Viktor Gutmann

Viktor Gutmann was born in Vienna on 10 November 1921. He obtained the Higher School Certificate in 1939 and studied chemistry at the Technische Hochschule of Vienna. His studies were interrupted due to military service from 1941 to 1943. In 1944 he obtained the degree "Diplom-Chemiker" and in 1946 that of Dr. techn. (equivalent to PhD). He became Assistant Professor at the Institute of General Chemistry in 1946 and spent the years 1948–1950 as a British Council scholar at the University Chemical Laboratory in Cambridge under Professor H.J. Emeléus, F.R.S. There he obtained his second PhD degree in 1950. Back in Vienna, he obtained the *venia docendi* in inorganic chemistry both at the Technische Hochschule and at the University of Vienna in 1952. In 1952 he was appointed Full Professor of Inorganic and Analytical Chemistry at the University of Baghdad, but returned to Vienna in the following year. He became Associate Professor and Chairman of the Institute of Inorganic and General Chemistry in 1957 and was appointed Full Professor of Inorganic and General Chemistry in 1960.

In IUPAC he served in the Division of Inorganic Chemistry as a Secretary from 1959 to 1967, as Vice President from 1969 to 1973, and as President from 1973 to 1975. In Vienna he acted as President of the Chemisch–Physikalische Gesellschaft and Vice President of the Austrian Chemical Society. He organized four major Conferences, namely the VIII International Conference on Coordination Chemistry in Vienna in 1964, the IV International Conference on Non-Aqueous Solutions in Vienna in 1974, the 31st Conference of the International Society of Electrochemistry in Baden near Vienna in 1975, and the 4th International Conference on Solute–Solute–Solvent Interactions in Vienna in 1978.

Viktor Gutmann received the ScD at Cambridge in 1964, the Doctor honoris causa at Budapest in 1978 and at the Kopernikus University at Torun in 1982. He has been elected as a Full Member of the Austrian Academy of Science, of the German Academy of Science Leopoldina in Halle, and of the Mediterranean Academy of Science, Honorary Member of the Hungarian Academy of Science and of the Polish Chemical Society, Corresponding Member of the Academy of Science in Göttingen, of the Accademia Peloritana in Messina and of the Brunswick Scientific

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Society. He has been awarded the Rudolf Wegscheider Prize and the Erwin Schrödinger Prize of the Austrian Academy of Science, the Science Prize of the City of Vienna, and the Honorary Cross for Science of the Republic of Austria. Further awards include the Carl Friedrich Gauß Medal, the Josef Loschmidt Medal, the Wilhelm Exner Medal, the Johann Joseph Prechtl Medal, the Gold Medal of the City of Vienna, the Gold Medal of the Accademy Barcelona, the Gold Heyrovsky Medal of the Czechoslovak Academy of Science, and the Medal of the Technical University of Bratislava.

He has been invited to lecture in many places and has presented more than 500 lectures, among them more than 54 plenary lectures at international conferences. During his scientific career he published about 450 research papers, 5 books — some of them translated into other languages — and edited 7 books. Ninety PhD students performed their thesis under his direction, and 60 scientists from all over the world spent longer periods at his Institute, many of them as visiting Professors.

Viktor Gutmann lives in Perchtoldsdorf, about 10 miles from his Institute. He is married, has one stepson and two daughters — all of them married — and seven grandchildren. He enjoys family life immensely, is active in mountaineering, is administrator of the Lutheran Church in Perchtoldsdorf and is a lay preacher there.

A CELEBRATION OF INORGANIC LIVES

INTERVIEW OF VIKTOR GUTMANN
(Technical University of Vienna)

H. GAMSJÄGER*

On behalf of the editor of this Journal I would like to ask you some questions with regard to the pressures which drove you to science, what you did and how you did it, the people who influenced you, and the environment in which you worked.

Let me start by asking you to provide some information about why and how you decided to become a chemist.

First of all, I should like to express my thanks to Professor Barry Lever and to yourself for the opportunity for this talk. Let me start with my childhood, which I spent in the outskirts of Vienna. My father was chief designer of locomotives, and I had great interest in railways and streetcars. My father's hobby was the painting of landscapes. From him I learned to see the beauties of nature, and from my mother I received a Christian education, which led me to an astounding attitude to nature. This may be the reason why I was not much impressed by the formulas presented by our teacher in chemistry. I could not see relations between the properties of a material and those of its constituents expressed in the formulas. So I simply learned the subject matter by heart, in order to obtain reasonable marks. With this approach towards chemistry, I passed the higher school certificate in March 1939. There was no need to decide immediately which subject I should take at the university. Austria was part of Germany, and admission to university was granted only to those who presented evidence for the absolvation of a 6 months' term in the paramilitary organization of the Reichsarbeitsdienst. With the understanding of being released from there by the end of September 1939, I joined this organization on 1 April 1939. A few days before I should have been released, the German Army invaded Poland, on 1 September 1939, our unit was subjected to military law, and permission for discharge was granted only to those who provided evidence for enrolment either in chemistry or in medicine. So I decided to become a chemist.

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H.G. How did you feel as an undergraduate, with little interest in the subject, and what induced you to finally become a solution chemist?

Indeed, I had first of all to learn to be concerned with studies which were not of immediate interest to me. With some reluctance and with moderate success I passed the courses in analytical chemistry, but felt much better with synthetic work.

In May 1941 I was called up to join the army and spent most of the time in an anti-aircraft gun unit in Berlin-Humboldthain. I had the opportunity to attend lectures given at the Technische Hochschule Berlin–Charlottenburg by Werner Kossel and by Friedrich Hund and found opportunities to study physical chemistry, notably with the books of Karl Lothar Wolf. In October 1943 I was lucky to obtain leave from the army in order to complete my studies in Vienna. At that time I wished to become an organic chemist, but this proved to be impossible, because all of the benches in the laboratory had been taken before my arrival. I was offered a bench place at the Institute of Physical Chemistry, where Professor Hermann Schmid asked me to work on the kinetics of the polymerization of indene. I observed the enormous influence of molecular oxygen on this reaction and decided to study its effects in the course of my PhD thesis. Early in 1945 I was again called up by the army and spent the last days of the war in North Germany. From there I walked a distance of over 600 miles back to Austria and eventually reached Vienna. Immediately I resumed my PhD work and found that molecular oxygen was incorporated into the polymer chain, probably at the start of the reaction. I received the degree of Doktor der Technischen Wissenschaften, which is equivalent to a PhD degree, and became Assistant Professor at the Institute of General Chemistry of the Technische Hochschule (now The Technical University) of Vienna. Professor Alfons Klemenc proposed that I work on sodium orthonitrate, which was prepared from sodium oxide and sodium nitrate. It showed an X-ray powder diagram which was different from that of sodium nitrate, to which it was hydrolyzed by water. This aroused my interest about the differences in molecular arrangement in the solid state and in solution. With interest I read Jander's papers on "Waterlike Solvents" with emphasis on the self-ionization equilibrium, which he also assumed for liquid sulfur dioxide. Here I had difficulties in understanding the proposed ionization of thionyl chloride in sulfur dioxide into doubly charged SO^{2+} ions, rather than to $SOCl^{+}$ ions, in as much as the conductivity of the solution was low. On the other hand, Jander's interpretation was straightforward for a molecular description for the interaction of thionyl chloride with tetraalkyl-ammonium sulfite to give the tetraalkylammonium chloride and sulphur dioxide. The formal analogy to water and other protonic solvents pleased me, and I wondered if nitrosylchloride and sodium nitrate might react in liquid dinitrogen tetroxide in an analogous manner.

In 1948 I was elected to a British Council scholarship for a two year term at the University of Cambridge. When I arrived there in October 1948, Professor Harry Emeléus asked me about my research interests. I mentioned the N_2O_4 problem to him and was told that this work was already in progress under the direction of Dr. Cliff C. Addison at Nottingham. He added that the concept of self-ionization had been applied for the interpretation of ionic reactions in liquid bromine trifluoride in his laboratory, and that this work was performed by Dr. Alan G. Sharpe and Dr. Alfred A. Woolf. He suggested that I try to obtain new fluorides of transition metals in low oxidation states. As I also followed the work on bromine trifluoride, I soon started to use this solvent for the preparation of various transition metal complex fluorides. In 1950 I submitted a thesis entitled "Studies on Fluorides of Transition Metals" and obtained the degree of Doctor of Philosophy at the University of Cambridge. In the remaining weeks of my scholarship I started work on liquid iodine chloride and iodine bromide and was happy to explain the ionic reactions in these solvents again by means of the self-ionization concept. The analogy to water included even redox reactions: whereas potassium reacts with water to give hydrogen by discharge of hydrogen ions, it reacts with iodine monochloride to yield iodine by discharge of iodonium ions.

Back in Vienna, I submitted the "Habilitationsschrift" (post-doctoral thesis) on "Interhalogen Compounds as Ionizing Solvents" and obtained the *venia docendi* for Inorganic Chemistry both at the Technische Hochschule and at the University of Vienna. Laboratory work in these institutions was still hampered by post-war conditions, but I succeeded in carrying out conductometric and potentiometric titrations as well as preparative studies and extended these to arsenic trichloride, phosphorus oxychloride and sulphuryl chloride as solvents, whereas Spandau in Braunschweig investigated thionyl chloride, Gerhard Jander and Klaus Brodersen in Berlin mercuric bromide as solvents. I was pleased to find the solvent system concept widely applicable to ionic interactions in all of these media and published a review jointly with Spandau in *Angewandte Chemie.*

You mentioned that these analogy considerations were straightforward and applicable to ionic reactions in different inorganic media. I wonder if you were interested to see in what ways a deeper theoretical understanding might be obtained?

In the first place, I was interested in understanding qualitative changes. I had difficulty with the Brønsted theory of acids and bases because of its one-sidedness: according to these definitions of prototropism, an acid is required to contain ionizable hydrogen atoms, whereas a base does not have to contain particular atoms. So I considered the interactions in the halides as solvents as due to halide ion transfer reactions, rather than to proton transfer reactions, with the result that an acid may be considered either a cation donor or an anion acceptor.

In summer 1952 Dozent Ingvar Lindqvist, University of Uppsala, Sweden, came to me, in order to obtain more insight into my work, which up to then I had carried out alone. Ingvar Lindqvist was of my age, and he had done excellent structural work on polymolybdates and polytungstates. We talked about structures in the solid state and in solution, and one day he suggested the occurrence of halide ion transfer reactions. I showed him my notes on this point, and we agreed to prepare a joint publication on “Ionotropism”, which appeared in the *Zeitschrift für Physikalische Chemie* in 1954. In the meantime I spent an 8 month period as a full Professor of Inorganic and Analytical Chemistry at the Royal College of Chemistry and Pharmacy of the University of Baghdad/Iraq. At that time the book by the Austrian born chemist Ludwig F. Audrieth appeared, with whom a good friendship developed.

After my return to Vienna my interpretation by means of the solvent system concept was questioned by William Groeneveld, who suggested at the ICCC Conference in Amsterdam that $SbCl_5 \cdot POCl_3$ should be formulated as $Cl_5SbOPCl_3$ rather than $POCl_2^+$ $SbCl_6^-$, as I had proposed on the basis of the solvent system concept.

How did you react to this challenge?

Strangely enough, I had so much confidence in the solvent system approach that I assumed that Groeneveld must be wrong. I extended our studies to measuring the ionic mobilities in $SbCl_5$ solutions in $POCl_3$ and was pleased to obtain high transference numbers for the solvent cation, which supported the view of the occurrence of chloride in transfer reactions. Even when, in 1958, my friend Lindqvist had shown that Groeneveld’s proposal was indeed in agreement with the results of X-ray work on the solid compound. I thought that the situation in solution might be different. In that year I paid my first visit to the United States of America. Professor Fred Basolo had invited me to lecture at a conference at Northwestern University. There I learned that several American solution chemists favoured the Lewis theory of acids and bases and suggested the interaction of $Cl_5SbOPCl_3$ with tetraalkylammonium chloride as a substitution reaction:

$$Cl_5SbOPCl_3 + Cl^- \rightleftharpoons SbCl_6^- + POCl_3$$

I had to accept this interpretation as an alternative description and my long-lasting confidence in the self-ionization concept was shattered. This experience made me cautious with regard to all chemical concepts.

As a plenary lecturer at the IUPAC Congress in Munich in 1959 I discussed both possibilities. At that time ethylendiamine, acetonitrile and dimethylsulfoxide became available as solvents. It was clear that self-ionization was unlikely to play a decisive role in these solvents, and I decided to study these solvents, including spectroscopic and polarographic techniques.

In 1957 I was entrusted with the provisional directorship of the Institute of Inorganic and General Chemistry at the Technische Hochschule of Vienna and soon had about 15 PhD students, some of them following preparative lines on phosphorus–nitrogen and boron–nitrogen chemistry. Apart from the above mentioned solvents, we included phenyl phosphonic dichloride, benzoyl chloride and benzoyl bromide as solvents in our program. In the course of our polarographic work on ethylendiamine we designed the electrode arrangement, which is still used in non-aqueous solutions. In 1962 we studied gases polarographically in solutions of non-protonic solvents and published in 1963 a contribution to a book on Lewis acid catalysts.

Extensive lecturing trips through the United States were arranged for me in 1960 and 1961, including attendance at the VI ICCC in Detroit. At the VII ICCC in 1962 in Stockholm I was entrusted with the organization of The VIII ICCC in Vienna, which took place in September 1964. More than 800 participants took part in this meeting, where, apart from thermodynamic and kinetic aspects, ligand field theory, metal clusters, π-complexes and Pearson's concept of hard and soft acids and bases were extensively discussed.

On the basis of the Lewis concept you introduced empirical solvent parameters, namely the donor number and the acceptor number. Would you like to give us some information about the development of these concepts?

As I mentioned before, I had learned to consider solute–solvent interactions as EPD–EPA interactions. This meant that solvent molecules may react either as donor or as acceptor molecules towards solutes. In order to understand the changes in solute properties, as caused by different solvents, we followed Lindqvist's proposal to characterize solvent donor properties by the solvation enthalpies towards a reference acceptor. He had determined the enthalpies of interactions of some donor solvents, with antimony pentachloride and with tin tetrachloride. With Erhard Wychera I took up this lead and also determined equilibrium constants for the aforesaid reactions, but finally decided to introduce the $-\Delta H$ value for the interaction of a given solvent in dichloroethane as its donor number, and to use the differences in donor numbers in order to account for differences in other solution properties. We soon found relations to spectroscopic, thermodynamic and kinetic parameters and were even able to account for the differences in redox properties as expressed by the differences of the polarographic half wave potentials for a given solute in different solvents. Fortunately, it turned out that the donor number was related to the enthalpies of interaction towards other acceptor molecules, such as arsenic (III) chloride or iodine. Jointly with Ulrich Mayer it was shown that the ionization of a covalent substrate was not a matter of the electrostatic solvent parameters, but rather of its donor properties, as long as solvents of similar acceptor number were compared. Many chemists, however, found our criticism of the elementary electrostatic approach inconvenient. Instead, they criticized the donor number concept, because it was based on the measurement of an enthalpic rather than of a thermodynamic term. Fortunately, it turned out that in all of the solutions under investigation linear free energy relationships exist, so that the use of the enthalpic term is justified. When we set up the donor number scale we hoped to use it as a guide-line to account for differences in properties of a solute in solvents of different donor number, and hence to account for the qualitative aspects rather than for precise calculations. I pointed out repeatedly that consideration of the donor number is only a rough guide to compare solvent properties, because the acceptor properties and the highly specific features in the interactions between solute and solvent are not taken into account by applying only the donor number. It was also clear that, using the donor number, a property of solvent molecules is expressed. So I introduced — in particular for the so-called "structured" solvents, such as water or ethanol — the term "bulk donicity". We also realized that a pure solvent does not exist, and that it would be impossible to obtain a real "non-aqueous" solvent, as each of them contains traces of "unremovable" water, at least in the ppm region. I therefore suggested to use the term "solvent of extremely low water content".

Faced with these facts, it is obvious that precise predictions cannot be made. Because of the emphasis on quantitative aspects in chemistry and the low regard for the qualitative aspects in modern science, several efforts were made to introduce more "reliable" parameters, with which, however, greater precision was obtained in some cases, with consequent loss of precision in other cases.

You mentioned the influence of the acceptor properties of a solvent; perhaps you could give us some information about your efforts to take these properties into account.

Indeed, one of the arguments against the donor number concept was the fact that several other empirical solvent parameters had been in use, namely Winstein's Y values, Kosower's Z values and Dimroth and Reichardt's E_T values. These parameters were always useful, when the donor number failed and vice versa, but nobody could explain these facts. Since the introduction of the donor number I had always been aware of the need to introduce an acceptor number. This was however, more difficult, because we could not find a strong reference acceptor which reacted with every solvent by coordinating only one solvent molecule. Fawcett and Krygowski suggested that the E_T values might be related to the solvent acceptor properties in 1975. In the same year, with Gerhard Gritzner, the $E_{1/2}$ values for the hexacyanoferrate (III)–hexacyanoferrate (II) redox couple were measured in different solvents and their differences considered as being due to differences in acceptor properties. Jointly with Ulrich Mayer, triethylphosphinoxide was finally proposed as reference acceptor and the ^{31}P chemical shift in the NMR spectrum taken as a characterization of the solvent acceptor properties. The linear relationships between acceptor number on the one hand, and Y values, Z values and E_T values on the other hand, clearly indicated that all of these parameters expressed the solvent acceptor properties, and that the terms "ionizing power of a solvent" or "solvent polarity" were no longer appropriate. Quantum mechanics has clearly established the shortcomings of the elementary electrostatic approach, which cannot account for the charge transfer which takes place in every molecular interaction. For this reason the electrostatic approach is unsuitable to account for changes in structural features, such as those in internuclear distances or in bond angles.

Relations of the acceptor number to kinetic parameters were found jointly with Roland Schmid and the late Professor A.G. Parker in Perth in Australia. When invited as a Visiting Professor to Vienna, Professor Parker showed numerous relationships between acceptor number and thermodynamic properties, such as the free energies of transfer of halide ions and the effects of the solvent acceptor number on the rates of solvolysis and S_N2 reactions. These results of our joint efforts were published in the *Journal of Organic Chemistry* in 1978.

You mentioned the changes in internuclear distances as induced by solute–solvent interactions. Perhaps you could tell us how you reached the formulation of the rules which are sometimes called "Gutmann rules", and in what way they meet the requirements of our modern ideas on chemical bonding.

A few years before the introduction of the acceptor number, I came to the conclusion that each molecular system must be in a position to respond to each action from outside by appropriate "reactions", in order to minimize the changes provoked by the former. For example, Lenz's law in electro-magnetism states that an induced electric current is produced to oppose the change produced by the external magnetic field. The law of conservation of mass and energy as well as the law of inertia and indeed the energy principle imply certain abilities to counteract forces from outside. And the theorem of Le Chatelier–Braun states that the equilibrium is altered by a constraint, placed on the system in such a way as to tend to annul the effect of the constraint. In order to meet such requirements for the electronic rearrangements in molecular systems, I proposed the concept of chemical functionality. According to this, each electron transfer from a donor is followed by an electronic rearrangement that can be described by its function as an electron-pair acceptor, and the other way round. This concept was not accepted, although it was never shown to be wrong.

So I had to look for a different form of presentation. I started from Lindqvist's observations in 1963 about the changes in intramolecular bond lengths due to donor–acceptor interactions. I found several experimental results and quantum-chemical calculations which led me to formulate the bond length variation rules. These rules are completely independent of any model assumptions, as they describe the changes all over the system under consideration, and indeed they imply system behavior. The loss of negative charge of the donor atom is overcompensated by attraction of negative charge from other areas of the donor component. I called this the pileup effect of negative charge at the donor atom. The original gain in negative charge at the acceptor atom is over-compensated by an induced flow of negative charge towards the peripheric atoms, I called this the spillover effect of negative charge at the acceptor atom. According to this extended donor–acceptor approach, the changes are not confined to the sites of interaction, but rather spread over the whole of the system under consideration, and they are particularly well–pronounced at its boundaries. Most convincing evidence for the well-pronounced changes at the boundaries is provided by the famous work of Max Perutz on haemoglobin. He has shown that following oxygen coordination at the iron atoms, which takes place in the lungs, two protons are released from the ends of the protein chains, remote from the iron atoms by at least 143 atoms, in order to remove the carbon dioxide from the cell plasm of the erythrocyte. Consequently, two protons are attached to these positions far remote from the iron atoms, when oxygen is given up from them at the tissues, in order to allow dissolution of carbon dioxide as hydrogen carbonate ions in the plasm. Evidently, two different processes, namely the oxygen transfer from the lungs to the tissues by haemoglobin molecules and the transfer of carbon dioxide from the tissues to the lungs by means of the cell plasm, are interrelated.

At this point I ought to tell you that by the time I had started to write the book “The Donor–Acceptor Approach to Molecular Interactions” in 1974 (published in 1978), I consulted Dr. Gerhard Resch, a physician of internal medicine with much experience in homoeopathy. When I told him about my concept, he immediately realized its impact and its suitability to open up a general understanding of the properties of highly diluted solutions. From his experience as a physician and from his profound philosophical knowledge he knew that an observable system always acts as a unity through its outermost spheres, which therefore must bear all information of the whole system. He welcomed the consequence of the extended donor–acceptor approach, according to which the solvent contraction in a hydration sphere of a solute in water leads also to contractions between water molecules at the interface, which must be responsible for the lowering of the vapor pressure of the solution. We realised that so far all efforts in solution chemistry were concerned mainly with the influence of the solvent on the solute, this means with the active role of the solvent and by the passive response by the solute; he expressed the hope to study the effects of the active role of the solute and the passive response by the solution.

The investigation of these questions, however, is difficult, if not impossible, and I wonder how you proposed to overcome such difficulties?

You are quite right, there are fundamental difficulties to investigate these features. The first one concerns the obvious limitations for observation and for measurement of the actual structural differentiations and their dynamical changes within the solution. Their knowledge is, however, mandatory if we wish to gain access for the understanding of the quality of a system, such as of a solution. The second difficulty concerns the limitations of our concepts in chemistry, namely the molecular concept and the concept of chemical bonding. Their extension is, however, not a problem of chemistry, but rather a philosophical problem. The reluctance of scientists to deal with philosophy has been expressed by Carl Friedrich v. Weizsäcker in the following way: "In contact with the object of his research, the physicist develops thought patterns, which do not always conform to the traditional methods of philosophy. He senses this and acquires an instinctive aversion to philosophy, which he considers only as a commodity sold to him by a suspicious merchant, which he feels he cannot use in his household. ... without realizing that with the rejection of the philosopher he has not got rid of philosophy, but has himself become a philosophic dilettante. Unconscious philosophy is, however, on the whole, of worse quality than conscious philosophy, and so it is precisely the most profound thinker of modern physics who returns to philosophy".

We consider the main philosophical problem in the application of the nominalist's view to science. According to nominalism, the conceptions are merely names, which have nothing in common with a concrete object under consideration. This view allows the definition of concepts by means of the imagination and fantasy of the investigator and hence changes of their original meanings in arbitrarily reduced ways. We have become accustomed — although without being aware of it — to use names obtained in these ways with the result that we cannot reach the real things as they are, but rather our own model assumptions and we also become used to this view. This applies to many fundamental concepts, such as to those of order, continuum or quality.

Could you give us a relevant example concerning your aims to extend our basic chemical concepts?

Let me try to do this with the term "continuum" which was originally used in order to express uninterrupted and dynamically maintained relations, and which is a requirement for the existence of the observed differentiations. In modern science the term "continuum" is associated with non-differentiation or homogeneity, and sometimes even with rigidity. The latter property had been erroneously attributed to the so-called "ether", which was considered to serve as a reference system for the velocity of light. When this was shown to be incorrect by the results of the well-known Michelson experiment, the ether concept was dropped instead. It is not always realized that quantum mechanics, the theory of relativity and mathematics, also requires a continuum which influences and maintains the enormous differentiations observed on the atomic and on the molecular level.

Do you wish to say indeed, that isolated atoms and isolated molecules do not exist?

Indeed, observability and measurability by the investigator as well as the occurrence of interactions between atoms and molecules exclude their complete isolation. When we talk about isolated atoms, we mean widely separated atoms, for example in a vacuum, where they are still loosely connected. These connections have been eliminated in the model of the ideal gas, which — for this reason — would be unobservable and could not exist. As described in our articles in *Chimia Oggi* in 1992, we consider the atoms and molecules "embedded" within continuous relationships and influenced not only by their interactions with the molecular environment, but also by the actions of the continuum, which, unfortunately, are not directly accessible. This statement is in agreement with recent results of quantum mechanics, which describe — in principle — the whole universe as one continuum by means of a charge density pattern, which is fluctuating in space and time. These fluctuations are influenced by the properties of the continuum, which is also described by quantum chemists as a "vacuum that is not empty". This vacuum bears the zero-point energy and has "structure-modifying" properties. Recent papers by Puthoff revealed the responsibility of this "vacuum" for the so-called Lamb shift in the hydrogen spectrum, the unreproducible relaxation times of excited electrons, and hence its "ability" to influence the properties of the parts. Indeed, we suppose that each selected subsystem would not be conceivable, had it not an ability to maintain its chief characteristics in different environments. In other words: we consider everything as organized, subject to a system organization, and if this statement is true — and so far we have not been shown to be wrong — it is not only justified, but rather a central demand of science to investigate system organizations.

Would you care to explain what you mean by a system organization and to tell us in what ways it may be investigated?

Here we are again faced with a philosophical problem: a system organization is a requirement for each real thing and not the result of our interpretation. It cannot be coincidental and requires different parts and a patterned structure of the dynamics of the system which coordinates the activities of the different parts according to the specific requirements of the whole. The fact that the system retains its integral configuration implies that every change of any part of the system affects the actions of every other part, just as if at all times every part "knew" the activities of every other part, in order to maintain the integrity of the whole system in cooperation with the other parts. According to this statement, the editor of the IUPAC Journal *Chemistry International* has suggested to have our contribution entitled "Intelligent Molecules".

In order to learn about these activities — which are not directly observable and not measurable — we have to start from the observable differences of the parts within the system under consideration.

These differences cause continuous motions, so that any change in each given area is bound to lead to changes, however small, in all other areas, according to a certain motion pattern.

Faced with the non-observability of such motion patterns and with the enormous multiplicity in nature, you will have to simplify and provide a new model.

It is true that we have to make use of abstractions, but we suggest to use them as tools in the course of a scientific cognition, and not as results of the investigation. Because of the existence of local and temporal differences in energy, we concluded that superordinations and subordinations must exist, and that all of the various parts are subjected to them. Such hierarchically ordered relationships must be continuous, but for observations discontinuities are required. Gerhard Resch suggested the introduction of artificial discontinuities by means of abstractions and to order the observed differences according to common criteria. All that can be summarized under a certain criterion may be considered as a hierarchic level. Each always remains an integral part within the whole system, and hence the mutually influencing hierarchic levels must be chosen in such a way as to recognize similar differences in dominance in the course of the continuous transitions. In order to choose the hierarchic levels properly, we must take into consideration all qualitative observations and all quantitative data within the macroscopic and the microscopic areas and include all so-called "anomalies". Hence the investigation of system organization cannot be the task of a specialist in one field, but requires joint efforts of scientists with broad interests.

Would you like to illustrate these points by applying them to a particular system?

Let me, first of all, say that we proposed to distinguish between the system organizations of molecules, which we call "molecular system organizations", and those of the multimolecular systems, such as clusters, liquid or solid materials, and which we suggested be called "supermolecular system organizations".

Since most of the readers of this interview will be interested in complex molecules, I may mention our approach towards tris-(phenantroline) iron complexes. These show various "anomalies". For example, in the oxidized and in the reduced state the Mössbauer isomer shift is about the same and is influenced little by different solvents or by different substituents. It is obvious that the different magnetic properties of oxidized and reduced species should not be ascribed to d orbitals of the coordination center, but rather to the electronic state of the whole molecule.

The region around the coordination center appears to act as a regulating "unit" for the redistribution of the charge density pattern over the whole molecule after each change in environment, and hence we suppose that in the molecular unit the coordination center is the hierarchically highest level that is observable.

Similar considerations may be made for biologically active molecules, such as haemoglobin.

You suggest that the concept of system organization may be of importance for future developments in biochemistry and biology. How can you be sure that such suggestions have not been made before?

You are quite right, this approach is not new, but it has never been fully accepted. The biologist Bertalanffy strongly recommended the application of system theory, and other biologists suggested the application of cybernetics to biological systems. Paul Weiss, a native of Austria, strongly emphasized the need for investigations of system behaviour in biological systems, but his advice was not heeded, possibly due to insufficient methodological directions.

Do you feel that you succeeded in mastering these difficulties?

The approach we suggest is most unusual for a scientist, because each system organization is immediately related to the quality of the system under consideration. In the first place, this approach is not meant to serve quantitative predictions, but to rather provide a qualitative understanding of things as they are. Again we are faced here with a philosophical question, because in present science the quantitative aspects are given priority over the qualitative aspects.

Chemistry is concerned with qualities and their changes, and so I think that our approach may be suitable to open up a new pathway for scientific knowledge of qualities and their changes. That is why I think that this approach may bring us back to the fundamental questions for an understanding of chemical changes and to encourage scientists to overcome the specialists' attitude and to gain a broad understanding.

I am optimistic of applying the approach of supermolecular system organization to materials science, in particular to provide a better understanding for the preparation of highly metastable materials. Metastable states are characterized by activation parameters. Catalysis is concerned with questions of decreasing activation energies, whereas our approach may be suitable to learn how to increase activation parameters to obtain materials of greater metastabilities, which means of better developed system organization.

In particular, our approach may be suitable to provide a better understanding of the liquid state and of solutions both in water and in non-aqueous solvents. We have shown the enormous influence of dissolved gases, notably of oxygen, on the preservation of the information content of solutions in water, and in this way I seemed to have returned to my starting point in chemical research in 1946, when I noted the enormous influence of dissolved oxygen on the polymerization of indene.

Let me finally ask you in what ways you will be able to continue your efforts after retirement?

Free from all duties, I hope to intensify our theoretical and philosophical studies with Gerhard Resch and possibly to write one or two books. I think, however, that the fate of our approach may no longer depend so much on our own efforts, but rather on engagements of scientists in various fields who are prepared to reorganize their expertise knowledge from the point of view of system organization.