Mazar and I, aided by an able administrative staff, were able to build from scratch a new campus in town. When I felt in 1959 that the main work was done and that a longer stay in that office would cut me off for good from scientific work, I resigned.

During my vice presidency, an event occurred which guided my work into a completely different direction. My then PhD student, Dov Koller, invited me on a trip to the Negev Highlands, to show me the impressive remains of ancient desert agriculture in a wadi (dry river bed) near the ruins of the ancient Nabatean city of Avdat. Looking at the ancient terrace walls, water conduits, channels, and fields, I remembered Petra and what I had seen there. All of a sudden my interest as to how this could have functioned was rearoused and I decided on the spot to try and find out. Somebody told me that I should meet one of our students, Naftali Tadmor, who, as he said, was himself bewitched by the Negev. We met and were immediately drawn to each other as if by some magnetic force. He introduced me to the hydrological engineer Leslie Shanan, and the three of us decided that we would dedicate ourselves to the investigation of the ancient agriculture of the Negev Highlands. For the next 20 years until the untimely death of Tadmor, we worked together as a team.

The late archaeologist, Johanan Aharoni, helped us in the historical-archaeological part of our work. From 1954 to 1958 we spent at least one week each month in the Negev surveying and excavating ancient farms and villages. In 1958 we published the first of a series of papers on this topic, and in 1971 we summarized our experiences in a book (7).

But we were not satisfied with theorizing about how the ancient farms functioned. The instigation to do more than theorize came from my wife Liesel. In August 1956 we were eating our lunches near the ancient Nabatean town of Shivta, and hotly exchanging our opinions about the working of the ancient farms, when my wife said, "Why don't you test your theories and reconstruct an ancient farm?" And so we did. We first rebuilt a farm near Shivta, then one near Avdat, and later a much larger one in Wadi Mashash.

Our chief motivation was to find out if the water source of the ancient farms was really the runoff the ancient farmers collected from a catchment area and led through channels into the terraces of their farms. When we saw that this worked in our reconstructed farms, we went one step farther and wanted to know if we could grow agricultural crops in the farms and if this could be important for turning part of the desert into agriculture land. As a result we now have three flourishing runoff farms in the Negev and various runoff projects in a number of developing countries [see the second edition of our book (7)].

The Avdat farm, which started as an agricultural project, also became a research station for the ecophysiology of desert plants and cultivated plants growing under desert conditions, and for the investigation of structure and function of desert ecosystems. This started in 1971 when Otto Lange, Detlef Schulze, Ludger Kappen, and Uwe Buschbohm of the University of Wurzburg came to Avdat, together with their sophisticated equipment for measuring in situ photosynthesis and transpiration. They stayed with me in Avdat for eight months, undisturbed by any outside interference. During this time we worked as a happy team, living in our desert home, working in the field, and, during our meals and in the evenings, discussing our work, theories, and opinions about everything under the sun. It was like living in a scientific kibbutz. The results of our investigations were published in more than 30 scientific papers. This was only the beginning of the manifold multidisciplinary research projects at Avdat. We investigated the effect of salinity stress at different levels on photosynthesis, transpiration, ion uptake, water and osmotic potential, biomass and fruit set of the pistachio tree; also of various degrees of salinity on structure and composition of the soil. In parallel we investigated the effect of water stress on the almond tree. These two trees were chosen for these experiments because they are, together with the olive, of all the fruit trees tested the most promising for practical desert runoff agriculture. A third project deals with the decomposition process of organic material and the nutrient and especially the nitrogen cycle in three different desert ecosystems, with special emphasis on the question of who the main decomposers are.

As I arrive at the end of my story, I look at my scientific and personal life; it seems to consist of a series of lucky accidents that have enabled me to live a full and happy life and to fulfill most of my scientific ambitions in spite of World War I and II, the rise of Nazism, the Holocaust, and the continuous fighting in Palestine and Israel. But sometimes, lying awake at night, I ask myself if these "accidents" guiding me safely throughout my life were really only chance events. I will never know.

PHILOSOPHY OF NATURE

In telling the story of my scientific life I have here and there inserted some remarks of a general nature which I would now like to expand upon by summing up the lessons that my life and experiences have taught me concerning what was once called the "philosophy of nature." The motivation for giving my own view of the "philosophy of nature" here stems from my experiences with many students who harbor a naive trust in the absolute "truth" of science and a belief that, given time, science will enable us to understand the working of nature and of the whole universe. One of the reasons for our naive belief in the all-explaining nature of science is the equating of knowledge with understanding; another one is that we all grew up in an atmosphere of materialistic philosophy. I agree with this type of philosophy in one respect only: Scientific research is only possible if we act *as if* our mind would be able to unravel all the secrets of nature, but we should not fall into a trap of our own making in believing that this is really true. In carrying out our work we should always be conscious of the limitations of science. These limitations lie not in the fact well known to neurophysiologists that we use only 2–3% of the computing potential of our brain. In the future we may be able to use more of this innate potential. This will increase our knowledge of nature, leading to the invention of much more sophisticated tools of research and to the formulation of more advanced theories. It will not necessarily mean that we will apply this new rationally acquired knowledge in a rational way to our daily life, nor will it bring us nearer to any absolute truth about nature.

First of all, there is simply no "absolute truth," but only relative truths, theories which change with the accumulation of new knowledge. The absolute truth that we endeavor to chase is a phantom that recedes as we try to approach it. This statement can be based on various arguments. One is that each step taken on the so-called "frontier of science" into the yet unknown turns a seemingly simple problem into a more complex one that poses more questions than the original problem did. Du Bois-Reymond, a great physiologist of the past century (1818–1896), has illustrated this with a fitting simile: A man faces a closed door. He tries hard to open it. He succeeds—and finds himself in a passageway leading to many other closed doors.

I take photosynthesis as an example. In my early student days, the professor, when talking about photosynthesis, wrote the following formula on the blackboard:

$$6CO_2 + 6H_2O \rightarrow C_6H_{12}O_6 + 6O_2$$

This was simplicity itself and seemed to explain the whole phenomenon. It was so simple that we were certain (including the professor) that soon man would be able to duplicate the process in factories. How naive we were! Since then thousands of scientists have worked on the problem, and the more we know the more we realize how complex the process is.

I take another example from physics. In my student days the Bohr model of the atom was the ultimate and seemingly final model explaining the structure of all matter. It was so beautiful and attractive because it seemed to mirror the solar system. It is superfluous to describe what has happened since then to the model. The innumerable new subatomic particles, some of them no more than mathematical symbols, detected since then have made the Bohr model untenable today, and the structure of matter is still a riddle.

All this means that in basic research we continuously are faced with a multitude of new facts that we cannot put together into an understandable pattern. Then somebody with intuition will build a theory which puts all the pieces together into a unified model. When new facts are found, the theory has to be abandoned and so on. There is a continuous interplay and contradiction between accumulated knowledge and the meaning of it. We are dealing with the spiral of unification - discordance - synthesis. Interestingly enough, Hegel, the father of this idea of the triodic "progress" of science, human thought, and human society, has illustrated his system with an example taken from plant physiology: "The seed of a plant is an initial unit of life which, when placed in its proper soil, suffers disintegration into its constituents, and yet, in virtue of its vital unity keeps these divergent elements together and reappears as a plant with its members in organic union" until the whole process is repeated (my addition).

The quest for truth has therefore no end because the dialectic spiral points to infinity. Our motivation for questing is a combination of awe, curiosity, and doubt. It does not stem, as we sometimes pretend, from our noble wish to benefit mankind and to improve society.

There is also another reason to believe in the limitation of our cognitive ability. It concerns the nature of our brain, which, as we are told, works like one of the products of our brain, the computer. We improve almost daily on the quality of these man-made machines, increasing their storage and combining capacity. We teach them languages, word processing, drawing, music, etc, and in certain respects they are supposed to be more intelligent than we are. Can we in the same way improve our own brain computer? Certainly not, at least not above a certain innate potential. The limit to this potential lies in the physiological and material structures of our brain. We improve man-made computers by changing the material they are made of, putting in more and better computing units, changing circuits, etc. All this we cannot do with our brain because we have no control over its material structures as it emerged during evolution.

I can only allude to another possible limitation of our cognitive ability. We can only think in language, our thinking tool. The languages may be of different kinds, including the symbolic language of higher mathematics. Therefore our thinking activity is incarcerated in the cage of language. We can, to a certain extent, improve on language as we do in mathematics, but our thinking cannot jump out of language and be absolute, not tied to it.

In addition to these theoretical arguments concerning the limits of our thinking potential there is a more realistic argument regarding the physical existence of *Homo sapiens,* the only species that can think about the limits to thinking. I do not have to prove that there is a danger that our species may soon disappear. The question which I, and I suppose many others, have asked again and again is why and how mankind could have come to such a state?

I, for myself, can answer this question only by referring to what Arthur Koestler has called "the pitfalls of mental evolution" (9), i.e. the asymptotically increasing distance between the curves of what he calls "physical power of the race" and the curve of its "spiritual insight, moral awarencss, charity and related values." The latter "curve" is nearly not curved.

It is more an overall straight horizontal line. Since the time our species appeared on the scene of evolutionary history, its structure and physiology have not changed and its emotional, spiritual, and moral qualities changed very little. Their time curve shows here and there some ups and downs but remains basically a nearly horizontal line. The species' physical power line, however, looks very different. For several hundred thousand years this time curve remained also more or less straight until at some point something happened that had never happened before in evolution. Man started to develop his inventive capacities on his own without being forced to do so by any evolutionary pressure. He invented tools, learned, and became a power never before seen on our globe, a power that could change the environment drastically and could even interfere in the order of the solar system.

In doing so the species jumped out of the normal evolutionary framework. It seems to me that the Bible has symbolized that event by telling us that the moment man ate from "the tree of knowledge of good and evil" (Genesis 2:17) he was chased out of paradise. He became "*Homo sapiens*," the *knowing* man. From there on the physical power curve rises "in leaps and bounds; and in the last fifty years . . . the curve rises so steeply that it now points almost vertically upward" (9). The discrepancy between the two curves has a deep biological meaning. It points to a basic evolutionary disharmony.

The inventive capacity that our species freely evolved increasingly lost all ties with our biological and spiritual attributes. *Homo faber* (man the maker) came into being sometime in our past. In the beginning he created only simple stone tools. Then the pace increased dramatically until he invented atomic weapons, means to leave the globe, and so forth. But the spiritual, moral, and social means needed to control the use of these inventions remained at the stone age level, creating an evolutionary paradox. A creature of nature changed its environment without being able to adapt itself to it fast enough and in an adequate way. Theoretically, this creature should have been able to progress by consciously self-made adaptations parallel to its own inventional capability.

Evolution always shows that when a gap opens between environmental change and adaptability of a species the species disappears. When this happened in the evolutionary past it was a slow process. In man's case today it could be a very fast one leading to racial suicide, again an evolutionary first.

Now I would like to close with some more specific observations addressed to biologists and plant physiologists.

I belong to the fast disappearing race of "botanists," i.e. people who, in spite of the need to specialize in certain fields, still have a *working* knowledge of botany as a whole. I consider this to be so important because only thus can the plant as a complex unit be understood. Since the whole is more than the sum of its parts, it can be comprehended only by considering all the structures and functions in their totality and their mutual interplay.

The counterargument to such an attitude is that today no single individual is able to implement this, and anybody who tries to is a charlatan. I myself was a victim of such an argument. For many years I gave the course of general botany to our first-year students until one of my former students and younger colleagues asked me to discontinue the course claiming that it was too superficial. This may have been right but even a "superficial" survey of the whole field is better than no such survey at all, or a course on general biology given by a large number of specialists, each of whom naturally stresses their own field. In such a multi-man course the plant as a whole disappears under the hands (one should say: under the words) of the many lecturers.

I understand well that in order to survive as professionals we have to work and to train our students in very specialized fields. But if this is all we do, we fail to *educate,* which is very different from training. If we lose the general view of our field, we will know more and more about less and less and understand less and less about the working of the whole. We could counteract the dangerous trend of overspecialization by introducing *obligatory* courses on the philosophy of nature and of science history. I remember that once in

Germany a science student was obliged to read at least one course in philosophy.

Overspecialization is dangerous because it hinders us from realizing that "the organism in its totality is as essential to an explanation of its elements as its elements are to an explanation of the organism" (9). It leads also to what I have called the "forgotten problem syndrome." In two papers I have given many examples of this syndrome, which is simply the fact that in our ambition to be always at the "forefront of science," as the phrase goes, we leave behind a trail of unsolved problems, the existence of which we forget. It has, for example, been known for more than a hundred years that many plants have integumental stomata. Haberlandt, the first to report this, also asked if these stomata have a function. Even today we do not know because the problem was forgotten.

Another example concerns the process of double fertilization of angiosperms. Modern textbooks describe this process glibly as if we understood it perfectly. In reality the most important parts of this process are unknown to us and the problems involved forgotten, as I have tried to show elsewhere (1–3). Parenthetically I confess that I have a grudge against most textbooks because they do not stress enough the many things we do not know and pass over the unsolved problems giving the student a lopsided view of science.

I began my story telling how, at age 13, I was awed by the phenomenon of germination. Today, after having gained some scientific knowledge, I still harbor the same feeling when looking at the incredible orderliness of living beings, their high negative entropy, and their high degree of adaptability. What is the cause of this orderliness? The genes? What causes the orderly function of the genes exactly at the right time and place? Supergenes? And if so, what controls the supergenes? And so I conclude my story appropriately with a question mark.

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