

PRIMARY DOCUMENTS

Johannes Wislicenus, "Concerning the Position of Atoms in Space: An Answer to W. Lossen's Question"

*Translated from Berichte, 1888, 21, 581-585
by Peter J. Ramberg*

In the final issue of last year's *Berichte*, W. Lossen publicly challenged van't Hoff and me to reveal our views concerning the nature of affinity units on the basis of our geometrical ideas, by asserting that the question of the position of affinity units in space must be considered *before*, and not *after*, addressing the question of the position of atoms in space and, above all, a definition of affinity unit must be given" (1).

I agree completely with Lossen that our considerations about the configuration of molecules exclude the assumption that atoms may be "material points." One cannot avoid imagining them as spatial objects, thereby transferring the location of the units of chemical effect [Wirkungseinheiten] on multi-valent elementary atoms into different regions of these spatial objects. In principle, this idea is in no way hindered by difficulties, provided we conceive the so-called elementary atoms not as atoms in a strict sense, but as composed of groups of still more fundamental atoms [Urelementaratom] of a simpler sort—similar to the more compound radicals at more complex levels.

This notion is, however, neither peculiar nor new. The majority of chemists interested in this question might well share it today. Considering the proof of periodicity in the relationship between the properties and weights of the elementary atoms, in which they resemble by decisive analogy both the compound radicals of organic compounds, and, in their periods, to the homologous and heterogeneous series of the latter, the complexity of elementary atoms is certainly more probable than their simplicity. This view is also supported by physics, especially by the most recent research in spectroscopy.

While recognizing that Lossen's request is justified in general, I must, on the other hand, decisively reject the point raised by the above statement. Exactly the opposite: only *after* the spatial arrangement of elementary atoms in molecules is determined, and not *before*, is it possible to consider Lossen's question seriously. As I have shown in my paper "On the Spatial Arrangement of Atoms in Organic Molecules" (2) and will shortly show in detail on the basis of experimental research, the question concerning the spatial arrangement of atoms is

accessible to experimental test. On the basis of recently concluded investigations, I have convinced myself and am thoroughly satisfied that it also passes these tests. As a result, the initially purely hypothetical assumption is reinforced, that the independent rotation between two mutually monovalently bound carbon atom systems is prevented whenever they enter into a divalent linkage [zweiwertige Verkuppelung] (3). These facts now give quite important evidence for the existence of a double bond between neighboring carbon atoms and also for their corporeality.

For the moment, however, investigations on the configuration of molecules offer the only way to reach conclusions about the form of elementary atoms and the spatial distribution of their spheres of influence [Wirkungssphären], designated as affinity units. Or does Lossen know another way? The things that we chemists manipulate are chemical molecules—only from *their* properties, through *inductive* inferences, have we acquired our knowledge about the properties of the elementary atoms. As long as we had only extremely limited knowledge about the properties of molecules, each inductive conclusion remained uncertain, and the speculations based on them, the *deductive* conclusions, remained deficient, even sometimes positively wrong.

The empirical study of the quantitative compositional relationships of chemical compounds led initially to the law of multiple proportions and then towards a new scientific atomism. The efforts made towards determination of the atomic weights, however, remained for a long time very unsatisfactory and controversial—at least as far as their true values were concerned—until—and this is predominantly the accomplishment of organic chemistry—truly comparable molecular weights could be ascertained for a large number of chemical compounds. Only from these molecular magnitudes were actually comparable atomic weights derived. Furthermore, after establishment of the atomic weights, the study of molecular composition resulted in the concept of valence, out of which, again only from the study of compound molecules [Verbindungsmoleküle], emerged the law of atomic linkage, and so forth. The empirical elucidation of the way in which atoms are spatially distributed in molecules will follow an entirely similar course, and this distribution will in turn yield clues about the geometric properties of the atoms of our elements. These properties will become all the more certain as our empirically gained knowledge about the geometric properties of molecules becomes more certain.

Today these conceptions about atomic form and the positions of their areas of chemical effect [Wirkungs-

zonen] can only remain very uncertain. We certainly have the need, of course, to form such conceptions, but must always bear in mind that all theories derived by way of speculation are still very unreliable. Of course, that should not prevent us from forming deductive conclusions. We must certainly be aware, however, that the value of these conclusions, in combination with inductively derived knowledge, lies in the fact that they direct the imagination along the new avenues of empirical research which it seeks. Sometimes, indeed, the imagination is led along false paths, but between these paths lie the routes leading to the goal.

In view of today's situation of compelling facts that are urgent to such studies, exact scientists certainly have the right to occupy themselves with the further empirical pursuit of inevitable (4) hypotheses concerning the spatial distribution of atoms within the molecule, without immediately discussing the deeper lying reasons for each relationship before the general public. On the other hand, he who desires more speculation certainly has the right to make the attempt, and to provide the paths of *his* thoughts to his contemporaries; he is not, however, entitled to demand the same from others.

So I could, of course, simply reject Lossen's statement of "before" and "after" and his request, by challenging him with full conviction that a speculative discussion of the relationships in question could bring more clarity than the certain, admittedly long, path of empirical research. Nevertheless, once challenged, I want to grant the wish of my honored colleague at least as far as I can with good conscience.

Thus, I believe it more probable that atoms are spatial objects composed of atoms of simpler elements [Urelementen], than of point-like carriers of energy. Therefore, it appears more probable to me than any other assumption, that the atoms may be compared to compound radicals, and that like them, their affinity units are located in certain parts of those atoms from which they act.

I believe it possible that with time we shall not only obtain certain ideas about the form of elementary atoms, but also about the position of the relative locations of their spheres of influence [Wirkungssphären], and also that we will ultimately elucidate the actual essence of the specific chemical form of potential energy.

I do not consider it impossible that a carbon atom may be an object whose form more or less [perhaps quite closely] resembles a regular tetrahedron; further, it is not impossible that the causes of every effect that actually manifests itself in the affinity unit concentrate

themselves in the corners of this tetrahedral object, and for analogous reasons, would possibly be similar to the electrical effect of an electrically charged metal tetrahedron (5). The actual carrier of this energy would ultimately be the primitive atoms [Uratome], just like the chemical energy of compound radicals undoubtedly is a product of the inherent energy of the elementary atoms within them.

These are, more or less, the ideas that I myself have already had for some time about the very question imposed on me by Lossen. By no means do I attach to them the value of scientific conviction and I prefer not to be "nailed down" to them. Nor do I wish to get involved in a purely speculative discussion, since I voice these thoughts here not only in free personal discourse—how could it be otherwise—but from a position which above all is dedicated to the results of exact science. Such discussions are actually valuable only for their critical aspects if they are not conducted quite strictly on the basis of sufficient facts: they can contribute towards clarification in a negative sense, by detecting the untenability of expressed theories or theoretical opinions, but they are able to bring us no further in a positive direction. At the most they succeed now and then in bringing a precise expression to factually derived knowledge. From this standpoint I also judged, for example, Lossen's earlier longer paper "On the Distribution of Atoms in the Molecule" (6), without, however, agreeing with all the points contained in his critique.

This has turned out longer than I originally intended. It may remain unabbreviated as a critique of Lossen's viewpoint in relation to "before" and "after." Lossen's claim is the proof that even we chemists do good from time to time by making completely clear to us the path that we have to follow for the development of concepts. We must take actual obtained knowledge, and not a justifiable desire, as the starting point for our advancement into the unknown.

In conclusion, with full conviction of its greater justification, I oppose Lossen's thesis once again with the antithetical statement: not *before*, but only *after* establishment of the spatial position of elementary atoms in a compound's molecules can we address the question concerning the position of affinity units in the spatial objects of elementary atoms with the prospect of success. Ultimately these considerations can likely also lead to a satisfactory definition of affinity units.

Leipzig, Early February 1888
(Received on February 16)

REFERENCES AND NOTES

1. W. Lossen, "Ueber die Lage der Atome im Raum," *Ber.*, **1887**, *20*, 3306-3310 on p. 3309, second paragraph.
2. J. Wislicenus, "Über die räumliche Anordnung der Atome in organischen Molekulan und ihre Bestimmung in geometrisch-isomeren ungesättigten Verbindungen," *Abhandlungen der mathematisch-physischen Classe der königlichen sächsischen Gesellschaft der Wissenschaften*, **1887**, *14*, 1-77.
3. Wislicenus is referring here to the transformation from Fig. 1(A) to Fig. 1(B) in the preceding commentary, that is, to an elimination reaction. The rotation about the carbon-carbon bond is removed in these transformations. The idea of free rotation was originally van't Hoff's, but Wislicenus developed the idea to a much greater extent in his 1887 essay.
4. The German word here is "unabweisbare," which usually translates as "irrefutable." The sentence, however, is not entirely clear with "irrefutable." The reviewer of the first draft of this translation suggested that "unabweisbare" was a misprint or misreading of Wislicenus' manuscript for "unbeweisbare," which would translate literally as "unprovable" or perhaps "indemonstrable." Neither of these alternatives seems better than "irrefutable," however. "Inevitable" seems to fit better with the content of the paper and of the sentence itself.
5. A fragment of this sentence in German reads as follows: "... nicht für unmöglich ferner, daß die Ursachen jener Wirkungen, welche in den 'Affinitätseinheiten' zur tatsächlichen Erscheinung kommen, sich in den Ecken dieses tetraëdrischen Gebildes concentrieren..." Farrar translates this fragment as follows: "... that the causes of the effects which enter our actual experience as affinity units might be concentrated in the angles of the tetrahedron..." This, I believe, is a mistranslation, since there is no reference to "our" experience in Wislicenus' sentence. Farrar's subsequent footnote is therefore also inaccurate. Wislicenus was not a positivist, but he made no claims of actually seeing an affinity unit. See W. V. Farrar, "'Chemistry in Space' and the Complex Atom," *Brit. J. Hist. Sci.*, **1968**, *4*, 65-67, p. 66.
6. W. Lossen, "Ueber die Vertheilung der Atome in der Molekel," *Ann.*, **1880**, *204*, 265-364.