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# THE PUMPKIN HOUR IN SCIENCE

## HOW AND WHY MAJOR ACHIEVEMENTS TURN INTO MARGINAL FINDINGS

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ABSTRACT: By scrutinizing Theodore Richards' thirty years career success almost entirely due to the determination of the atomic weights, the author exposes the phenomenon of the pumpkin hour in science. It occurs when an all-embracing transformation of the conceptual background of a subject field that brings to revision of its basic tenets turns the fine carriage of some - not only theoretical but even experimental - results raised to the rank of outstanding contribution into a pumpkin of minor improvements with no crucial role in knowledge. The paper shows the mechanism governing the reappraisal of "pieces" of knowledge and reveals the link preventing the scientific community from proper assessment of new ideas and facts: a discovery is valued the higher the more the value it adds (by consolidating, unfolding, and supporting) to the conceptual background through the lens of which it itself is looked at.

KEYWORDS: Contribution; Appraisal; Relevancy; Recognition; Atomic weight

## INTRODUCTION

For an idea to be acknowledged as a valuable piece of knowledge, it must tie in with the conceptual background of the science it pertains to. The vast majority of active scientists have learnt and adopted a range of tenets setting a certain standard of perception, interpretation, and representation of phenomena they deal with. This "dogmatics" as the core of the conceptual background of a subject field draws the boundaries of the possible and admissible (Petrosyan, 2015; 181 -

182). Any further scientific finding gets passed through the alembic of the conceptual background and may claim the status of high achievement only if seems to be valid against it. And to the contrary, a new result fitting into the established frame of knowledge badly, cannot be adopted, for nobody is capable of appreciating the true value of what oversteps the limits of his understanding. The more the divergence from the known and learnt the more difficult is it to grasp the real meaning and future resonance of a discovery. And vice versa, an idea that closely fits the conceptual background has by far better chance of being understood and is assimilated more easily and, therefore, valued more highly.

This effect was vaguely felt of old by many philosophers and scientists. But it had been explicitly made out only in the beginning of the twentieth century. From that time onward, the theory-laden nature of experience and the inner unity of knowledge gradually became more and more obvious. Besides, the part of the scientific community as a whole in the recognition of findings began to be distinctly realized.

Early in the twentieth century, Pierre Duhem has shown that an experiment is not merely a precise observation of a phenomenon but also an interpretation of the data in the light of "the theories the observer has admitted." At that, not an isolated hypothesis is subjected to testing, but "a totality of hypotheses." Which of them is to be modified or replaced remains obscure - due to the close interweaving of the ideas they embrace; to say nothing of that some of them (like the law of inertia in mechanics, the law of multiple proportions in chemistry, or the law of rational indices in crystallography), of highest significance, taken individually, "have no experimental meaning." They are confronted with experience in unity with other associated statements (Duhem, 1906; 238, 307, 354). What to mend, change, or discard for it to satisfactorily explain the phenomena and, at the same time, not to run counter to the experiment a scientist decides on his own.

Half a century later, this idea had been pointed and broadened by Willard Quine. He noticed that cohesion was characteristic of not only the statements of a theory but also science as a whole. In his words, "from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic," it "is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions the readjustments in the interior of the field." That is why the truth values of individual propositions not always remain immutable. They "have to be redistributed over some of our statements." Their reevaluation "entails reevaluation of others, because of their logical interconnections." At the same time, "the total field is so undetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience" (Quine, 1961; 42 - 43). Hence, even transformations in the knowledge with no direct relation to a given proposition are fraught with the revision of its validity. As a result, what has been considered irrefutable may turn out to be a fallacy, and vice versa.

Another weighty step had been made by Norwood Hanson. He drew attention to a thing scientists twigged to, but preferred not to notice. Facts get a good deal of their meaning from the knowledge available. Thus, the wave-like behavior of light had different sense for Fresnel and Young, on one hand, and Compton, on the other. In the first case, light evinced a property like that of water or sound, while in the second, it appeared as a complex substance whose behavior only partly resembled waves that seemed to be a tangled manifestation of the nature of light particles. "The experimental context and theoretical backgrounds in terms of which these facts have significance," Hanson remarks, "are so vastly different as to put our understanding of them on virtually two distinct intellectual planes." In this regard, "the facts are what our hypotheses call to our attention" (Hanson, 2018; 166 – 167; 171). Therefore, no wonder that when the knowledge through the lens of which experience is passed undergoes essential alterations, the meaning of the facts observed may change.

Finally, an important emphasis had been introduced into the picture by Thomas Kuhn. He found that the decision whether to adopt or refuse a new paradigm "must be based less on past achievement than on future promise." Hence, it "can only be made on faith." Moreover, the choice between paradigms is perceived as a joint decision; it "may not be merely personal." The group agreeing with it "is not drawn at random from society as a whole." It represents "rather the well-defined community" of "compeers" which acquires the status of "the exclusive arbiter of the professional achievement." Communities of this kind become "the producers and validators of scientific knowledge" (Kuhn, 1996; 157 – 158, 168, 178). They turn into collective knowing subjects bringing in their verdicts within the well-established norms of activity.

Of course, Kuhn's treatment of the sentences passed by the scientific community as almost arbitrary choice that does not reckon with the inner logic of science could not but meet with a rebuff. Imre Lakatos noted that Kuhn failed to reveal the mechanism of the conceptual development of science and only replaced individual psychology with social. Meanwhile, "psychology of science is not autonomous," and "the – rationally reconstructed – growth of science takes place essentially in the world of ideas, in Plato's and Popper's 'third world', in the world of articulated knowledge which is independent of knowing subjects." And as Kuhn's portrayal of the growth of knowledge was far from such a reconstruction, Lakatos called it not merely "a caricature of the original," but "a caricature of a caricature" (Lakatos, 1989; 91 – 92). Sure, his recourse to the "third world" is very arguable. There exists no objective world of knowledge; instead, we have information objectified in signs which, for it to turn into knowledge again, should be construed inside individual minds and put into circulation through communication between them. Nevertheless, it is beyond doubt that judgments and assessments of the scientific community as to the validity of knowledge are based on not so much the tastes and beliefs of those making decisions as knowledge's own qualities, intrinsic relations, and ensuing possibilities.

But that is only one side of the coin. It concerns the veracity of knowledge. On one hand, when addressing the changes in the attitudes towards some elements of knowledge, their truth values, that is to say, the measure of correspondence to reality, reliability, or, at least, truthiness, are implied. And on the other hand, it keeps in focus the comparison of the pieces of knowledge with each other, and the choice, among them, of one most trustworthy and, just therefore, called on to supersede that which, due to the transformation of the knowledge surrounding it, has turned into a doubtful or false entity.

The other side of the coin, which remains in the shadow up to now, relates to the relevance of the results obtained, that is to say, the worth attached to them by the scientific community. Even if the validity of a finding is not impeached or changed by the transformation of the conceptual background it, nevertheless, may cease to be much considered. This is no less, and to practical effect even more, important a problem. For, the recognition of a scientific finding and the reward its author(s) may receive are more dependent on its relevance (worth) rather than being true. And it is a task of high priority for the philosophy of science to reveal the mechanism governing the revisions in the appraisal of one or another "piece" of knowledge.

Either of findings is assessed in direct proportion to the part it has the potential to play in solving the existent theoretical or practical problems. Its status will be the stronger the closer it is interwoven with the conceptual background. And to the contrary, if a finding does not respond clearly to a topic of vital importance, it hardly can reckon on a precedence place and, consequently, recognition as a major achievement.

There is a lot of sense in such an approach to the products of scientific labor. Rating a finding sought after higher than that irrelevant to the tasks being performed must, in idea, allow of not merely estimating the scientists' efforts at their true worth but also directing them, from the very outset, to doing what is able to bring more useful output. However, the snag is that revealing the relevance of a discovery is not a trivial job. Scientific mind not always copes with it even in relatively simple cases when the connection of a discovery with applied purposes and the existent knowledge can be easily traced. But when it comes to fundamental results, not only errors but also fatal delusions happen, what sometimes adds to evaluations a grotesque tint.

The limitations of the scientific community in appraising the worth of discoveries appears most obviously not when rejecting the findings afterwards recognized as major breakthroughs, though such incidents occur pretty often. Still more revealing are the opposite cases when to the rank of a key, constitutive result is elevated what has actually no great significance and sometimes is even void of any special meaning. And just one of such "aberrations" of scientific mind is scrutinized in this paper.

For many years, the work of the American chemist Theodore Richards in providing accurate determination of the atomic weights was highly appreciated and encouraged. The scientific community conferred on him all thinkable kinds of reward. The first evidences that the results obtained by Richards could not make an essential contribution to the growth of knowledge arose even before he had been crowned with a Nobel Prize. Year after year, their number accrued, but awards continued to rain down upon him as if out of the horn of plenty. But when it became clear that beyond a certain threshold, precise values of atomic weights were fruitless in principle, Richards turned, all of a sudden, from a scientific giant into an almost ordinary figure being mentioned in passing along with many others experimenting with chemical substances.

What was the nature of the transformation that converted Richards' fine carriage into a pumpkin? Why had his outstanding achievements become minor findings with no crucial role in knowledge? And how did science get out from under the charm of what seemed to be fundamental and magnificent?

It may be said that the fascination with Richards' results ensued from the common interest in atomic weights and his experimental skill rather than the actual contribution he had made to science. But this straightens out nothing much, for it remains unclear why concernedness with a result can so outweigh its actual potential. Therefore, it would be more efficient to look into the mechanism that has prevented the scientific community from adequate evaluation of Richards' findings. That would show, as well, what is meant in reality by importance when assessing a scientific finding "on a hot scent" and how much such an importance differs from that which is seen through the lens of the transformed conceptual background and from the height of more extensive knowledge and deeper understanding of the world.

## 1. THE RISE TO FAME

Richards was awarded with Nobel Prize in Chemistry (1914) for "his accurate determinations of the atomic weight of a large number of chemical elements" (The Nobel Prize, 2001; 80). Indeed, his contribution to both the determination of the atomic weights and the development of the related experimental procedures was altogether unquestionable (for an overview of Richards' meticulous technique to foresee possible errors and avoid them see: Herschbach, 2014). He was valued for his patience and perseverance in work and intolerance to inaccuracy. "Every substance," Richards used to say, "must be assumed to be impure, every reaction must be assumed to be incomplete, every measurement must be assumed to contain error, until proof to the contrary can be obtained" (Richards, 1911; 539). And for three decades, the scientist consistently followed this principle.

Richards' research career began in his student days from studying the atomic weight of oxygen. He came to the conclusion that it had been overestimated.

Working with copper oxide, he noticed that it contained nitrogen as an admixture. Richards inferred that the atomic weight of copper was less than generally assumed. The difference was too small (fewer than a percent), but it could not be explained by the errors of experiment. For these results and those allied to them, he had been honored in 1888, at the age of twenty, with the degree of doctor. No wonder that his research attracted a lot of attention.

At the turn of 90s, Richards, when completing his experiments with copper where barium was used by way of one of its salts, revealed that the atomic weight of barium was still less precise. In the course of this work, it came to him that the errors committed by his predecessors in the study of barium could be repeated when dealing with strontium, an element chemically very close to it. By correcting them, Richards obtained a more precise value for the atomic weight of strontium. So, passing from a problem to another, he accurately redefined the atomic weights of zinc, magnesium, nickel, cobalt, iron, silver, carbon, nitrogen, and other elements, over 60 all told. And in every case, his results looked to be a step forward as compared with the accepted values.

The recognition of Richards' achievements was total. It was not limited to Nobel Prize. In 1910, London Royal Society presented him with Davy Medal. A year later, London Chemical Society decorated Richards with Faraday Medal. In 1912, he received Willard Gibbs Medal of American Chemical Society. In 1916, Franklin Institute (Philadelphia) gave him the medal of the same name. Richards was Honorary Doctor of many scientific institutions and had been chosen twice President of American Chemical Society. And all this was meant for a person who devoted his career to almost nothing else but measuring the atomic weights and made no other significant contribution to science.

What had put so great value on Richards' research?

The scientist was not a pioneer in his work. From the time of Dalton who had supposed the elements to differ by their weights, there was a great interest in measuring them. But the way Dalton was thinking in could not yield very accurate values. Dalton took the weight of hydrogen as the reference element and set it equal to 1. As it was believed that the most stable combination is binary one, he assigned the atomic weight 8 to oxygen, since such was the proportion of oxygen to hydrogen in water (according to the formula HO). Some, as William Wollaston and Leopold Gmelin, were aware of the incongruity of this approach and proposed to call the values found by Dalton equivalent, instead of atomic, weights. However, the influence of Jöns Jacob Berzelius, who, keeping the letter of Dalton's notion of binary compounds, conducted hundreds of experiments in purifying the specimens and determining the atomic weights, and his stubbornness in imposing his views on the others brought to that "confusion and doubt concerning the whole of the atomic theory became so widespread that, by 1850, the theory was practically abandoned" (Briscoe, 1935; 19). Chemists lacked necessary concepts and data to establish the atomic weights with sufficient accuracy.

In 1811, the Italian chemist Amedeo Avogadro introduced the term "molecule" to denote the smallest particle of compound, and element in its free state. The particle of oxygen that, through combining with hydrogen, gave two particles of water, had been named molecule, while each of its halves, atom. In addition, it had been shown that combining powers of the elements are not the same. So, one atom of oxygen forms a molecule of water with two atoms of hydrogen, while a molecule of ammonia is composed of three atoms of hydrogen and only one, of nitrogen. The realization of these differences allowed of obtaining a means for more accurate determination of the atomic weights, which consists in multiplying the equivalent weight of the element by a factor that represents the combining capacity of the element as measured against that of hydrogen as the basic element (Briscoe, 1935; 29 - 30). However, as Avogadro's theory was far from being generally accepted, the muddle of the atomic weights and formulas for chemical combinations went on aggravating.

The situation has changed after another Italian chemist, Stanislao Cannizzaro, published in 1858 his work "The Sketch of a Course in Chemical philosophy" where he revived and substantiated Avogadro's theory and explained a lot of discrepant data. As he stressed, "we have only to distinguish atoms from molecules in order to reconcile all the experimental results known to Berzelius." Cannizzaro was opposed not only to the latter who "did not admit that the molecules of simple substances could be divided in the act of combination," but also to Gerhardt's supposition "that all the molecules of simple substances are divisible in chemical action" (Cannizzaro, 1910; 4 - 5). He showed that the halves of a molecule of hydrogen, oxygen, nitrogen, or their atoms, can make combinations with other elements severally and, at the same time, they are indivisible at least in the sphere of chemistry. Thus the measurement of the atomic weights became a plain and sensible work that, by the efforts of many scientists, and above all others, the French Jean-Baptiste Dumas and the Belgian Jean-Servais Stas, yielded a whole nomenclature of carefully measured values. In the issue, the Periodic Table arose which arranged the elements through linking their atomic weights with chemical properties.

However, it was not all plain sailing. Apart from that diverse errors emerged permanently when determining atomic weights, and respective corrections were made, the Periodic Table came across serious anomalies requiring explication. Say, cobalt followed nickel, while the latter was believed to be lighter (Lowry, 1915; 497). Still more troubled were chemists by argon discovered in 1894 by Lord Rayleigh and William Ramsay. Its atomic weight taken to be equal to 40 poorly tallied with the same value of that of calcium. This raised a point of adequacy of the periodic system. Thus, an Italian researcher, Raffaello Nasini, came to the conclusion that either no confidence should be placed in the inferences from the kinetic theory of gases or the Periodic Table should be thrown off (Nasini, 1895). But such an opinion was far from meeting with broad support.

Many were convinced that one or two elements with "strange" properties did not overturn the periodic system based on a whole raft of facts and conceptual generalizations. One of them was Dmitry Mendeléeff who regarded 40 as an unlikely value for the atomic weight of argon. In his opinion, it "does not answer to the available information on the periodicity of properties of the elements, depending on their atomic weights." As between chlorine (35.5) and potassium (39), "there is no reason to admit of any intermediate elements, while above potassium, all the places in the periodic system are filled in," that value "gives argon no place at all." To pull through, some supposed a molecule of argon to consist of two atoms and, consequently, its atomic weight to be 20. But, though Mendeléeff considered it quite possible, he was inclined to believe that argon is either "thickened" nitrogen ($N_3$) or an element with the atomic weight near to 6.7, whose "particle" (molecule) comprises 6 atoms (Mendeléeff, 1958; 454 – 456, 463, 467 – 468). That, according to him, would allow of putting argon in a vacant cell of the Periodic Table.

However, the snag was that Rayleigh and Ramsay had, by then, demonstrated that the molecule of argon is monatomic and its atomic weight is, indeed, equal to 40 (Rayleigh, Ramsay, 1895; 236). And there was nothing to do but find an explanation for why two elements with distinctly different features have virtually the same atomic weight, and arrange them in an acceptable way. All these complications and discords hit upon the idea that the atomic weights of the elements had been determined with precision which could not suffice, and their further measurements were expected to brighten the chemical picture of the world.

The other – and even more important – side of the coin was the notion of the structure of matter that prevailed in chemistry. As early as 1815, the British chemist William Prout, proceeding from the atomic weights established by then, noticed "the near approach to round numbers of many of the weights of the atoms" and put forward a hypothesis about their relations to each other. He built tables for "simple bodies" - from oxygen to barium – from which concluded that their atomic weights represented whole multiples of the weight of hydrogen (Prout 1815; 326, 330). This gave rise to the idea that hydrogen is nothing but the primordial matter things are composed of.

## 2. BELIEVING IN PRIME BLOCKS

Many took up Prout's idea. Among its firm adherents was another British scientist Thomas Thomson who saw in it a "sagacious conjecture." He got down to test the hypothesis, "because the establishment of its truth would at once give a simplicity to the atomic numbers, which has not been suspected, and would place the science of chemistry in a new and much more advantageous situation than it had ever occupied." Presenting his results, Thomson stressed that "for every substance of which I could procure a sufficient quantity to enable me to examine it fully, has been found not only a multiple of the atomic weight of hydrogen; but, if we except a few compounds into which a single or odd atom of hydrogen enters, they are all multiples of 0.25 or of two atoms of hydrogen" (Thomson, 1825: 26). This boosted the credibility of the uniformity of nature and continuity of its levels and –what is more – confirmed the constancy of the atomic weights and their fundamental part in chemical interactions.

Sure, not all of chemists accepted Thomson's result. All the more so that there was a taint of tendentiousness in it, which manifested itself in that any doubts were interpreted in favor of Prout's hypothesis. Berzelius who subjected the reasons adduced by Thomson to caustic criticism stated that Thomson, when determining “the atomic weight of each simple body,” goes beyond that task and “corrects all of what occurs before him.” He bases himself “on only one idea, namely that the atomic weights of all bodies constitute precisely the multiples of hydrogen,” and therefore, at first “reduces all the numbers found by his predecessors to the nearest multiple of hydrogen,” thereupon, recalculates the atomic weights of its combinations, and finally, “adjusts these deliberately corrected atomic weights” to fairly correspond to each other. “This work,” Berzelius concludes, “belongs to few from which science derives, in fact, absolutely no benefit.” Many things in it, its experimental part inclusive, are fabricated “at the desk.” And consequently, “the greatest favor the entourage can do to the author is to consider this work never come out” (Berzelius, 1827; 77). Even Stas who began his study of the atomic weights “with an almost absolute confidence in the exactness of the Prout principle arrived in the end at the conclusion that “it is no more than an illusion, a clearest type of hypothesis formally refuted by experience.” He had no doubt about that anyone abandoning his prejudices and following nature would share the conviction that “there is no common denominator for the weights of the simple bodies which combine to form all known compounds.” And therefore, “the base on which Dr. Prout has raised his law is itself devoid of foundation” (Stas, 1894; 311 – 312, 418). However, such attacks could not subvert the hypothesis. They breached it and pushed it to the sidelines, but were unable to kill the interest in it. The fascinating simplicity and heuristic potential of the idea charmed chemists, and they believed that more precise determinations of the inviolable atomic weights will surely have brought to its definitive confirmation.

But since the hope for experiment came true not in full measure, scientists tried to amend the hypothesis. In the early 40s of the nineteenth century, Jean Galissard de Marignac found it necessary to halve the value of the basic unit. After revealing that the equivalent of barium, as well as chlorine, is not “a whole multiple of hydrogen,” he considered the initial version of “Prout’s law” vulnerable. However, in his words, “it is enough to slightly modify the results of experiment to make it the precise multiple of half that of this equivalent.” Say, if to take barium for which experiment yields the values 68.55, 68.59 or 68.6, “all the chemists disposed to admit the existence of simple relations between the equivalents of simple bodies will adopt, without doubt, the figure 68.5” (Galissard de Marignac, 1891; 565). Dumas convinced of the soundness of Prout's idea went still further. In the end of 1857, he came to the conclusion that the atomic weights may be multiples of not only that unit or a half but also a quarter of it. Agreeing with that explicit violations of "Prout's law" are observed, he, nevertheless, did not regard it as an illusion and thought that a certain refinement might recover its nearly lost status. "The equivalents of simple bodies," Dumas reasoned, "are frequently whole multiples of the equivalent of hydrogen taken as a unit." But "when chlorine, barium, manganese, nickel, cobalt, and lead are concerned, as well as the double equivalent of aluminum, zinc, and strontium, the unit they are to be compared with is equal to only 0.5 of the equivalent of hydrogen" (Dumas, 1859; 12 - 13). Yet, successive splitting up of the basic unit did not look too convincing. That frightened off even some of those who liked the hypothesis.

Nevertheless, the attitude towards Prout's idea was, by and large, sympathetic. Despite its being far from complete agreement with experimental results, many took it as an anticipation of a law of nature. Marignac had expressed this, in 1860, better than anyone else: the hypothesis grasps the reality, while the discrepancies are due to the influence of extraneous forces. In his words, "a cause unknown but probably different from the physical and chemical factors familiar to us, which determines certain groupings of atoms of the unique primordial matter" interferes in the process of building the chemical elements from it, wherefore the atomic weight of each of them "is not exactly the sum of the weights of the primordial atoms which constitute it" (Galissard de Marignac, 1891; 701). Two decades later, Frank Clarke who himself made a visible contribution to establishing the atomic weights stated with assurance that "none of the seeming exceptions to Prout's law are inexplicable." And some of them studied properly will even turn out to be its strong confirmation. If to admit, in addition, half-multiples as legitimate, there will remain a small number of exceptions. And it would be more reasonable to assume that they "are due to undetected constant errors" than consider "the great number of close agreements … merely accidental." Clarke confessed that he "began this recalculation of the atomic weights with a strong prejudice against Prout's hypothesis, but the facts" forced him "to give it a very respectful consideration" (Clarke, 1882; 270 – 271). So instead of dismissing the idea, it would be wise to direct the efforts to revealing the mechanism diverting the phenomena from the patterns delineated.

Even more definitely this stance had been taken by John Mallet. Despite Stas' experimental results in measuring the atomic weights are undeniable, his conclusion that "Prout's law is disproved, or is not supported by the facts, appears much more open to dispute." May be, "fortuitous errors have been reduced within narrow limits." However, constant errors cannot be cleaned out to the end, as our knowledge of nature never is complete. On the other hand, "the forms of matter which as known to us under ordinary conditions we call elements may be susceptible of progressive dissociation at enormously high temperature." But if so, and "the atoms of hydrogen as commonly known to us form either the last term, or any term not far removed in simplicity from the last, in the progressive breaking up of other forms of matter, it is obvious that "Prout's law," or some modification of it, such as was many years ago suggested by Dumas, must be true, the atomic weights of all the other so-called elements must be multiples of that of hydrogen, or multiples of that fraction of the hydrogen atom which may result from the dissociation of this body itself" (Mallet, 1880; 1033). Otherwise, the Proutian hypothesis reveals not merely the quantitative relations but also the continuity between the elements. As Gladstone said in his presidential address to the Chemical section of British Association in 1883, "the remarkable relations between the atomic weights of the elements and many peculiarities of their grouping, force upon us the conviction that they are not separate bodies created without reference to one another, but that they have been originally fashioned, or have been built up from one another, according to some general plan" (Gladstone, 1984; 453). The elements represent not only a kind of rungs in the ladder of nature but also entities "unitized" from simpler blocks and, ultimately, from the primordial matter. And there is nothing incredible in that, under certain conditions, they may break up into their constituents.

## 3. THE UNSHAKABLE SAMENESS

Mallet did not confine himself to speculative arguments. He bolstered up the nonrandomness of the experimental data supporting "Prout's law" with statistical substantiation. Out of 18 most accurately established values of the atomic weights, 10 approximated to integers within $^1/_{10}$ of a unit. What was then the probability of their being due to a mere coincidence? The computation performed by him showed 1 to 1097.8 (Mallet, 1880; 1034 - 1035). Under such circumstances, it would be the height of lightness to brush the hypothesis aside.

This sense gained in strength after Robert Strutt confirmed, in the very beginning of the twentieth century, Mallet's inference, on the base of safer data obtained by Richards and quite another approach to the calculation of probabilities. He drew up a table for 9 most precisely defined atomic weights and, by summing up the differences between each of them and the nearest integers, received the total deviation from integral values. Its probability was 0.001159, or approximately 1 to 1000. Then, after adding to the table 18 more elements whose atomic weights were known to be accurate within two decimal places, Strutt computed the same probability for 27 elements (0.00174), which was only half as much again as that for the most precise atomic weights. Hence he concluded that "a calculation of the probabilities involved fully confirms the verdict of commonsense, that the atomic weights tend to approximate to whole numbers far more closely than can reasonably be accounted for by any accidental coincidence." The probability of such coincidence "is not more than 1 in 1000," so that "we have stronger reason for believing in the truth of some modification of Prout's law, than in that of many historical events which are universally accepted as unquestionable" (Strutt, 1901; 313 - 314). But if so, the atomic weights represent fundamental constants of nature rather than simple properties of the chemical elements. Consequently, their determination with increasingly more precision is necessary for not only and not so much confirming Prout's hypothesis as better understanding the surrounding world.

That was common vision in the last quarter of the nineteenth century. In 1886, William Crookes, in his presidential address to the Chemical section of British Association, stated that the elements quite probably originated from a single prime form of matter which he named "protyle" (from Greek words πρo – before, earlier than and ὑλη – matter, stuff). Sure, "everyone is aware that the recent more accurate determinations of the atomic weights of different elements do not by any means bring them into close harmony with the value which Prout's law would require." Nevertheless, "in no small number of cases the actual atomic weights approach so closely to those which the hypothesis demands, that we can scarcely regard the coincidence as accidental." That is why "not a few chemists of admitted eminence consider that we have here an expression of the truth, masked by some residual or collateral phenomena which we have not yet succeeded in eliminating" (Crookes, 1886; 425, 427). Thus, the confidence in Prout's idea was supported due to not only and not so much its being corroborated by experience as the conceptual background of chemistry. On one hand, the hypothesis specified the principle of uniformity of nature as applied to the chemical elements and, on the other, enabled to unscramble the chaotic variety of experimental material and draw up a rather coherent picture in which complex frameworks were built of elementary blocks.

Thin voices doubting the constancy of the atomic weights sounded, too. The same Crookes tried to construe "Prout's law" more malleably when explaining its poor congruency with the results of experiments. He surmised that the atomic weights, though representing a fundamental property of the elements, were not strictly determined, but, rather, distributed over a range of values. In his words, "when we say the atomic weight of, for instance, calcium is 40, we really express the fact that, while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42, and so on." Drawing a parallel with Newton's "old worn particles," Crookes admitted "that these heavier and lighter atoms may have been in some cases subsequently sorted out by a process resembling chemical fractionation." As he thought, "this sorting out may have taken place in part while atomic matter was condensing from the primal state of intense ignition, but also it may have been partly effected in geological ages by successive solutions and re-precipitations of the various earths" (Crookes, 1886; 429). However, the shades of doubt were heard no better than a voice crying in the wilderness, and few were hearkening to them. As Ernst von Meyer noticed, Prout's hypothesis "acted for a long period like a ferment, in that it gave rise to continually renewed speculations upon the connection which existed between the elements and their atomic weights" (Meyer, 1906; 386). Even those not disposed to support it and not believing that the atomic weights are multiples of that of hydrogen and, consequently, that the elements have evolved from the same prime matter were convinced of the constancy of the atomic weights and their unique part in explaining the phenomena of nature.

No wonder that measurement of the atomic weights was taken as an utterly topical task. On one hand, it was called on to straighten out the Periodic Table and to order the relations between the elements. The visible discrepancies in it and the dependency of an element's place upon its atomic weight were to be explicated. And on the other hand, the precise knowledge of the basic property of the elements that ultimately determined their behavior should lift the veil over the mystery of the universe.

Richards believed that "among all quantities worthy of exact measurement, the properties of the chemical elements are surely some of the most fundamental, because the elements are the vehicles of all the manifold phenomena within the range of our perception," and "the eighty or more individual numbers which we call the atomic weights are perhaps the most striking of the physical records nature has given us concerning the earliest stages of the evolution of the universe. They are mute witnesses of the first beginnings of the cosmos out of the chaos, and their significance is one of the first concerns of the chemical philosopher" (Richards, 1911; 538). Sure, Richards was not a "sectarian" not open to alternative views and "thought it worthwhile exploring whether elements might be further decomposable" or "composed of non-identical atoms" (Gay, 1997; 19). Nonetheless, he had no doubt that "the atomic weights of the primeval elements, although less general than these [mathematical functions], are much more general and fundamental than the constants of astronomy, such as the so called constant of gravity, the length of the day and year, the proper motion of the sun, and all the other incommensurable magnitudes which have been more or less accidentally ordained in the cosmic system" (Richards, 1919; 4). These words may evoke a smile nowadays. All the more so that they were uttered immediately before and even after the nonfundamental character of the atomic weights came to be obvious. However, it should be kept in mind that such was the common opinion of the scientific community, which had been echoed in the attitude towards Richards' research. Almost everyone shared the conviction that precise determination of the atomic weights is a key step to the construction of the comprehensive doctrine of the chemical elements.

## 4. TECTONIC SHIFTS

The irony is that just at the time the worth of the results obtained by Richards received the highest recognition, the events flared up which not only undermined their status but also brought about a change in science such that they ceased to be perceived as a mainspring of the progress of chemistry. Piquancy was added to it by that Richards himself was among those having made a contribution to the revision of the role of the atomic weights in the chemical picture of phenomena. He actually sawed off the bough he was sitting on, for his previous research got debunked through his own activity. It came out that even if his findings had never been obtained, there would be no great loss to science.

Since the end of the nineteenth century, radioactivity burst into the circle of scientific interests. Together with it, came bright awareness of things one could previously only conjecture. It turned out that (as some had foreseen) atoms of the chemical elements are unstable, and they may break up by certain rules. That brought an entirely new current into the context of chemical reasoning about reality.

Of final products of radioactive decay, particular attention was given to lead, since it was present in uranium and thorium of mineral origin everywhere. In 1913, Alexander Russell and Alexander Fleck, two students of the English chemist Frederick Soddy, discovered a simple law of radioactive transformations. The emission of $\alpha$– or $\beta$ –particles by an atom changes its properties in a strictly determined way. In the case of $\alpha$-particle, it shifts by two places in the Periodic Table towards the diminution of the atomic weight; but if it loses a $\beta$ –particle the shift occurs in the opposite direction by only one place. After three transformations in any order, when one $\alpha$– and two $\beta$ –particles are concerned, the element returns to its initial position. At that, the atomic weight decreases by 4 units. However, the atom not merely resembles, by its properties and even spectrum, the initial one but also is chemically indistinguishable from it. Soddy and – independently of him – the Austrian chemist Kasimir Fajans, proceeding from this rule, came to the conclusion that the atomic weight of lead obtained from radioactive minerals may be different from that of usual lead.

Indeed, if to take lead originating from the decay of uranium and thorium with the atomic weights 238 и 232, the final products occupying the same 82th place in the Periodic Table and being chemically identical should have the atomic weights 206 и 208 respectively. Meanwhile, the atomic weight of common lead is 207.2 (Soddy, 1920; 102 – 103, 105). Hence an inference suggests itself that the same chemical element may, depending on its origin, have various atomic weights. This represents a serious problem to be explicated.

As early as 1910, Soddy advanced the idea that there may exist elements chemically identical, but differing by their radioactive properties or even atomic weights. Somewhat later, in 1913, at the meeting of British Association, papers were read in the sections of Radio-elements and Periodic Law as well as Homogeneity of Neon, which not merely exposed the phenomena behind the idea but also corroborated it. Soddy coined the term "isotope" (from the Greek words ἶσος – identical and τὄπος – place) to somehow denote substances of this kind (Aston, 1948; 5 - 6). He had not left it at that, but got further by supposing that "common lead is a mixture of isotopes rather than a single homogeneous element" (Soddy, 1920; 105). That means atomic weight is not a fundamental or even a self-contained property; it derives from something else that determines the position of an element in the Periodic Table, and its chemical features.

But to persuade others of the correctness of the idea, speculation alone was not enough. Experimental confirmations were required. The atomic weight of lead obtained from uranium stuff free of thorium as much as possible had been found by Richatds and Max Lembert, the assistant of Fajans, whom the latter sent to Harvard, together with some specimens of the minerals. Experiments were conducted on lead extracted from uranite of North Carolina, and Bohemian and English pitchblende. In the first case, the atomic weight was 206.4, in the second, 206.6, and in the third, 206.8 (Richards, Lembert, 1914). As Richards noted afterwards, the difference was 0.3 percent, that is, "far beyond the probable error of experiment" (Richards, 1919; 5). But even more important were salient deviations of the established atomic weights from the "standard" of usual lead – depending on where the minerals came from.

In 1914, Soddy and Henry Hyman obtained lead from Ceylon thorite which contained 62 percent of oxide of thorium and only 0.4 percent of lead oxide. The low content of lead evidenced that lead was of radioactive origin, and the mineral pristinely had none of it. Out of 1 kg of substance, 1.2 g of lead had been extracted after careful refinement, whose atomic weight was determined in two different ways. One of them brought to the value 208.5 and the other, 208.4 (Soddy, Hyman, 1914). This demonstrated once more that the same element may have various atomic weights.

In 1915, the Austrian chemists Otto Hönigschmid and Stefanie Horovitz studied lead extracted from very pure pitchblende brought from East Africa, as well as from Norwegian breggerite. The values found by them were 206.05 и 206.06 respectively (Hönigschmid, Horovitz; 1915). In the same year, Thomas

Merton showed that the spectrum of lead with atomic weight 206.05 is completely identical with that of common lead (Merton, 1915). A year later, Richards and Charles Wadsworth published the results of their determination of the atomic weights of lead obtained from diverse uranium stuffs. In the case of Australian carnonite, they found 206.34, while for Norwegian cleveite, 206.08. The accurateness of the measurements was essentially depending on the proportion of uranium to thorium in the specimen: the lower the content of uranium the better (Richards, Wadsworth; 1916). That is why in 1918, Hönigschmid extracted lead from Norwegian thorite with still lower proportion of uranium to thorium and found its atomic weight to be 207.9. After one more year, for three Ceylon thorianites containing both uranium and thorium (11.8%; 20.2%; 26.8% of the former and 68.9%; 62.7%; 57% of the latter), he received the values 207.2, 206.9 and 206.8 respectively (Berry, 1946; 108 - 109). All this convincingly demonstrates that the atomic weights of lead significantly change from one specimen to another. In reality, it is not a constant of nature, but a mean value for isotopes a substance includes. As to common lead its atomic weight, very close to 207, may be construed as the average for the atoms of lead arising from the decay of uranium and thorium in the course of geological processes.

Theretofore, no variations of atomic weights were known. Minerals were obtained from diverse parts of the world, but there was no particular difference observed. Even if something of the kind were happened it would be accounted for by the errors of experiment. For that very reason, nothing was able to undermine the chemists' believe in the invariability of the atomic weights. But radioactivity overthrew this idyllic picture.

The old conception of atom as an immutable, inert, and indivisible entity could not cope with radioactive phenomena. Joseph Thomson showed that even the simplest atom (of hydrogen) is of exceptional complexity. Its second spectrum consists of thousands diverse lines whose frequencies are such that it is difficult to believe in their belonging to the same series. "The electron and the positive charge," Thomson observes, "produce an electric field all round them, and an electric field is probably a very complicated piece of mechanism." It may be represented as "consisting of a large number of lines of force, with one end on the electron and the other on the positive charge, spreading out into the space round the atom, and we may also suppose that these lines of force may move about even though their ends are at rest, and thus vibrate independently of the electrons" (Thomson, 1914; 21). As to atoms of the other elements their structure is still more complex (Thomson, 1907; 114 – 117). They are composed of many electrons (isolated negative electrical charges) moving in their orbits with considerable velocities in a field of positive electricity.

Some electrons in atom jump from their orbits and got thrown off beyond its "framework". Heavy atoms contain a great many electrons, and that, other things being equal, makes them more instable. The same are also most radioactive.

The heaviest among the known elements was uranium (238.5). It was followed by thorium (232.5). They both were highly radioactive. But the most radioactive among the known substances was radium (227). In this regard, it was far ahead (no less than a million and a half times) of uranium (Jones, 1913; 292). And this came into collision with the notion of the proportionality of radioactivity of an element to its atomic weight.

Radium possesses immense inner energy. This manifests itself particularly in that its decay is accompanied with the release of a huge quantity of heat. However, a radium atom's inner energy is nothing else but the total kinetic energy of its electrons. Hence, their velocities must be extremely high. Otherwise, these electrons supposedly move much more quickly than those in atoms of the other elements and, therefore, can escape from their atoms with greater probability. That is why radium should be more radioactive than, say, uranium or thorium whose electrons are by far slower.

The transformations of radium are accompanied with the emission of α-, β- и γ-particles. The decay continues until more or less stable lead called "the ashes of radium" arises. Something of the kind takes place with uranium, thorium, and actinium. Through a successive series of metamorphoses of one element, another comes out that noticeably differs from the former by both the atomic weight and the place in the Periodic Table.

Hence a number of fundamentally important conclusions may be drawn which have immediate relation to the structure of matter. First, atom is not a simple entity, but, to the contrary, possesses quite complicated inner structure. Second, it is instable and can alter and even break up into its constituents. And third, its interactions with the environment may bring to the loss of particles and the energy allied to them, which inevitably affects the atomic weight as well.

Therefore, atomic weight is not something constant and, all the more, unambiguously determinable, allowing of strictly accurate measurement. This notion from physical research of radioactivity burst into chemistry and entirely changed the priorities in it.

What was before considered impossible came to be quite feasible, and what seemed absolutely necessary turned out needless. The horizon of chemistry got expanded, and chemists began to look at research and the results obtained through another lens. The permanent orientedness to more precise determination of the atomic weights, which was a dominant for a century changed, all of a sudden, into a false guide and an inane mindset wasting the efforts of researchers and holding them away from deepening the understanding of the world.

## 5. THE DETHRONEMENT OF THE HERO

Tectonic shifts at the bottom of physical knowledge transformed also the bedrocks of chemistry. It came out that the chemical elements are not invariable or homogeneous. That each of them possesses its peculiar character and gives unique spectrum response does not mean the uniformity of its atoms. The same element may have "different atomic structure and stability" which manifest themselves when it undergoes radioactive change, "and in some cases also different atomic weights" (Soddy, 1920; 139). This fact in itself contained nothing radically new and did not go far beyond the Daltonian view of the structure of matter. However, it was slipping the chemical mind until radioactive phenomena had plainly demonstrated its reality.

But if there may be disparities in atoms of the same element it should be acknowledged that the very idea of precise determination of the atomic weights as a categorical priority gets completely depreciated. As it has been stated in a comparatively recent review by the Commission on atomic weights and isotopic abundances, "from the classical emphasis on better atomic weights, we have come to the understanding that it is not simply the accuracy of atomic weights that is all-important, but of equal significance is the uncertainty associated with the standard atomic weights" (De Laeter, Bölke, et al., 2003; 694). The "precise values" turn out to be coincidences rather than an expression of a law of nature. In Soddy's words, they are "of as little interest and significance as the determination of the average weight of a collection of bottles" with various degrees of fullness (Soddy, 1932; 50). And therefore, seeking merely to further improve the atomic weights appears to be a futile pursuit not worth resources and efforts spent on it.

But mischance never comes alone. Virtually concurrently with the idea of isotopes, arose the understanding that atomic weight could not play a fundamental role in the chemical picture of the world. The English physicist Henry Moseley, when studying the high-frequency spectra of various elements exposed to roentgen radiation, revealed, by comparing the results presented in graphical form, a striking regularity. Matching with oscillation frequencies (the reciprocals of wavelength) the respective spectral lines, he received as a resultant curve an almost straight line on which different elements turned out to be in points situated at equal distances from each other. That actually represented a convincing way of independent check of the order of the elements in the Periodic Table.

From the analysis carried out by Moseley, at least three conclusions were to be derived. First and foremost, the disposition of the elements on the diagram is identical to one obtained on the principle of chemical analogy, even when it runs counter to the strict sequence of the atomic weights (as in the cases of argon and potassium). Further, all the elements of the rare-earth group find on the curve their own places and, consequently, should take them in the Periodic Table – instead of being put in one and the same place (as proposed by some chemists). And finally, the circumstance that the elements are separated from each other by equal distances clearly indicates the exact numbers and characters of the elements expected to discover. Hence, the atomic number of an element is a more important property than its atomic weight.

Moseley conjectured that "there is in the atom a fundamental quantity, which increases by regular steps as we pass from one element to the next." At that, "this quantity can only be the charge of the central positive nucleus" (Moseley, 1913; 1031). It means that the charge of atom of an element rather than its weight determines the place it occupies in the Periodic Table. That one could, basing on the atomic weights, build up a more or less acceptable, though contradictory and inconsistent, systematics of the chemical elements gets possible only due to a kind of correlation between their atomic weights and charges. Anyway, the significance of atomic weight for chemistry is secondary and derivative; it sheds on the order of the elements but a reflected light.

Lastly, Richards' work had been largely devalued even in the purely technical respect. While it might before be said that, although his results had lost much of their meaning, he developed exact methods of measuring the atomic weights, which are of self-contained importance, by 1919 Aston advanced in the method of positive rays so far as to apply, with all might, his first mass-spectrograph to neon, chlorine, and other elements (Aston, 1948; 55). That device allowed of having mixed the seemingly unmixable. It not merely measured – by the positions of lines on the photoplate – the weight of the atom begetting them, with a precision in no way inferior to that obtained in the best measurements by chemical means but also could establish in a mix of isotopes the atomic weight of each of them severally, instead of the mean value of their atomic weights (Soddy, 1922; 247 - 248). The further development of mass spectrometry made it real to determine atomic masses with accuracy better than one part in 107. And the chemical ("Harvard") method had been entirely supplanted by the "physical" one enabling to calculate the atomic weight of an element through its isotopic composition and the atomic masses of the isotopes it is comprised of (De Laeter, 2009). That marked the end of Richards' epoch.

Thus just in a short period of time atomic weight turned from a fundamental factor determining the properties of the elements into an ordinary parameter. In addition, as a mean value of isotopes entering into the composition of a specimen, an atomic weight could have no exact value. At that, there arose methods enabling, when necessary, to split up the atomic weight found into those of isotopes. That was a pumpkin hour in chemistry, for the public saw a pumpkin rind instead of the fine carriage that impressed them so much. Richards' great achievements made in the course of thirty years hard work got reduced to minor results and could no more attract the same amount of interest as formerly.

The first reaction of the scientific community to the changed situation was confusion. They could not decide how to explain their own shortsightedness. As it got clear that Richards' research was, "by definition," unable to be a great contribution to science, a task sprang up to justify its high appraisal in retrospect. It ran to curious things. In a book containing the outlines of lives and activities of outstanding chemists, which appeared immediately after the peripeties that transformed the chemical mentality, the author tried to make an impression that Nobel Prize had been given to Richards not for his improvements in establishing the "constant" atomic weights on the previous measurements, but for determining the atomic weight of lead of radioactive origin. The account of how the scientist had experimentally confirmed Soddy's conjecture was crowned with words: "This had its award in the Nobel Prize which came to him in that year (1914)" (Harrow, 1920; 75). However, so awkward attempts to wander away from the problem understandably could not have a due effect. And nothing better was found afterwards as to bypass, where possible, the point of Richards' place in the history of chemistry.

The farther in the past the events of the first decades of the twentieth century that overturned the notion of chemical element remained the rarer Richards' name was met in connection with them. It was mentioned either in passing or not mentioned at all, even when the contribution he made, perhaps in spite of his own will and choosing, to the corroboration of the hypothesis of isotopes was touched upon (See: Castelfranchi, 1932; 203 – 204; Findlay, 1948; 222 – 224). As for more recent publications they seem to try to consign Richards' case (and sometimes even his name) to oblivion, as if it never happened. Say, in a book entirely devoted to the history of discovering the chemical elements, and the development of notions related to them, there is absolutely no reference to him (Ede, 2006). Thus the scientific community shifts the responsibility for their inability to establish the real value of a result in the time it is made known on to the shoulders of the researcher. Meanwhile, even if Richards was guilty of something, only of that he shared the common mistake about the atomic weights and their part in chemical cognition.

## BY WAY OF CONCLUSION: APPROVAL OF THE NEW AS SELF-AFFIRMATION OF THE OLD

For a long time, the scientific community supported and encouraged the search for the "absolute" values of parameters which can nowise have definite values. This fitted into the notions of how things function and expectations as to what is to be obtained. But when the "matrix" of prescriptions for the behavior of nature, and forecasts on its metamorphoses underwent a radical transformation the scientific world suddenly got "enlightened" and began to make completely different demands on nature that ensued from the new picture. Therefore, some of the "pieces" of knowledge formerly taken as highly relevant and of exceptional importance for science had been sidelined. And on the contrary, what was not noticed, or ignored, as, for instance, the ideas of the possibility of decay, or "diffuse" character of the atomic weights, came to the fore and got adopted. At that, the measure of consonance with the conceptual background was among the key determinants of the worth of the new knowledge. In this regard, the approval of a finding by the scientific community looks to be, first and foremost, an act of self-affirmation.

From the times of Dalton and Berzelius, a conviction prevailed in chemical mind that all atoms of an element are identical, and the atomic weights are the basis for ordering the elements. It had been only strengthened with the emergence of Prout's hypothesis that induced scientists to believe that the whole world consists, ultimately, of atoms of hydrogen as the prime matter. The hypothesis was supported by both empirical data and their generalizations evidencing the constancy of the atomic weights.

However, the confidence in the hypothesis was evoked by not only and not so much experience as the conceptual background of chemistry. On one hand, it specified the principle of uniformity of nature as applied to the chemical elements and, on the other, allowed of ordering the chaotic variety of experimental material and drawing a rather coherent picture where complex frameworks were built up of elementary blocks. Even those not considering the atomic weights multiples of that of hydrogen and, therefore, not reducing them to a single primal form did not doubt about the exceptional role of the atomic weights in explaining the order of the universe.

First, the atomic weight of an element was regarded as its fundamental property which, depending on no other parameters, conditioned the rest of its features. Second, the place of an element in the Periodic Table which expressed the order existing in the world was determined, primarily, by the atomic weight. And third, the atomic weights were taken to be constant; they should be neither time-varying nor changing as affected by attendant circumstances. In a word, they represented fundamental constants in which the mystery of the origin of the universe and the rules it conforms to were encoded. Hence, their precise measurements must (a) make great objective sense; (b) be quite feasible in practice (experiment), and (c) appear as indispensable for chemical theory, since without them no ordered picture of the elements is thinkable. No wonder that obtaining increasingly better values of the atomic weights was perceived as a key task of chemistry.

However, the immersion in the phenomena of radioactivity radically changed the disposition. Experiment demonstrated that the atomic weights appreciably alter from one specimen of a substance to another. In reality, they are not constants of nature, but, rather, mean values for isotopes the specimen is composed of. Isotopy, in turn, implies an atomic weight to be a derivative of something else rather than fundamental or even a self-contained property. Atom is not a simple entity, but has a very complex inner structure. It is instable and may break up into its parts. The interactions of an atom with the surrounding medium may bring to loss of particles and energy, which inevitably tells on its weight. Moreover, the ordering of the elements should be made according to their positive charges - instead of atomic weights whose role is secondary and subordinate. Consequently, atomic weight is not something invariable and strict that lets in the possibility of precise measurement of its magnitude.

TABLE 1. THE CONCEPTUAL BACKGROUND OF THE DOCTRINE OF CHEMICAL ELEMENT

| THE INITIAL CONCEPTUAL BACKGROUND | | |
|---|---|---|
| THE BASIC TENETS | THE EMBODIMENT IN CHEMICAL KNOWLEDGE | THE GENERAL FOCUS |
| ▪ uniformity of nature<br>▪ an order behind the apparent chaotic variety<br>▪ continuity between the levels of natural phenomena | Prout's hypothesis, very simple and with considerable heuristic potential, served as a specification of the main principles as applied to the available material of chemistry and, therefore, | The atomic weights are constant and play a paramount role in chemical interactions |

| | | |
|---|---|---|
| | actually guided chemical research | |
| **The verdict on Richards' results** | An outstanding achievement revealing the order in the relations between the chemical elements | |
| THE TECTONIC SHIFTS INDUCED BY RADIOACTIVITY | | |
| Atom is not a simple entity; to the contrary, it has a quite complex and complicated inner structure | The interactions of atom with the environment may lead to the loss of particles and energy, which inevitably influences also its weight | Atom is instable and may break into its constituents |
| THE CONCEPTUAL BACKGROUND AFTER THE TRANSFORMATION | | |
| THE KEY DISCOVERIES THAT OVERTURNED THE PREVIOUS CONCEPTUAL BACKGROUND | THE CHANGES IN THE CONCEPTUAL BACKGROUND | THE GENERAL FOCUS |
| ▪ A chemical element is not something immutable and homogeneous. Its atomic weight, being a mean value of isotopes, is a result of coincidence rather than expresses a law of nature<br>▪ The place of a chemical element in the Periodical Table is | ▪ The atomic weights may vary, and they imply no strictly determined values<br>▪ As a property, atomic weight is of secondary and derivative nature and cannot play a self-contained role in chemical interactions | Beyond a certain limit, any further improvement in measuring the atomic weights becomes, in itself, meaningless. And seeking to obtain increasingly more precise values for them, all the more so by means |

| | | |
|---|---|---|
| determined by the positive charge of its atom rather than its atomic weight<br>▪ Mass-spectrograph does not yield to chemical methods of measuring the atomic weights in precision, exceeds them in simplicity, and can establish the atomic weight of each of isotopes | ▪ Chemical methods of measuring the atomic weights give way to spectrography as a more effective means for the same task | of chemical methods, turns into a false guide |
| **The verdict on Richards' results** | A marginal contribution specifying an accessory parameter of the chemical elements | |

Thus, it is the conceptual background that appears to be the touchstone when assessing new knowledge. A "piece" of it is highly valued when increases the worth of the tenets of the scientific field. Even a quite ordinary result may be raised to the rank of an outstanding achievement if it steadily supports the learnt conceptual background. Meanwhile, what cuts across and, all the more, undermines it is taken for something unimportant and unfit or even antiscientific. In the "big science," "unwanted" ideas of this sort are most often suppressed in order to not waste energy on their "unmasking" and refutation (Petrosyan, 2016; 56 – 58). In any case, they get forced out of circulation in every possible way.

Such an attitude towards scientific results is not a product of efforts of individual persons or even their groups. This is a quite natural process, a manifestation of self-preservation instinct. The dominating "matrix" must just impede the consolidation of ideas which can become its "gravediggers," that is to say, are capable of devaluing and discrediting it, and, to the contrary, encourage and further the facts and discoveries it can rely on.

What results are preferred? First of all, those consolidating the conceptual background: clearing up complex and tangled matters, expounding paradoxes, eliminating contradictions and discrepancies, making up deficiencies and filling in gaps. Further, of primary significance is what expands the conceptual background, imparts consistence, coherence, and wholeness to it, and brings the subject field to conformity with findings and generalizations penetrating into it from outside. And finally, worthy of recognition seems to be what bolsters up the conceptual background – reinforces its attitudes and orientations, confirms the consequences it begets, and, eventually, proves its title to domination in scientific mind. But if so, most wanted will be the results securing the conservation and strengthening of the already accepted and wide-spread ideas and conceptions rather than those enabling qualitative transformation of knowledge or its radical progress. What goes far beyond the horizon of the conceptual background is taken with suspicion and apprehension, as something dangerous, destructive, and subverting the basic postulates knowledge is founded on. And only under the influence of fresh tendencies coming from other (mainly related) fields, scientific mind revises half-heartedly the framework of the conceptual background no more able to cope with the challenges.

In a nutshell, the value of a result is rated the higher the more the worth it adds to the conceptual background as the lens through which the result itself is assessed. Understandably, this is a vicious circle. But it is just what lies at the bottom of evaluations practiced in science. And the biggest problem is that it cannot be broken in a simple way.

In case of works directly oriented to practice, the worth of a piece of knowledge may be somehow “measured” with a reference to the importance of the tasks they are called on to take part in. But as regards basic – and especially “inquisitive” (with no immediate projection on practice) – research whose contribution to the development of science (all the more, over a long haul) is of key priority, there can be no linking to specific goals. And, although it is in principle amenable to practical evaluations, their extremely mediated character and the high level of uncertainty in the transition from basic knowledge to its technological or social applications necessarily bring to that the last said in making sense of the prospects goes to the existing conceptual background.

In the area of basic knowledge, the new always is held captive by the old and is measured by the latter’s yardstick. Erroneous assessments turn out to be there not only possible or even probable, but absolutely inevitable. That is why in respect of basic discoveries, relevancy (being in demand) as a criterion of their recognition not merely fails; it becomes useless and frequently even hampers the development of science. With what and how to replace it, is one more key problem. But that is an issue for a special discussion which would be very opportune to initiate.

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