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Science in the Making and the Philosophy of Science : the Case of the Law of Constant Composition Proportions

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SCIENCE IN THE MAKING AND THE PHILOSOPHY OF SCIENCE: THE CASE OF THE LAW OF CONSTANT COMPOSITION PROPORTIONS

I. INTRODUCTION

Linus Pauling, an outstanding chemist of our times and Nobel Prize winner for chemistry in 1954, when asked if he had any precept for scientific discoveries, said "I do, and a simple one, always have many ideas and reject those that prove to be false."[1] This belief readily brings to mind the spirit of the nowadays so popular Popperian philosophy of science. This latter distinguishes in the process of inquiry two opposite aspects, or even two successive stages. The first is the initial stage of finding a new concept, while the second is a critical analysis and appraisal of the hypotheses and the refutation of the ideas that have proved to be worthless.[2] Since, characteristically enough, modern philosophy of science has tended to focus on the latter of these two stages only,[3] countless studies have been devoted precisely to the principles apparently governing that "rejection of ideas that prove to be false", to use Pauling's phrasing again. But a philosopher of science will be able to add very little to what Pauling said about the first stage, that is about simply having many ideas!

Such a pattern of research interests is by no means accidental. As Popper himself has argued in his *Logic of Scientific Discovery*, a logical reconstruction of the processes conducive to new solutions is neither possible nor necessary. No such possibility exists, Popper argues in support of his opinion, because each such process has an irrational element in it. There is no need to do that either because a new scientific result is arrived on any logical path of reasoning. New knowledge emerges by virtue of specific spiritual acts materializing in the individual's consciousness

[1] W. Osiatyński, *Zrozumieć świat. Rozmowy z uczonymi amerykańskimi*, Warszawa, 1977.
[2] Cf. K. R. Popper, *The Logic of Scientific Discovery*, London, 1974 p. 31.
[3] I mean here specifically its most basic, "orthodox" trend that had taken its shape under the influence of the tradition of logical positivism.

or subconsciousness, and their investigation should accordingly be entrusted to psychologists. If we employ the tools of logic alone we shall be unable to say anything at all about the process of formation of new concepts.

To conclude this brief presentation of the view, which will be submitted to a critical analysis in what follows, let us make one more point. Popper, but also Carnap, Reichenbach and other scholars of a similar frame of mind, took antipsychologism as their starting-point. Those authors were aware of the need to distinguish between what can and should be studied by means of the specific instruments of logic and what falls into the research domain of psychology. Yet that need does not necessarily entail the consequence that the philosophy of science should purge its field of vision of the entire process of emergence of new knowledge in all its aspects, a consequence that seems to have been a by-product of that attitude. The fact that in the emergence of new knowledge creative acts materializing in the individual's consciousness or subconsciousness play some role by themselves does not imply that that process exhausts itself fully in such acts and that, accordingly, it is the psychology of science alone that is competent to tackle that problem. On the contrary, we must, as the ancient saying goes, give God what is God's and the emperor what the emperor's is, for it is no one else but the philosopher of science who, applying the tools specific to his discipline, will be able to bring up the typical characteristics of discovery-generating situations, of objective situations that shape a new knowledge.[4]

The orthodox philosophy of science, which so typically tends to relinquish the study of processes of emergence of new knowledge, strives to combine that relinquishment with curiosity regarding the dynamics of science, the general regularities of the development of scientific knowledge and the concomitant historical orientation of its inquiries. According to I. Lakatos, the philosophy of science should furnish tools relevant to a rational reconstruction of the history of science and its performance is the better the higher the number of historical events it can account for, the greater the number of facts it can situate in the "internal history", to use Lakatos' own term.[5] It is easy to notice the consequence-laden disharmony between the two mentioned programmatic features of the contemporary philosophy of science. There seems to be no way to cope successfully with the task of rationalizing the historical process of development of science as long as one adhers to the opinion that the philosophy of science can and should employ its specific instruments to study only established knowledge. The endorsement of that opinion has so far landed the contemporary philosophy of science in full impotence and helplessness in the face of a number of phenomena connected with the formation of new knowl-

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[4] For a discussion of this point see E. Pietruska-Madej, *Od logiki wiedzy ku 'logice niewiedzy'. Nauka jako przedmiot badań logicznych w opinii Reichenbacha, Hansona i Poppera*, "Studia Filozoficzne" 5 (1980).

[5] I. Lakatos, *History of Science and Its Rational Reconstruction*, in: *Boston Studies in the Philosophy of Science*, VIII.

edge, phenomena that have too often been pointed out by historians to be recognized as fringe manifestations in science.

To substantiate this contention with an example let us recall a statement by a contemporary historian of science, a statement so very typical that analogous ones can be freely supplied. D. J. Solla Price says that had Boyle not discovered his law, then somebody else would have had to do that. And, indeed, Mariotte did it. Had Planck not discovered his constant, we would hare told of Joe Blogg's constant. One can get the impression as though every fact and every theory were lying waiting to be discovered. More still, when their time has come they often get discovered by several competing people.[6] But let us ask the philosopher of science how to explain facts mentioned by Solla Price. How to interpret the mentioned necessity of the given discovery to be made at any given historical moment? How to account for the intuition that "the time for the discovery has come"? We shall get no answer. The same historian adds that the fruit on the tree of knowledge gets ripe when its proper time has come. Man, according to Price, has relatively poor possibilities of influencing the rate or direction of research. Thus, for instance, it is hard to explain why, in spite of heroic efforts and huge expenditures, we have failed to attain a level of knowledge that would enable us to cure cancer.[7]

In this way, scientific-historical readings are bound to raise questions with regard to which the philosophy of science is helpless and whose solution must not be expected from the psychologist: Why does science sometimes manage to withstand genial insights only to rediscover the same idea years later? Why does it at other times explode with a series of stimultaneous discoveries? How to account for failures of premature discoveries, how to distinguish between discovery in the subjective, psychological sense and scientific discovery proper? It seems that the key to the solution of this type of problems is to be sought in penetrating analyses of certain objective features of the context within which new solutions crop up, the features of the discovery-generating situation that decides about the emergence and the fate of scientific ideas. The understanding of a discovery implies, then, an identification of the mutual logical relations between the individual elements of the knowledge of a given period in connection with the targets and regulative ideas of scientific inquiry typical of that period. What is meant here are the logical relations that objectively exist within the given system of knowledge, not infrequently beyond the awareness of individual researchers, beyond what has been explicitly articulated by then. In performing such a reconstruction of the objective situation science is bound to take recourse to the specific tools of philosophy of science. Hence the expectation that it is precisely philosophy of science that, as a discipline, is capable of grasping the general features of such a state of science

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[6] D. J. Solla Price, *Czym się różni nauka od techniki*, „Kwartalnik Historii Nauki i Techniki" 1 (1973), p. 7.

[7] Ibid., p. 10.

which historians of science rather inaccurately describe as the situation having matured for a scientific discovery to be made.

Historians seem much more often than philosophers to realize the significance of those "situations of ripe apples" that are bound to drop at the slightest waft. But the occurrence of such situations is of fundamental significance precisely from the standpoint of philosophy of science. It is such situations that substantiate the leading contention of this study: the process of formation new knowledge has also an extra-psychological dimension. It is precisely this circumstance that, in contrast to traditional views, enables philosophy of science to deal not only with the assessment and critique of established knowledge but also with the study of the phase conducive to its articulation. Thus philosophy of science would have an additional task in developing a theoretical model of the discovery-generating situation, a situation from which new scientific concepts emerge. The reconstruction of objectively existing logical relations between the individual components of the knowledge of the given period seems to be of crucial significance to the understanding of the process of development of new ideas. It is precisely those relations that delineate the "field of manœuvering" to science and determine not only the problems themselves but also the space for possible solutions. The creative subjective act by which a new idea turns up it all but suspended in a vacuum: on the contrary, it is bound to find its place in precisely that space.

Before we proceed to some philosophical generalizations let us analyse some concrete discovery-bearing situations that actually occurred in science. The present essay is just such a tentative analysis, based on materials furnished by historians of science and aimed at bringing up the non-psychological aspects of one of the major discoveries in chemistry.

II. A CASE ANALYSIS: THE LAW OF CONSTANT COMPOSITION (DEFINITE PROPORTIONS)

The year 1799 is regarded as that of the discovery of one of the most significant laws of classical chemistry, of a law that has become the foundation of stoichiometry. Ten years after Lavoisier had published his *Traité élémentaire de chimie*, an encyclopedia of the new chemistry, J. L. Proust published a study of copper compounds in which he announced the law of definite proportions, also referred to as the law of constant composition.[8] According to that law, each chemical compound has fixed composition by weight of its elements. Proust's well-known empirical analyses consisted in showing by quantitative methods that the component elements in copper salts are not combined in fully arbitrary proportions but, on the contrary, their respective masses must remain in a definite mutual relation (specific to the given compound). In other words, the quantitative relations between the

[8] Some authors point out that that law was formulated by Proust already in 1797 in an essay in which he showed that iron reacts with oxygen to yield two compounds each of constant proportions. Cf. J. R. Partington, *A History of Chemistry*, London, 1962, vol. III, p. 647.

components of the given compound are identical independently of the conditions that occurred during the process of its formation (temperature, excess of one of the reagents etc.) and regardless of whether the compound was produced artificially in a laboratory or naturally, that is, by the forces of Nature. Let it be added that in stating the above Proust did not confine himself to the study of copper compounds (nor, incidentally, did so the other scientists interested in Proust's finding).

Thus, every chemical compound is, to use Proust's own phrasing, a "privileged product of Nature". Nature favours certain proportions while preventing the integration of elements at arbitrary relations. „She [Nature] never creates even between the hands of man, otherwise than balance in hand".[9] The forces binding the elements of a chemical compound display, as Proust sees it, a peculiar persistence. Less persistent are, for instance, the forces acting in solutions. This is why in solutions, just as in all mixtures, any arbitrary proportions are possible; constancy of compositoni does not hold.

By way of this distinction Proust defined the chemical compound as a specific construct that retains definite proportions in contrast to all kinds of mixtures. This statement naturally had very significant consequences. All that does not conform to the law of definite proportions: was ruled out from the field of interest of the chemist. Thenceforward chemists focus their research interest on chemical compounds as Proust understood them. The object of chemical research was defined as the set of all things that preserve definite proportions. Proust's law furthermore asserts that the elements of that set maintain definite proportions. This gave rise to a self-fulfilling system of beliefs: Whatever might have challenged Proust's law ceased to be of interest to chemists for a long time. Proust's law consolidated its position and, while its importance to stoichiometry is impossible to overrate, we must also realize that for a long time it diverted the attention of chemists from phenomena and processes in which definite proportions are not preserved.[10] Meanwhile, however, Dalton came up with his atomic theory that accounted for the law of definite proportions[11] and proved to be an unrelinquishable element of the new chemistry with its dynamic and fertile programme. After these indispensable remarks let us now proceed to the main question of this essay. We shall take a look at the factors conducive to the discovery of the law of definite proportions.

1. THE PROGRAMME OF QUANTITATIVE CHEMISTRY

Down to the last quarter of the 18th century chemistry had dealt mainly with the qualitative composition of substances. A huge empirical material had been collected which permitted to attribute definite qualitative compositions to the parti-

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[9] Quoted after J. R. Partington, op. cit., p. 650.

[10] Cf. W. I. Kuznetsov, *Podstawowe prawa chemii — ewolucja poglądów*, Warszawa, 1950, pp. 157 f.

[11] One typical view in this line reads for instance: "If matter is composed of atoms, then the law of constant proportions is a natural consequence of its structure" I. Asimow, *Krótka historia chemii*, Warszawa, 1970, p. 77.

cular chemical individuals (these latter were identified in view of a definite set of physical and chemical features). While nowadays the chemist identifies a compound by studying its qualitative and quantitative composition and establishing the specific bonding of the elements constituting the molecule, at that time he satisfied himself with an analysis of the qualitative composition. For the underlying assumption then was that it is precisely that qualitative composition that is responsible for the physical and chemical properties of the given compound. That common belief even led chemists then to attribute specific principles as carriers of definite properties to individual chemical substances. The procedure of attributing a hypothetical common component as carrier of properties to all substances that have the given property was widely applied in chemistry, to mention but the principles of metallicity, the phlogiston as the principle of combustibility, or the *acidum pingue* as the principle of alkalicity.[12] Even in the writings of an apparently modern chemist, Lavoisier, certain traces of that mode of thinking are clearly detectable.[13]

But at Lavoisier's time chemistry became a quantitative science.[14] The above way of identifying chemical individuals which consisted in describing their qualitative comdosition alone now proves to be insufficient; it is inconsistent with the Lavoisierian programme of developing quantitative chemistry. That evident dissonance is an essential factor conductive to the situation that generated Proust's discovery. To overcome that dissonance it was necessary to introduce procedures for the study of the quantitative composition of substances. This latter, in turn, led to determining weight relations between particular components of the chemical compound.

Although in this we clearly face the relations between the particular components of the knowledge of the given period and the given discipline, it would be difficult to explain in methodological categories what the above-mentioned dissonance consisted in. We cannot justifiably talk of any logical contradiction between those components, or of anomalies; either of these terms would as though be too "strong". The situation studied essentially presents a disharmony between the particular components of the knowledge; the disharmony due to the circumstance that some definite idea has not been consistently implemented throughout the given discipline of science. In any case, however, that dissonance is one of the significant components of the discovery-generating situation.

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[12] Concerning this topic cf. E. Pietruska-Madej, *Metodologiczne problemy rewolucji chemicznej*, Warszawa, 1975, p. 183.

[13] See E. Meyerson, *Identité et réalité*, Paris, 1920, p. 370.

[14] Lavoisier's teacher G. F. Rouelle, who advocated his own theory relying on the ideas of Stahl, was well aware of the significance of scales as a chemical instrument. Cf. R. Rappaport, *G. F. Rouelle — An Eighteenth-Century Chemist and Teacher*, "Chymia" 6 (1960), pp. 68 f. But Lavoisier was the first to use scales to measure gases and to formulate the law of conservation of mass, which disclosed the importance of measurement. Thereby he also demonstrated the effectiveness of physical methods in chemistry. Rouelle still thought that those who carried out measurments were "only" physicists and did not deserve to be called genuine chemists (cf. R. Rappaport, *Rouelle and Stahl — The Phlogistic Revolution in France*, "Chymia" 7 (1961), p. 73).

2. THE DEVELOPMENT OF CHEMICAL STATICS

The call to look for quantitative characteristics concerned the question of how to practice chemistry rather than what should primarily be studied. This latter issue, which used to be given a specific solution in every period of development of science, implies in fact two different questions. First, we should ask which of the problems is recognized as fundamental at the given stage of development of a given discipline, and, secondly, which of them is recognized as feasible enough for research purposes.

In view of the huge variety of chemical phenomena and for want of a clear line of distinction between those pertaining to chemistry and those belonging to the domain of physics, the above two questions were at Proust's time of paramount importance.

The answers Proust implicitly gave to those questions fully accorded with the ideas of Lavoisier. For this reason, when trying to follow up the maturing of chemistry to Proust's discovery of the law of fixed proportions, we should perhaps pay particular attention to some points of Lavoisier's programme.

Generally we can say that the chemistry of Lavoisier's time seems to have been advancing in two trends.[15] The first trend betokens an interest in the problems of chemical substance: the study of its quantitative and qualitative composition is linked with the analysis of the characteristics of the given substance which manifest themselves in reactions described in a specific manner. Namely, what was investigated were: reaction substrates, its products, and their comparison permitted definite conclusions. That research trend, which essentially consisted in the statical investigations of the chemical compound, can perhaps be opposed to another trend, that implying interest in the course of the chemical process itself. The study of the mode of chemical reactions required the analysis of chemical forces, of that "love" and "hate" of ancient scientists, of attraction or repulsion or of affinity—if we may here apply the terms that came to be used later.

The fact that these two possibilities do exist faces one with a necessity of choice. In setting out to conduct a concrete investigation the chemist had to decide which group of problems should be tackled first. Thus, in keeping with what has been said at the beginning of this section, one had to decide which of the groups was currently recognized as more fundamental and which of them was mainly for technical reasons, more accessible to research.

The overwhelming reformative impact of Lavoisier's work on the development of chemistry encourages us to seek out in it the ideas that would facilitate the indispensable appraisals and choice, the more so that but a few years elapsed from Lavoisier's death to Proust's explicit publication of the previously formulated law.

What are the pertinent suggestions therefore that Lavoisier's *Traité élémentaire de chimie* may imply? Its introductory chapter indicates a number of general ideas

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[15] This of course is not to say that the chemists of the time can be reasonably divided into two clearly disjoint classes of scientists each dealing with utterly different problems. What is meant is the predominance of a definite group of problems then.

regarding the mode of practising chemistry. Lavoisier complains that all too often had physicists abandoned themselves to guesswork rather than worked by inferences and that their guesses passed down from generation to generation and supported by the authority of their advocates gradually acquired the status of fundamental truths. There is but one way to escape such risky speculations and this is by submitting scientific reasoning to rigorous rules, simplifying it, and steadily putting the results to the tests of experiment. Reasoning alone may lead us astray, in contrast to facts, which are gifts of Nature and cannot delude us. The epistemological theories of the French Englightenment[16] keep cropping up in Lavoisier's argument; incidentally, he quotes literally from Condillac. Thus, the author of the *Traïté élémentaire* continues, where facts are mute, speculative artefacts are good for nothing. In virtue of this assumption, Lavoisier resolves not to tackle the problem of chemical affinity in his book. To be true, he emphasizes, chemical affinity had been studied by many scientists (Geoffroy, Gellert, Bergman, Scheele, Morveau), but the facts established so far are neither accurate nor certain enough to be recognized as a sufficiently reliable foundation for the science of affinities. Moreover, Lavoisier suggests that only in the future, after chemistry has solved more fundamental and more directly accessible issues, will it be possible to accumulate a knowledge of chemical affinities and to turn it into a discipline of the exact sciences.[17]

For the time being, though, Lavoisier discards the problem of affinity, of the nature of forces acting in the course of chemical reactions, as a secondary issue to which chemistry has not matured yet; this is dissimilar to the problem of quantitative relations between reaction substrates and products and the quantitative characteristics of the composition of particular chemical compounds.

From the point of view of analysis of the motifs conducive to the discovery of the law of definite proportions, this circumstance is of capital significance. It was one of the essential components of the situation out of which that discovery arose. Proust's work is evidence of this. He geared his own investigations exactly in the manner described above. It was precisely the study of the quantitative characteristics of individual chemical substances and of relations between the quantities of the particular components of a given compound that enabled Proust to present the law of definite proportions in the way we know it today. More still, how strongly Proust was impressed by Lavoisier's arguments can be read out from the manner itself in which he wrote on topics connected with the investigation of chemical forces. What was important to Proust developing his concept was how to distinguish between chemical solutions and compounds. Proust suggested that the force acting in solution essentially differ from those acting in compounds: the latter are stronger than the former, which accounts for stronger bonding. Yet Proust refrains from

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[16] Concerning the attitude of Lavoisier to the philosophical tradition see E. Pietruska-Madej *Metodologiczne problemy...*, pp. 132 f.

[17] A. Lavoisier, *Traité élémentaire de chimie*, Paris, 1793, pp. 8, 9.

any quick answer to the question concerning the characteristics of those chemical forces; he candidly admits that he is afraid of erring in a domain that has not yet been illuminated enough by facts.[18]

To recapitulate what has been said so far, let us again point to another feature of the situation in science that had generated the discovery of definite proportions. What I have in mind are the criteria of appraisal and choice of what should be investigated first established by Lavoisier. I say "should" because the chemist of those times was to focus attention, firstly, on fundamental problems that theory suggested to be tackled before others and, secondly, on problems that were indeed technically feasible for chemical investigation.

3. EMPIRICAL PREMISES

(a) With the advance of analytical techniques, more and more substances (previously known as a complex of definite physicochemical properties) were given their qualitative characteristics by indicating and distinguishing their constituent elements. As mentioned before, the establishment of qualitative composition had seemed to be the foundation for the chemical identification of any substance. We must, however, recall the assumption underlying that belief. It was assumed that all substances that were identical—in the sense of displaying the chemical and physical properties the chemist of that time was interested in—must be composed of identical constituent elements. Accordingly, the common belief then was that the establishment of composition was necessary as a significant indicator of the properties substances have.

One essential element of the situation in the chemistry of Proust's times conducive to the discovery of the law of definite proportions was that the qualitative composition of the substance proved to have been totally insufficient as an indicator of the substance's properties. Experiments then conducted demonstrated that the same elements may constitute different chemical compounds, or, to put it differently, that substances of differing physical and chemical properties may have the same qualitative composition.

Examples of such substances had been known earlier but now their number was rising, and with the expansion of qualitative methods these latter were gradually moving to the focus of interest in chemistry.

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[18] Concerning this cf. J. R. Partington, op. cit., p. 650. Of course some of those, previously close, collaborators of Lavoisier later took to the investigation of chemical reactions in their dynamics. But we can cite their history by way of additional argument for the view that the situation of science at the time was still too immature to solve that kind of problems. Thus, for instance, Berthollet's studies, which were at that time much ahead towards the later discovery of the law of action of masses, failed then to be properly appreciated. It was an unfortunate circumstance to Berthollet that his dispute with Proust related to a domain in which the law of constant composition is indeed valid. There were no theoretical, but above all no technical possibilities for studying the chemical bonds that do not conform to Proust's law while fulfilling the conditions of Berthollet. The history of discovery of chemical phenomena not fulfilling the classical laws of stoichiometry is discussed by W. I. Kuznetsov, op. cit., chap. III.

It should perhaps be pointed out in this connection that it had already been Lavoisier who recognized that when substances display identical qualitative compositions it is the mutual quantitative relations of their constituent elements that are decisive about such substances being different from one another. This was, in his concept, true for instance of the property of the strength of acids. In virtue of his assumption that oxygen is an acid-forming element Lavoisier explained that the strength of any acid depended on the amount of oxygen bound in it. Incidentally, the belief that not only qualitative composition but also quantitative properties of a chemical substance are significant found its expression in the rules for developing chemical nomenclature worked out by Lavoisier and his co-workers[19]. Suffice it to mention designations such as *l'acide sulfurique*, *l'acide sulfureux* or designations of salts indicative not only of the respective qualitative composition but of the saturation degree as well.

The difference in degree of acid and base neutralization was observed by J. B. Richter. Understandably enough, then, it was Richter—the author of the term stoichiometry to denote the science of determining the quantitative laws of chemical bonds and a scientist who viewed chemistry as a domain of applied mathematics[20]—that noticed that some metals (such as iron or mercury) may constitute compounds with oxygen at two different proportions. This, let it be stressed, is a conclusion almost identical with those formulated by Proust in 1797 (in his *Recherches sur le Bleu de Prusse*), two years before the date commonly accepted as that of the discovery of the law of definite proportions. Proust showed then that in uniting with oxygen iron succumbs to the law of Nature that determines fixed proportions for a metal and oxygen to form two different compounds. According to J. R. Partington, that discovery of Proust's had been anticipated by Richter. While this opinion is not shared by all historians,[21] let us observe how the programme of quantitative chemistry had been conducive to the discovery of the law of definite proportions. Once it had been found that identical elements may constitute unions of differing properties, that is, different chemical compounds, it was easy to gather that the chemist must not content himself with establishing qualitative compositions alone. The fact that the same qualitative composition corresponds to different substances suggested the idea of searching for some additional characteristics of the given compound that are responsible for that difference. The new quantitative chemistry naturally implies that what does play some role in that area are the proportions at which the constituent elements unite. In fact, conducive to that idea were more than one element of the then contemporary chemistry, of which we have managed to mention but a few here.

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[19] A. Lavoisier, M. de Morveau, C. L. Berthollet, A. F. de Fourcroy, *Méthode de nomenclature chimique*, Paris, 1787.

[20] These ideas are put forward in the classical study by J. B. Richter, *Anfangsgründe der Stochyometrie*, vols I–III, Breslau, 1792–4.

[21] Cf. Partington, op. cit., pp. 675–8.

(b) Empiric chemistry noticed another kind of phenomena that might be indicative of the circumstance that the proportions at which the constituent elements of chemical compounds unite are anything but arbitrary. There had been descriptions of reactions in which the substrates involved reacted strongly with one another and yet some part of one of them remained unchanged.

One example of this phenomenon were the frequently described (Boyle, Priestley, Lavoisier and others) experiments with the "calcification" of a metal (for instance, tin) in a closed vessel. It had been noticed that if a greater amount of metal is taken into the reaction and passed below a closed bell-glass then, apart from the expected product ("calx" or oxide), some amount of pure metal remains too. If the metal is burnt in open air or under a bell-glass with a great amount of oxygen, then all of the metal is used up in the reaction.

Another example of that type of reactions was the reaction of neutralization which yielded, in addition to a neutral salt, a certain amount of acid or base. Further, it was significant that in some experiments neutral salt alone could be obtained. Such and similar observations could be indicative of quantitative proportions playing a significant role in the formation of chemical compounds. The law of conservation of mass had already made it clear that the totals of product masses and reaction substrates are equal. Hence, if that total of product masses admitted by the law is composed among others of part of the amount of the compound classed already at the substrates side, that extra amount can presumably be regarded as superfluous in the given reaction. In other words, if some of the substrates are introduced into the reaction in excess then it will be found among the reaction products in the "unreacted" state. Yet what does "in excess" mean here? The question why in some cases we find among the reaction products a certain amount of one of the reactants, sometimes a certain amount of another, and at other times we manage to obtain a pure product, was bound to emerge in that situation. In making the chemist perform detailed measurements it codetermined the field of his interest on the eve of the formulation of the law of definite proportions.

(c) Of the empirical premises making up the background from which the law of definite proportions of chemical compounds was to emerge, it is the result of studies on the reaction of neutralization that deserves special attention. Historians of chemistry are wont to write about those studies by way of presenting the reasons that spoke for the law of equivalents; yet those studies also seem to have been of significance on account of the formation of views concerning definite proportions of composition of compounds.

What seems particularly significant in this connection are of course the quantitative analyses of the neutralization reaction. K. F. Wenzel, R. Kirwan, or W. Homberg are but a few names of the many chemists who were interested in that problem. In his investigations of the alkalic properties of potash, Homberg noticed that in order to neutralize a definite amount of that compound he needed to use definite, rather than arbitrary, amounts of acids and that the relative quantities of the latter amounts to be used depended on the specific acid applied. Cavendish was another of the many chemists to study quantitative relations at which given bases can get fully neutralized by definite acids. Invariably the scientists found that those proportions are anything but arbitrary and that to obtain pure salts their amounts had to be selected accurately. Richter's investigations became the most renowned studies in this line. He was interested in the constancy of proportions between the masses of active acids and bases constituting a neutral salt. Richter presented a tabular compilation of such constant mutually equivalent quantities for individual acids and bases. His table had a general character: it could be used to determine the composition of still unknown salts to be made up from the reagents included in the table. Incidentally, Richter suggested that analogous rules are valid for other compounds (cf. for oxygen in oxides).[22]

Such and similar quantitative data may have invited the conclusion that since the forming compound "uses up" definite amounts of substrates then each compound must retain definite proportions by weight of its constituent components.

4. PRAGMATIC PREMISES

Chemistry that was interested in chemical statics and simultaneously fulfilling the postulate of quantitative investigations concentrated therefore on the study of quantitative relations between substrates and reaction products, of the quantitative contents of individual elements in compound, and the quantitative relations between the components of individual compounds. That type of research is of great importance both in practical applications and in analytical chemistry. In our endeavour to grasp the epistemological situation that generated the concept of the law of definite proportions let us try to realize exactly what that significant stoichiometric law implied for that type of research.

Let us imagine a fragment of the operation of analysis of some unknown chemical substance. For analytical purposes a reaction was performed using a known quantity of that substance from which x grams of the compound A_mB_n was obtained. For simplicity let us assume that A_mB_n is the only product of the reaction that contains the element A we are interested in as a component of our substance. The analyser's most immediate task will be to determine the amount x_A of element A in x grams of the compound A_mB_n. Knowing already the law of conservation of mass of individual elements in chemical reactions the analyst can infer that the same amount of that element was contained in the initial dose of the unknown compound. Yet his knowledge of the quantity x alone does not suffice to determine the quantity x_A. He must additionally perform the sometimes very complex analytical operations to determine the quantity x_A of element A in the obtained dose x of the compound A_mB_n. More still, analogous involved investigations of the same

[22] More precisely, Richter's table contains acid radicals and cations.

type have to be performed always and again whenever that compound $A_m B_n$ is found among the reaction products. Briefly, a chemist who is not yet familiar with the law of definite proportions must in each case determine anew the weight proportions between the elements constituting the now studied dose of any given compound. It would be unjustified to assume that for the given compound, in our case for $A_m B_n$, the m_A/m_B ratio is constant and thus determining it once would do once and for all. But if Proust's law which asserts the constancy of that ratio is assumed, one is relieved of the necessity of carrying out analogous measurements anew whenever one has to do with the compound $A_m B_n$. In our hypothetical exercise, it will do if we determine the quantity x (the mass of the compound $A_m B_n$ obtained) and know the x_A/x_B relation which always holds for the given compound, to determine very easily the quantity of element A in the known amount of the reaction product $A_m B_n (X \cdot (x_A/x_B)$. Next, making full use of the law of conservation of mass we find what amount of the studied element is contained in the known dose of the substance taken initially for analysis.

The simplification of the procedure thus secured is so considerable that it is perhaps justified to conjecture that the idea of constant composition must have been gradually suggesting itself to chemists interested in that type of processes. It must have been evident to the analyst that once the law of definite proportions should have proved to be valid a whole series of arduous analytical exercises might be skipped for some simple intellectual operations. Let us add that those simplifications comprised precisely the theoretical procedure and operations that after Lavoisier became the focal point of research interest to chemists and simultaneously were connected with the problem of applying chemical knowledge for practical purposes.

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In presenting some aspects of the situation of chemistry on the eve of the discovery of the law of definite proportions I wish to point to the following circumstance. That law had been emerging from chemical knowledge gradually, in what was a natural way, still before it got its explicit formulation and before the series of experimental investigations were carried out to answer the straightforward question whether or not chemical compounds display definite proportions of composition. For this is what analysis was leading to—the analysis of the individual components of chemical knowledge taken separately less so than the analysis of the interdependences, between them with a view to the rules and purposes of scientific research. The gradual maturation of chemistry toward that discovery consisted essentially in the formation of an appropriate constellation of premises, or, more precisely, of certain cues indicative of a so far unknown scientific truth. An additional argument in favour of that view is perhaps the fact that the idea of definite proportions of composition had, still before Proust made his discovery, been cropping up with increasing frequency by way of a tacit assumption. That this was indeed the case can be shown by an analysis of the chemical knowledge of the time as well as though logical reconstruction of the chemist's research procedure.

5. THE LAW OF DEFINITE PROPORTIONS AS A COMPONENT OF UNARTICULATED KNOWLEDGE

The idea of definite proportions of composition in chemical compounds can be traced as a tacit assumption, if not phrased explicitly, in works by many outstanding chemists. This is of course true of scientists who attached proper significance to quantitative investigations. While analysing any given chemical substance they did not satisfy themselves with the determination of qualitative composition but also studied the quantitative proportions between the individual components making up the respective compounds. Once they had established those proportions beyond any doubt they did not repeat their investigations. They assumed tacitly that the quantitative proportions of the components in the same compound obtained at another time and by a different reaction would be the same. Let us take a look at the way in which the idea of definite proportions was functioning in Lavoisier's renowned investigations.

In determining the chemical composition of water Lavoisier decomposed it to synthesize it subsequently from the alleged components. In this way he showed that water is a composite substance and determined its components. A much-publicized experiment of his consisted in decomposing water in an appropriate vessel using active carbon. Of the reaction products he determined the quantities of "a very light gas which easily burns in air" hydrogen and of "carbonic acid" (actually anhydride, that is, carbon dioxide). From this latter product Lavoisier had to quantitatively separate oxygen, as only this element was a component of the water he analysed. In doing so Lavoisier conducted no empiric analyses but instead relied on the analyses of the "carbonic acid" he had analysed at another occasion.[23] He wrote: "I had found before that to produce 100 g of carbonic acid one needs 72 g of oxygen and 28 g of carbon." Remarkably enough, it is only by assumption that the quantitative proportions of the compound (of carbon dioxide in his case) made that type of procedure meaningful. Having determined the quantity of oxygen that was bound with the previously determined amount of hydrogen, Lavoisier formulated his final general conclusion: water is a compound of hydrogen and oxygen bound with each other at such and such a weight proportion. His generalization of the result he obtained also attests to his tacit assumption of the idea of constant proportions. When, in another experiment, Lavoisier synthesized water he took

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[23] Previously, the composition of carbon dioxide designated "bound air" had been determined by J. Black. Concerning this issue see J. R. Partington, *J. Black's Lecture on the Elements of Chemistry*, "Chymia" 6 (1960), pp. 21 f

already strictly accurate quantities of both gases correctly anticipating the appropriate amount of the product.[24]

In other investigations he followed an analogous pattern. Once he had established credible and verifiable weight proportions between a compound's constituent elements Lavoisier assumed their constancy and in subsequent investigations relied on the data thus obtained. The quantitative characteristics of the compound were presented by realting it to 100 g of it or else by indicating the weight percentages of its individual components.

Lavoisier's case is by no means unique. We could easily indicate studies by other chemists who tacitly assumed constant proportions of composition. Kuznetsov mentions M. V. Lomonossov in this connection.[25] We might also recall T. Bergman, who presented the quantitative characteristics of many chemical substances,[26] or several other 18th-century chemists. This significant circumstance has also been noted by Partington: "The assumption that compounds were of definite composition seems, therefore, to have been tacitly recognized during the eighteenth century by all chemists who concerned themselves with quantitative investigations."[27] But even if we should recognize this as too sweeping a generalization, we shall certainly be justified in holding the following view: the determinants of a discovery-generating situation include not only elements of articulated knowledge but also a knowledge that is tacitly assumed in the science of the given epoch.[28] That knowledge can be revealed and exposed by studying the logical interdependences between the individual components of articulated knowledge and by reconstructing the research procedures applied.

6. NEW COMPLEMENT-QUESTIONS

A methodological analysis of the complex process of formation of a discovery-generating situation in science cannot ignore the issue of the questions that express the scientific problems being obtained at the given moment. An analysis of that process in terms of the questions phrased in virtue of the logic of questions seems to be indispensable for its comprehension. While this issue goes beyond the limits of the present essay, we must mention the significant role of composite questions for the problem considered. As regards what is called complement-questions,[28] their correctness or incorrectness depend essentially on the assumption implied in

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[24] Lavoisier, op. cit., p. 91. Cf. also M. Daumas, D. Duveen, *Lavoisier's Relatively Unknown Large-scale Decomposition and Synthesis of Water*, "Chymia" 5 (1959), pp. 113 f.

[25] Kuznetsov, op. cit., p. 15.

[26] Partington, *A History of Chemistry*, op. cit., p. 187.

[27] J. R. Partington, *A Short History of Chemistry*, London, 1948, p. 153.

[28] This is reminiscent of Polyani's "tacit knowledge" (M. Polyani, *Knowing and Being*, London, 1969). A comparison of Polyani's intuition with the idea presented here would however exceed the limits of this essay.

the questions.[29] Various propositions recognized at the given stage of historical development of science may function as deliberately adopted assumptions (sometimes auxiliary hypotheses are included in the set of assumptions). In this way, individual element of the knowledge scientists have at the moment may delimit the *datum quaestionis*, thus channelling the researcher's attention into some definite domain.

But the assumptions of the question, let it be pointed out here, are not always adopted deliberately. Sometimes they can be brought out only in result of a logical analysis of the questions; occasionally such assumptions come to light only after later discoveries. For this reason, in analysing a discovery-generating situation we must take account of two groups of questions: (a) questions about what assumptions, as explicitly articulated propositions, are accepted in the science of the given period, (b) questions about what assumptions were actually, though tacitly, accepted but did not belong to the set of assumptions deliberately recognized in the science of the given period. Let us start with the first group of questions.

(a) The revolution in chemistry effected by Lavoisier and his contemporaries was followed by a great many new scientific problems that came up for the first time ever and which found expression in complement-questions. Their assumptions were the newly discovered ideas of chemistry. For instance, the question "What amount of oxygen will be used up during the combustion of a definite amount of a given substance?" assumes implicitly the Lavoisierian interpretation of combustion as a process of uniting with oxygen. The question would be meaningless if the earlier view that combustion is the liberation of phlogiston were still recognized as true. Secondly, some questions that with the previously accepted knowledge had still been admissible turned out incorrect in the light of the new knowledge. This is a circumstance of particular significance to our problem. The Lavoisierian law of conservation of mass in chemical reactions, which by then had been commonly accepted and applied, and which said that the total masses of the reaction products are equal to the total masses of the reaction substrates, essentially prohibited any questions that might imply the possibility of free creation of matter in the course of the chemical process. In its stronger formulation—the one used by Lavoisier himself—that law says that, the mass of each element participating in a reaction is preserved. This wording of the law puts up a further prohibition: it rules out the possibility of formulating questions concerning the formation of a certain amount of one element at the expense of decreasing the amount of another during a chemical reaction. Lavoisier wrote simply that in the course of a chemical reaction the quantity and quality of elements do not change and that what does take place are only changes and modifications.[30] By ruling out some questions as meaningless Lavoisier opened the possibility of formulating new complement-questions, namely questions

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[29] This nowadays common term is discussed in detail by K. Ajdukiewicz, *Logika pragmatyczna*, Warszawa, 1963, pp. 86 f.

[30] The criteria of correctness of questions are considered by Z. Cackowski, *Problemy i pseudoproblemy*, Warszawa, 1965, pp. 172 f.

about the manner in which those "changes and modifications" materialize. For, if one assumes that during a chemical reaction neither the amount nor the quality of elements change, then we can imagine that they may perhaps unite with one another freely at arbitrary proportions which might depend, for instance, on the reaction conditions. Or else, one can presume that that arbitrariness is subject to any limitations or, in extreme cases, that the proportions at which elements unite in forming a given compound are constant. The question what kind of qualitative changes and quantitative modifications can be expected in result of elements reacting with one another arose in a natural manner from Lavoisier's chemistry and it contains the components of that chemistry as its assumption. This question was explicitly answered by Proust: elements may not unite at arbitrary, but at constant proportions, such that are "distinguished by Nature" and definite for each chemical compound.

(b) Let us now turn to the second group of questions, those about what expressly or tacitly accepted assumptions were not explicit propositions formulated implicitly or constituting part of the recognized chemical knowledge of the given historical period. Before Proust presented the law of definite proportions many empirical investigations had been carried out to answer the question "What is the composition of substance *n*?" Since not only the components of substance *n* were determined in them, but also the quantitative proportions between them, the actual problem in question was the qualitative composition; those questions referred to the general characteristics of a given type of chemical compound, while the answer formulated on the basis of empirical investigations was normally phrased thus: "Substance *n* is a union of elements A and B in an $a{:}b$ proportion." The answer, then, did not refer to any concrete dose of the substance produced in clearly defined conditions (say, of temperature, pressure, acting masses etc.). This would have been the case if a variation in the compounds composition depending on those conditions were admitted. But this was not what actually happened; on the contrary, the conclusions from that type of empirical research were given a general validity. We shall easily notice that the contention that compounds have constant compositions was the tacit assumption underlying such questions and that it was only by virtue of its validity that any such question was indeed meaningful.

Thus, one symptom of science maturing towards a new discovery to be made may also be the circumstance of a new yet not explicit idea start to function as an assumption of complement-questions. This issue, incidentally, is closely related to the problems discussed in Section 5 concerning unarticulated knowledge.

The gradual "densification" of discovery-conducive factors ultimately carries that unarticulated knowledge to a point at which it gets its explicit expression. Then the historian of science proclaims: a discovery has been made. The philosopher of science, however, will observe that a new hypothesis has been formulated and will ask for its justification. The complex process of formation of a new knowledge, its gradual emergence on account of specific mechanisms of science tends to escape the attention of the traditionally framed student of science. In perceiving the subjective aspect of scientific discoveries he has so far contented himself with the idea of a mystical discovery-act and refrained from the study of the objective laws governing the discovery-process.

7. CONCLUDING REMARKS

The above analysis of the process of maturation of scientific discovery is presumably incomplete. It falls short of being a sufficient foundation for drawing general conclusions, though it must be added that an analysis of the discovery of the law of conservation of mass in chemical reactions has led us to analogous conclusions. That law, too, was gradually emerging from 18th-century science and long before it was published (by Lavoisier in 1789) it had emerged in science as a tacit scientific assumption or, for that matter, as unarticulated knowledge.[31]

Still, the above analysis demonstrates beyond any doubt that the origin of what we used to call discovery is an extremely complex process. What, then, are the features of the discovery-generating situation in view of the material presented here? In characterizing such a situation one can note changes at the level of science (in the special field of knowledge in which the discovery was made) and, not infrequently, at the level of metascience (patterns of research behaviour). Moreover, the geography of the domain of research changes too; areas in which the studied discovery is to appear come to the fore and gain in importance. Next develop the discovery-inducing criteria of choice and evaluation of what should be studied, first in view of theoretical and practical needs and then of what can at the given moment be feasibly studied in view of the technical possibilities available. Channelled thus, the investigations carried out add to the empirical knowledge accumulated. This in turn gradually narrows down the space of possible solutions to the problem whose actual solution is precisely that discovery. Solutions that are evidently contradictory to the data available to scientists are not put forward; on the contrary, it is assumed that those solutions will fulfil certain definite logical criteria.

Let us add that the maturation of a situation to a discovery proceeds not only in result of the appearance of new facts or even of empirical laws but also in result of a general shift of previously known data toward the focus of interest of researchers. A singular role in that process pertains to such empirical and theoretical premises to a scientific discovery that may serve as vindicatory grounds to the new idea (justifying it empirically or theoretically, directly or via longer chains of indirect evidence).

The new idea starts functioning at first as an element of unarticulated knowledge, as a tacit assumption of definite theoretical operations. Composite questions begin to be asked in science in which that idea is logically implied as an inherent though unrealized assumption.

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[31] A. Lavoisier, *Œuvres*, Paris, p. 101.

The observation that before a definite idea has surfaced in the set of articulated accepted scientific propositions it functions often as an element of unarticulated knowledge is of paramount significance. It is further evidence of the contention that the situation preceding the emergence of a new concept in science must be submitted to philosophical-scientific analysis. Moreover, that observation helps explain the facts noted down in the history of science that progress has often been achieved by revealing and formulating tacitly accepted assumptions (as was the case of the discoveries of the laws of definite proportions or of conservation of mass in chemical reactions)[32] or else by their disclosure or revision, which usually leads to revolutionary discoveries (for instance, Einstein's revision of the "obvious" ideas concerning space and time). Finally, the above observation justifies the methodological postulate for specialists to pay close attention to the problem of those tacit assumptions, for one can expect that such an attitude will be epistemologically fruitful.

These, and presumably other unmentioned factors conducive to a discovery can be identified when science is treated as a certain whole in which parts condition one another and are themselves conditioned by other domains of culture, as a whole having its own specific dynamics. It is in such a perspective alone that we can hope to reconstruct that situation of converging cues conducive to a new discovery, cues whose integration by means of that idea marks a progress from the standpoint of the current needs of science. Only a philosophy of science that is equipped with the categories of logic is capable of bringing out the actual objective reasons justifying a change in science.

In listing a number of discovery-generating factors it should perhaps be stressed that not each of those factors taken individually but only all of them together make up the constellation of premises out of which the new idea emerges. Only in their total do they constitute the new knowledge and codetermine its substance. These determinants, in particular the above-mentioned empirical data justifying the emerging new concept, are of course not right away identified as such and hence cannot become the psychological motivation of the discovery. Their actual place within the internal mechanism of science that shapes the new idea is fully revealed only by an *ex post* analysis of the objective situation in science, an analysis displaying the logical interrelationships between the individual elements of the knowledge of the given epoch. Such an analysis does not simply describe the facts on the grounds of the historical documents of the epoch, still less does it present the discoverer's inner experiences. On the basis of historical data it reconstructs the objective situation in science unveiling logical dependences between the individual elements of the knowledge such that before have perhaps not been perceived or described explicitly

[32] Cf. E. Pietruska, *Découverte de la loi de conservation de la masse — Analyse methodologique*, "Organon" 12/13 (1977), pp. 211 f.

but whose identification seems to be indispensable for a comprehension of the emergence of the new knowledge.

It is in this sense that the phase of formation of new knowledge may, in contrast to what has traditionally been held as true, be a subject of inquiry by the philosopher of science. More still, philosophy of science has one of its most urgent tasks in scrutinizing the formerly unheeded *objective* aspect of the scientific discovery.