

Roald Hoffmann

Theory in Chemistry

A most difficult task for a scientist, or any human being, is to give a fair and yet critical account of the work of others. I could not possibly do justice to the achievements and failures of all of theoretical chemistry, nor do I wish to become a negative citation of the future by failing to predict where the forthcoming successes of theory will be spawned. Rather, I would like to make some informal and highly personal comments on the role theory plays in chemistry.

What is theory? I, for one, would never abdicate the practice of theory to professional theorists. Theory is sound, logical thought. Mathematics is a materialization of logic, and so it is inevitable that much theory carries a mathematical component. Yet is not the design and analysis of a beautiful experiment theory? I think it is. The kind of logic that goes into a classical mechanistic experiment such as the Doering and Roth determination of the preference for a chair transition state of a Cope rearrangement, or the Woodward chlorophyll synthesis, or the building of Turner's photoelectron spectrometer, is good thinking at its best. It is theory.

This kind of theory has been eminently successful. No wonder, for logical thought is the essence of the scientific method. The tremendous advance of chemistry in the 200 years since Priestley's discovery is the consequence of rational thought.

Suppose we indeed consider the enterprise of those people whom chemists call theoreticians—scientists who do not carry out experiments. (We have to be careful not to stretch that definition by default to include research managers and other planning staff! Or perhaps we should count them in. In a sense they, too, are theorists, and their far-fetched schemes are sometimes met with the same derision that is accorded to some theorists.) Professional theoretical chemists, with few exceptions a phenomenon of this century, have been working in two different ways. First, there is the development of theory associated with an experimental situation, that allows the transformation of macroscopic observables into information concerning the microscopic properties of molecules. I



have in mind here the kind of theory that takes a set of faint lines on a photographic plate, the high-resolution electronic spectrum of a molecule, and from it extracts the geometry of that molecule. The full and impressive apparatus of quantum and statistical theory is put to work in this process, and often it has yielded the information required.

Another occupation of theoretical chemists is the rationalization and prediction of observables, from the existence of a particular molecule to, say, its quadrupole moment. Given the human character, rationalization poses no problem. Prediction is another matter. Press releases claiming that we don't need to do experiments because a computer can predict the result of any experiment justifiably are treated as a source of hilarity by experimentalists who know better. By and large, theory has not predicted much chemistry.

There have been some exceptions, some instances where theory made an impact on an experimental field. I would call such an instance a credibility nexus—a place and time of interaction in which a group of experimentalists, otherwise skeptical of theory, suddenly found itself faced with the success of a simple theory. That set of specialists quickly became converts, often zealots. These episodes of doubt and conviction are important to identify because they play a crucial role in the peculiar symbiosis of theory and experiment to which I will return later.

Here is a personal selection of some of these credibility nexuses. One is the experimental probing of the Hückel rule by organic chemists since World War II: tropylium and cyclopentadienide, two- and 10-electron ions, annulenes simple and bridged. It is important to note the counterproductive part of this story—the romance with Hückel's rule became an infatuation with stabilization and resonance energies as derived from simple Hückel calculations. Many man-years were spent in attempts to make molecules that were supposed to be stable but in fact weren't.

A second instance seems to me to have occurred as a consequence of Walsh's rules. Here the clientele were

spectroscopists. It is my impression that, though they knew about molecular orbitals from the pioneering work of Hund and Mulliken, they really didn't believe much in the utility of the theory until Walsh demonstrated in 1953 that geometry changes in excited states of polyatomics were derivable from simple symmetry and bonding diagrams.

A third instance may have been the interaction in the late 1950's and 1960's between simple Hückel calculations and electron spin resonance spectroscopy. That a most simple theory gave a realistic spin distribution for an anion radical of an aromatic molecule made many physical chemists respect a theory they would otherwise have been likely to reject. A fourth instance was the revival of crystal field theory in the middle and late fifties, contemporaneous with a renaissance of inorganic chemistry. A fifth case is the current activity concerning critical phenomena. Here is a vital interweaving of experiment inspired by theory, theory motivated by experiment, binding in a truly interdisciplinary way chemistry, physics, and engineering.

There are other credibility nexuses that I could describe—indeed I had the good fortune to participate in one, the complex of ideas called orbital symmetry conservation. It is possible to speculate on such nexuses in the future: I would place my bet on the rational design of a pharmaceutical that will bind to a preselected virus component, or on the design of a transition metal-catalyzed process for some economically important material.

The very fact that we must seek out these moments of profound connection is testimony to the primarily experimental nature of theory. Theorists must apply their art intelligently if they wish to maximize the prospects of their making an impact on the science. Perhaps it is appropriate here to enumerate some of the circumstances in which chemical or physical theory just might have an inherent advantage over experiment:

- Theory allows calculations on unstable molecules, unstable conformations of stable molecules, transition states for reactions not followed, to be performed with as little or as much difficulty as the corresponding computations on stable molecules. The purpose of such calculations is not merely to quantify the energetic misery of these unstable species, but to discern from the calculations what are the factors responsible for their instability. If we understand these factors, we will be able to devise a strategy for moving the molecules to lower energy. Theory serves uniquely here, for the constraint of the Boltzmann

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factor makes unlikely the experimental probing of such metastable species.

- Chemistry is discontinuous, but theory allows and often dictates continuous variation. One example is the Karplus curve for vicinal proton-proton coupling constants, derived theoretically as a continuous function, probed experimentally by discrete conformationally fixed molecules. Another example is the concept of a continuously varying dimensionality in the recent advances in critical phenomena, contrasted with the reality of three dimensions and the modeled "reality" of one or two dimensions.

- Observables in chemistry may be the resultant of several simultaneously operative physical mechanisms. The measurement cannot resolve these mechanisms (though a series of observations on related molecules may provide that resolution—witness the elegant dissection of through-space and through-bond interactions in Heilbronner's photoelectron studies). But theory has no problem in resolving mechanisms. One can calculate the contribution of each physical factor, or if all factors are already in the calculation one can throw away certain matrix elements, keep others, thus manipulating the theory to isolate the separate effects.

- Theory can simplify. The strong dictates of thermodynamics often prevent the observation of the simplest version of a reaction type or of a molecule. Substituents may be modifying a reaction path very slightly, and yet the parent reaction with no substituents may be masked by an entirely different process. Theory is not hampered by the reactions observed and can, in fact, examine the simplest variant of a reaction. Of course, this is a curse as well as a blessing. Substituents may be what makes an important reaction go, but the theoretician has an innate tendency to throw away those perturbations and to idealize the problem to the soluble stage. On occasion the explicitly soluble may be totally impractical. The stage is set for a classical dialectical contest between the practical experimentalist and the oversimplifying theorist. Both will gain if they persist in their struggle.

Though theoretical chemistry is not rich in its predictive successes, it is an essential component of our discipline. Chemistry cannot exist without theory, because chemistry is done by men, not machines. And men build their expertise, their intuition, on the organization of knowledge.

In my mind, the most important role of theory in chemistry is to provide a framework in which to think, to organize experimental knowledge. I picture chemistry as a living organism—an ever-expanding amorphous sphere with extensions along frontier areas, capable of movement, progress, that may be lightning fast and excruciatingly slow. I see theory as a kind of primitive nervous system of this organism, needed to organize the frontier information, to connect it to the accumulated store of knowledge, to communicate among the myriad advancing areas. The cues for further action that this nerve system gives need not be 100% correct—how could they be? Only a slight bias for the correct option, when a million decisions are to be made, endows the organism with the statistical equivalent of an intuition.

The peculiar feature of theory is that it generally trails experiment, and yet experiment cannot survive without it.

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The most lively and productive extension of our organism has a leading edge of experiment. Behind it lies a rich, interwoven mass of theoretical and experimental knowledge. Without the framework of theory, however primitive, chemists cannot discuss their experimental results. And without theory to organize the facts, to provide an intuition for the next experiment, the mass of facts quickly accumulates to smother the investigative urge of the experimenter.

If we examine the history of chemistry, we find ourselves, for good reasons, considering the achievements of individual men. Most of these men were or are experimentalists. And yet if we attempt to analyze what made their contribution great, we find that it is neither a single stupendous discovery nor a mass of thousands of unconnected experiments. The lasting work of good chemists is characterized by vision, breadth, scope, an ability to gather in diverse experimental facts and unite them into a coherent whole. Whether it is recognized explicitly as such or not, the organizing principle of such great achievements is some kind of theory.

There is another aspect of theory, not independent of the framework role discussed above. There is an innate psychological factor that makes a man rejoice in understanding a phenomenon. Just as in the very chemical pleasure of making a molecule that was not on earth before, there is, of course, a selfish, exclusive, psychological component in this—the self-gratification phenomenon. I believe, however, that there is more to man than that, that the search for understanding is uniquely human. Spinoza wrote, "To understand is the absolute virtue of the mind." Theory is a search for understanding.

Understanding is not synonymous with predictability. Perhaps complete predictability constitutes complete understanding. But we have not achieved that divine state, the deductionist's paradise, and we labor in pursuit of

partial understanding. Here there is a possibility of a division among theorists, for it seems that we are fated by our mathematical tools to make a choice between complete predictability of observables for individual molecules (that ideal, completely correlated, *ab initio* wave function for H_2O) and incomplete predictability but qualitative understanding of the mechanisms that make a related group of molecules behave in a certain way (the Walsh diagram that tells us which XH_2 molecule should be linear, which one bent). There is a matter of scientific taste here, but my personal choice is for the latter. The understanding of a trend seems inherently more chemical. Molecules show regular properties, and it is this first organizational feature in the treatment of experimental facts that one wants to understand. It is simply so much fun to think about what the next, yet unmade, molecule in a series might do. To me an observable perfectly calculated is a marvelous thing, but in a way it closes a door. The discovery and understanding of a regularity opens many doors—what will the extremes do, how can one manipulate the trend, modify it, turn it upside down?

I'm certain that 100 years from now people will think theoretically in different ways. Valence theory is ephemeral. Yet, I feel not at all bothered by the knowledge that what I have to say concerning, for instance, the reasons for the orientational preference of an ethylene in the equatorial site of a trigonal bipyramid d^8 transition metal complex will not last the test of time. My satisfaction is finding that somewhere in the marvelous chemical literature reposes a test of my highly specialized prediction, or that I can influence a friend or even a stranger thousands of miles away who reads my article to make the molecule that can test my idea. My pleasure is in the existential interaction of theory and experiment, the unexpected connection perceived, the give and take of predictions verified and disproved, of neat experiments stimulated.