CHAPTER 7

Reassessing the Notion of a Kuhnian Revolution
What Happened in Twentieth-Century Chemistry

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7.1 Introduction

As commentators have almost universally concluded, the work of Thomas Kuhn has had a profound influence on the history and philosophy of science and numerous other fields of study. Kuhn's SSR has been subjected to a plethora of criticisms, which led him to clarify, and in many cases revise, his initial views on most of the key ideas that he famously proposed. Kuhn scholarship continues to flourish and indeed underwent a recent revival following the fiftieth anniversary of the publication of SSR (Richards and Daston, 2016; Devlin and Bokulich, 2015). It may be fair to say that there is now an equally extensive literature addressing the views of the later Kuhn as there is addressing the views of commentators on the first edition of SSR.

The present chapter seeks to explore the change in Kuhn's views on scientific revolutions in particular. I begin by citing the views of Alexander Bird who writes the following passage that I take to be of some significance for what is to follow in the present article:

> But it seems that whatever definition we employ it remains the case that the normal science versus revolutionary science dichotomy cannot do justice to the variety of episodes in science. Kuhn's terminology gives an artificial sense of there being two quite distinct kinds of scientific change. Reflection on this variety suggests that the distribution of episodes is not bimodal but instead shows a greater degree of continuity, with intermediate cases being not especially less frequent than the extremes. (Bird, 2000, 54)

My own analysis of Kuhn's understanding of scientific revolutions will be carried out by narrowing the scope of enquiry to consider the views of the editor of this volume, Brad Wray, who over recent years has developed an interest in the history and philosophy of chemistry. Wray has published a defense of Kuhn's notion of scientific revolution by proposing a possible new revolution that concerns how twentieth-century chemists changed the criterion for identifying a chemical element from atomic weight to that of atomic number.

In a recently published article, Wray claims that chemistry underwent a significant change in theory in the twentieth century and that this represents a "classic textbook case of a Kuhnian revolution" (Wray 2018a, 209). In so doing Wray refers to what he calls a "new conceptual understanding of what it is to be an element." In the present chapter I examine these claims and I revisit the episode that Wray is referring to, in order to see whether it should indeed be regarded as a classic Kuhnian revolution.

I especially want to consider (i) the notion that there was a change in theory, (ii) the idea of a classic Kuhnian revolution, a phrase that contains a good deal of ambiguity, and (iii) the question of a conceptual understanding of elements.

7.2 A Change in Theory

I believe that many scientists might be tempted to dismiss Wray's claim for a change in chemical theory on first hearing of it. Nevertheless, I also believe that his proposal is more intriguing and subtle than may appear at first. However, I find it puzzling that Wray uses the term "classic Kuhnian revolution," since this terminology would seem to invoke the original sense of how Kuhn proposed that scientific revolutions occur. While it may be correct to claim, as Wray does, that chemistry underwent profound changes regarding its underlying theory, I do not believe that the episode that Wray describes has much to do with change in theory as generally understood in science.

Wray's alleged classic revolution concerns the fact that chemists turned away from using atomic weight to order the elements in favor of the use of atomic number. However, this does not appear to be a change in theory by any stretch of the imagination. The theoretical change that was taking place at the start of the twentieth century was rather the abandonment of classical mechanics in favor of quantum mechanics.

According to the generally held account, this program began with Planck's discovery of the quantum of action in 1900,[1] followed by Bohr's application of quantum concepts to the hydrogen atom in 1913. In the same series of articles Bohr also provided a semi-empirical explanation for the

[1] Interestingly, according to Kuhn, Planck continued to treat energy in a continuous manner in his article of 1900.

periodic table by drawing on a judicious mixture of chemical and spectroscopic arguments in order to deduce the electronic configurations of many elements in the periodic table (Bohr 1913). This revolution in theory, if one insists on calling it so, was continued by Sommerfeld's relativistic extension of Bohr's theory and the introduction of elliptical orbits. Next came Pauli's exclusion principle, Heisenberg's uncertainty principle, and Schrödinger's wave equation. Soon afterward quantum mechanics was applied to chemical bonding by Heitler and London and eventually it became the underlying theoretical description for most aspects of chemistry.

Wray does not mention any of these far more revolutionary theoretical changes but instead locates what *he* calls a revolutionary change in theory in the transition from the use of atomic weight for ordering the elements to the use of atomic number. I would like to suggest that this development may be more akin to a change of focus from the entire atom (atomic weight) to just the number of protons (atomic number) rather than any theoretical change.

Wray also associates this change with what he interprets as a Kuhnian anomaly. Wray is referring to the fact that certain pairs of elements such as tellurium and iodine are incorrectly ordered if atomic weight is used, whereas they fall into a chemically correct sequence if one uses atomic number instead. Although I am happy to concede that such pair reversals represented anomalies in the general sense, I do not believe that they represent Kuhnian anomalies, as I will attempt to explain.

Before moving on let me just concede that Wray's proposal seems to have a certain appeal in that it features a clear-cut case of anomalies that were recognized by the chemical community and not merely labeled as such by Kuhn or his followers. This case would also seem to offer a good counterexample to Toulmin's objections that scientific discoveries often occur in the absence of anomalies (Toulmin, 1970). The case that Wray proposes is not subject to such a critique since the change in chemists' thinking was indeed motivated by what chemists themselves referred to as anomalies.

However, there are a total of only four such anomalies in the entire periodic table, even if one considers the modern periodic table that extends up to element 118. At the time that Wray is considering, only two such anomalies were known, namely those involving tellurium and iodine as well as cobalt and nickel.[2] The correlation between atomic weight and atomic number is in fact an extremely good one and as a result these anomalies were not unduly

[2] The other two anomalies feature the elements argon and protactinium, neither of which was known at the beginning of the twentieth century when chemists switched from using atomic weight to using atomic number to order the elements.

troublesome. Moreover, there are no known cases for which the reversal involved anything but immediately adjacent elements.

I suggest that the problem that originally existed in the two mentioned cases was nowhere as pressing as Wray seems to believe. As I. B. Cohen has written, "the profundity of a revolution in science can be gauged by the virulence of conservative attacks as by the radical changes in scientific thought that it produces" (Cohen, 1985, 414). As far as I am aware there were no virulent attacks in any part of the chemical community following the proposal that ordering of the elements should be based on atomic number instead of atomic weights and nor did it involve any radical changes in scientific thought. According to Cohen's view, at the very least, we may not therefore be dealing with a scientific revolution.

As I see it, the anomalies involving the pair reversal of certain elements do not warrant being regarded as Kuhnian anomalies that would result in a scientific revolution. It is a historical fact that many of the discoverers of the periodic system such as Newlands, Odling, Hinrichs, Lothar Meyer, and most famously Mendeleev, all recognized the need to reverse the placement of these elements, although they lacked any fundamental justification for doing so (Scerri 2020, 73–112).

Let us compare this case with what is generally taken to be a genuine Kuhnian anomaly, such as the advance of the perihelion of Mercury that necessitated a genuine Kuhnian revolution before it could be resolved. It is not so much the small differences in atomic weight connected with the pair reversals that make me doubt their possible status as Kuhnian anomalies. After all, the perihelion case also amounted to an extremely small departure from what could be calculated from the then available Newtonian mechanics. It is more the fact that pair reversals were readily tamed and did not demand any theoretical changes.

Of course, it was gratifying when the justification for pair reversal was provided as a result of the discovery of atomic number. However, neither chemists nor physicists lost much sleep over the matter during the fifty or so intervening years between the discovery of the periodic table and the change to atomic number as the more correct ordering principle.

Pair reversals certainly never amounted to a Kuhnian crisis as I see it. In any case, there was absolutely no change in the order in which any element was presented in the periodic table, either before or after the adoption of atomic number. All that happened was that the correct insight of chemists that, for example, tellurium should precede iodine, was given a physical basis following the move to using atomic number. One might say that physicists were able to catch up with what chemists already knew, solely on the grounds of chemical reactivity. These historical circumstances, and especially the complete lack of any change to the periodic table before and after the introduction of atomic number, do not bode well for those who wish to view this development as a scientific revolution.

In ending this section, we should also recall that Kuhn himself was careful to note the variety of scientific discoveries and whether they might count as examples of his way of characterizing scientific change. According to Kuhn the discovery of X-rays provoked resistance but not a crisis, and the revision was easily assimilated without any struggle between competing paradigms. As Kuhn also wrote, "if an anomaly is to evoke crisis, it must be more than just an anomaly" (SSR-1, 82). I believe that the case being considered in the present article would also be considered as rather marginal by Kuhn since it invoked neither resistance nor any form of struggle between paradigms.

The other anomaly that Wray discusses concerns the discovery of isotopes, that is to say forms of the same element having different atomic weights, which I will turn to in due course.

7.3 What Is a Classic Kuhnian Revolution?

A Kuhnian revolution in the way in which it is generally understood involves the notions of paradigm, anomalies, crises, and revolution all leading to a new paradigm. As is well known, this account met with a great deal of resistance from many critics from the time that Kuhn first published it (Scheffler 1967; Shapere 1964; Toulmin 1972). In particular Kuhn was criticized for his notion of paradigms and for claiming that the change from one paradigm to a subsequent one was associated with incommensurability or an inability of scientists from opposing paradigms to even communicate with each other.

The Kuhnian revolution that Wray proposes and describes as being "classic" would not in fact seem to be classic since it lacks most of the qualities that Kuhn assigns to scientific revolutions. I can only conclude that Wray is not referring to the writings of the "classic Kuhn" but to his later writings. Or as Wray explains Kuhn's later view, "[i]nstead of referring to theories as paradigms, [Kuhn] came to believe that theories are scientific lexicons. Each theory is a scientific vocabulary that orders the relevant concepts in specific ways, with very precise relationships between the concepts." (Wray 2018a, 210)

As is well known, in more than thirty years following the publication of his 1962 book, Kuhn constantly revised many of the central themes in his philosophical account of theory change. For example, Kuhn changed the

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meaning of the term paradigm in such a way that it came to mean the work of a far more restricted group of scientists than he had originally suggested. Similarly, the term "scientific revolution" changed because, in his lexical turn, Kuhn turned his attention to the language that scientists use.

Wray has been a leading contributor to the field of Kuhn scholarship and is far better acquainted than I am with the twists and turns in Kuhn's later thinking about science and how he may have revised his seemingly very "revolutionary" views set out in his book of 1962.

I therefore find it a little surprising that Wray persists in using the phrase "classic Kuhnian revolution" thus blurring the issue of whether he means the original Kuhnian account or the highly revised account following Kuhn's later turn to the lexicon of science.

My quick gloss on this issue would be to say that if the phrase is intended in the earlier Kuhnian sense, then I completely disagree with claiming that the change from atomic weight to atomic number represents such a revolution. If on the other hand Wray intends the phrase "classic Kuhnian revolution" in the later sense, in which all attention is directed at the language and terms used by proponents of rival paradigms, then he may have a stronger case, although I will have some further comments on this possible interpretation.

7.4 Brief Digression: Vogt and Kragh's Views of the Same Scientific Episode

Wray is not the only author to have suggested that the change from atomic weight to atomic number represents a Kuhn-style revolution. In a recent book review the chemist Thomas Vogt seems to believe that the change in the definition of an element that occurred as a result of the work of Van den Broek and the isotope crisis was "radical" and that it constituted a scientific revolution (Vogt 2017, 108). He then says, "The resolution of this 'isotope crisis' during the first 25 years of the 20th century had all the scientific, historical, and political complexities of a scientific revolution and is described in detail by Kragh (2000) . . . After this scientific revolution chemists never saw Nature at the microphysical level as before." (Vogt 2017, 108)

It is rather unfortunate that Vogt should have chosen this particular source since Kragh does not seem to consider the change in the understanding of what constitutes an element, and the discovery of isotopes, to have been a revolution in the Kuhnian sense.[3] As Kragh explains:

[3] Vogt makes no reference whatsoever to the work of the later Kuhn such that one can only suppose that he is referring to the classic and well-known earlier Kuhnian account.

> Great theoretical changes occurred during the period, but these did not lead to a wholesale refutation of older chemical concepts such as the periodic table and the notion of an element. The periodic system survived the revolution and although the chemical element was reconceptualized it occurred in such a way that continuity with the older definition was secured. (Kragh 2000, 447)

Admittedly Kragh uses the word "revolution," which might be why Vogt chose to cite him. However, it should be noted how Kragh describes this change in rather muted and very un-Kuhnian terms. According to Kragh there was no "wholesale refutation of the older chemical concepts," and the reconceptualization occurred in a way that secured "continuity with the older definition." Whatever kind of revolution Kragh might be referring to it does not seem to resemble a revolution as envisaged by the earlier Kuhn. A few lines later Kragh writes,

> Neither quantum mechanics nor the proton-neutron model of the nucleus necessitated further changes. The element and the periodic system are thus examples of conceptually robust chemical entities. Their histories indicate the force of the pragmatic chemical viewpoint and the value of retaining older theoretical notions, at least in a correspondence-like manner and up to a point. The reinterpretation of the element that occurred in the period kept the connection with the older concept through the principle of conservation of the elements in all chemical transformations. (Kragh 2000, 448)

Contrary to Vogt and more recently Wray's view of a Kuhnian style revolution in the concept of an element, Kragh emphasizes "conceptual robustness," "the value of retaining older theoretical notions" and keeping a connection with the older concept of an element. Indeed, Kragh also draws support from the work of the historian Mary Jo Nye and agrees with her that "chemistry and physics were beginning to share disciplinary terrain" (Nye 1989, 448). This too does not sound like a case of Kuhnian incommensurability associated with a scientific revolution. Indeed, the final sentence of Kragh's article emphasizes this point even further. "To Aston and many of his colleagues, there were no fundamental disagreements between physics and chemistry, only different ways of conceptualization and presentation" (Kragh 2000, 448).

However, unlike Vogt, I believe that Wray may be claiming that this episode is a revolution in the sense of Kuhn's later writings, to which I turn more fully in the following sections.

7.5 Returning to Wray's Own Proposed Revolution

Following the preliminary comments mentioned earlier, I now examine Wray's article in greater detail. It would appear that Wray is equivocating on the sense in which he is interpreting a "Kuhnian revolution." As I have already suggested, by calling the episode under discussion a "classic textbook case" Wray would seem to be referring to the original and generally well-known sense intended by Kuhn. It may also be worth noting in passing that what the later Kuhn thought about scientific revolutions has not in fact permeated science textbooks.[4]

The impression that Wray might be referring to the original sense of a Kuhnian revolution is further strengthened on hearing that the example that he proposes to discuss represents "another case." This too would seem to point to the classic cases of scientific revolutions that Kuhn discussed in his classic book of 1962.

In the opening remarks of the article that I am discussing, Wray states that

> [t]he aim of this paper is to provide an analysis of the discovery of atomic number and its effects on chemistry. The paper aims to show that this is a classic textbook case of a Kuhnian scientific revolution. The analysis serves two purposes. First, it provides another case of a Kuhnian revolution, thus offering support for Thomas Kuhn's theory of scientific change, which has been subjected to criticism on an ongoing basis. (Wray 2018a, 209)

Wray immediately follows these remarks by the statement that the change in the meaning of an element "provides a compelling unifying narrative of some of the most important research in chemistry in the early twentieth century" (Wray 2018a, 209). Although I have not conducted a poll among my colleagues in the chemistry department at UCLA, I think I can safely say that the change from using atomic weight to using atomic number, and the change in the way an element is defined, would be very unlikely to make a list of "the most important research in chemistry in the early twentieth century."

Wray explains further how Kuhn's original understanding of paradigm changed in his later writings:

> Kuhn regarded this new characterization of theory change as merely a clarification of his original account, presented in *Structure*. Even he

[4] Not only has the later Kuhn not entered into science textbooks, but even the earlier Kuhn is very seldom, if ever, mentioned. Scientific textbooks remain steeped in logical positivism. A casual survey of about fifty chemistry textbooks that I have conducted confirms this point. Perhaps the situation is different in physics education but I doubt it.

> came to realize that the notion of a paradigm was far from clear. In his later writings, the term "paradigm" was reserved for those specific scientific accomplishments that become templates for solving other related outstanding scientific problems. (Wray 2018a, 211)

However, it would appear that the notion of a crisis, another classic term from the earlier and better-known Kuhn, is retained in his later writings. As Wray explains: "Much of the apparatus associated with the earlier paradigm-related notion of revolutionary theory change was retained in Kuhn's more recently developed lexical change model of theory change. For example, he continued to believe that anomalies played a crucial role in the process of scientific change that ultimately lead to revolutionary changes of theory." (Wray 2018a, 212)

I highlight this point because I do not think that Wray provides much evidence for the occurrence of any crises. Indeed, if anything Wray's account appears to highlight the protracted, piecemeal, and disparate contributions that led up to the change from the use of atomic weight to that of atomic number in order to identify chemical elements.

Wray proceeds to give a historical summary of nineteenth-century chemistry, especially as it pertains to the discovery of elements and the measurement of their atomic weights, leading up to the discovery of the periodic system in the 1860s.

According to Wray, who prefers to regard historical events through the lens of Kuhn, the occurrence of pair reversals represented the first important anomaly that would eventually precipitate what he nominates as a classic Kuhnian revolution. In making this claim he accepts that the early discoverers of the periodic system were perfectly capable of dealing with this anomaly. Wray writes,

> Anomalous phenomena have to be dealt with in some way, and Mendeleev and other chemists felt that such a solution was reasonable, given their knowledge of the properties of the various elements. Other pairs of elements had also posed similar problems for chemists, as long as they assumed that atomic weight was the key to classifying chemical elements, including, for example, potassium and argon, and cobalt and nickel.[5]
>
> Anomalies provide the research topics in a normal scientific tradition. And scientists sometimes choose to set some anomalies aside, to await the efforts of future scientists who may be better equipped, conceptually and technologically, to tackle the problems. Ultimately, though, these specific

[5] As a matter of fact, the pair reversal involving argon was not an issue for the discoverers of the periodic table for the simple reason that this element was not discovered until 1894.

> anomalies did contribute to bringing about a radical change of theory in chemistry. (Wray 2018a, 213)

It would appear that Wray is claiming that pair reversals, a situation that did not constitute a crisis in chemistry, nevertheless would ultimately contribute to a revolution. Wray then makes a further claim in support of a classic Kuhnian revolution when writing:

> The result, though, was a radical change of theory of just the sort that Kuhn would regard as a revolutionary change of theory.
>
> Perhaps most significant in this process was the discovery of atomic number ... The discovery follows the pattern that Kuhn identifies in *Structure*, in his analysis of the discovery of oxygen (see Kuhn 1962/2012, pp. 53–57). Consequently, it is fruitless and futile to attempt to pinpoint who discovered atomic number, and when exactly the discovery was made. (Wray 2018a, 214)

These statements may contain a contradiction as well as a factual error. If the discovery in question was indeed similar to that of oxygen, namely gradual, and not attributable to any particular event, it raises the question of whether this can be regarded as a radical change. Secondly, if ever there was a scientific discovery that could be pin-pointed rather accurately it would indeed be that of atomic number. There is little doubt among historians of science that the theoretical groundwork was laid by the Dutch econometrician and amateur scientist Anton Van den Broek, while the experimental confirmation was supplied by the English physicist Henry Moseley (Scerri 2018).[6]

Wray continues by discussing the discovery of isotopes and how they initially presented an additional anomaly to the pair reversal of certain elements. In highlighting the importance of the discovery of atomic number as providing a better means of identifying any element, Wray also explains how this development helped scientists to isolate the seven elements that remained to be discovered within the old boundaries of the periodic table.[7] In this instance I believe that he may be overstating his case.

Although focusing on atomic number and its characterization via X-ray spectra did serve to direct the search for these elements, it by no means

[6] Wray mentions the contribution of Van den Broek but is under the misapprehension that Van den Broek carried out some "laboratory work." In fact, all of Van den Broek's key articles were of a completely theoretical nature.

[7] I am referring to the limits that encompassed the original sequence of naturally occurring elements from H for which Z = 1 to U for which Z = 92.

settled the numerous claims that were made over a thirty or so years period, until they were all discovered or synthesized.[8] In fact, in several instances the scientists concerned claimed to provide X-ray data to support what they believed to be the isolation of one of these missing elements, only to be refuted by further research on the part of others (Scerri 2013, 183–185).

7.6 The Dual Sense of the Concept of Element

I believe that there is a missing central theme that is highly relevant to Wray's claim regarding the change in the understanding of an element as being defined by its atomic number rather than by its atomic weight. I am referring to a long-standing discussion in the philosophy of chemistry literature concerning the dual meaning of the term "element." An element can be regarded in the sense of Lavoisier as the last stage in chemical composition of a compound. This sense of element was called a simple substance by Lavoisier and was highly influential in the course of what is generally regarded as *the* Chemical Revolution that was centered around his work.

The other sense of "element" is sometimes labeled as element as a "basic substance," meaning as a fundamental substance that underlies all the properties that an element manifests as a simple substance (Scerri and Ghibaudi, 2020).[9] It is this sense of element that survives when simple substances enter into chemical combination to form compounds. Sodium chloride, to take an example, does not contain sodium and chlorine as simple substances but only as basic substances. This distinction was revived by none other than Mendeleev, the leading discoverer of the periodic table, who went as far as to say that the periodic table was primarily a classification of elements as basic substances and not simple substances.

> It is useful in this sense to make a clear distinction between the conception of an element as a separate homogeneous substance, and as a material but invisible part of a compound. Mercury oxide does not contain two simple bodies, a gas and a metal, but two elements, mercury and oxygen, which, when free, are a gas and a metal. Neither mercury as a metal nor oxygen as

[8] Wray claims that the rate at which these elements were discovered was quite striking. This is not the case since it required a period of about thirty years between the discovery of protactinium and the synthesis of promethium. The discovery of the seven missing elements owes as much if not more to the development of new technologies such as particle accelerators in which five of the seven elements in question were artificially synthesized.

[9] This terminology is due to the translation of Paneth's paper into English as carried out by his son Heinz Post. Some authors have objected to the use of the word "substance" in the definition of both senses of the concept of "element" (Earley, 2005).

> a gas is contained in mercury oxide; it only contains the substance of the elements, just as steam only contains the substance of ice, but not ice itself, or as corn contains the substance of the seed but not the seed itself. (Mendeleev 1891, 23)

In this way Mendeleev was able to explain that the only two allotropes of carbon that existed in his era, namely diamond and graphite, both belonged in the same position in the periodic table. Said in other words, the position for carbon in the periodic table does not represent diamond or graphite but carbon in general or carbon in the more abstract sense of a "basic substance."

This issue is of great importance to the scientific episode that Wray is proposing to call a scientific revolution. When isotopes were first discovered they did indeed pose something of an existential threat to the periodic table because it appeared as though the number of elements was very rapidly proliferating and it was by no means clear where, if at all, they should be accommodated into the periodic table.

The person who was responsible for solving the problem was the philosophically astute radio-chemist Friedrich Paneth (Paneth 1962). It was Paneth who provided the philosophical and conceptual underpinning that paved the way for the new official definition of an element that was adopted by the International Union of Pure & Applied Chemistry (IUPAC) in the 1920s. I do not believe that such a change was primarily one concerning the lexicon of chemistry. It was rather a fundamental reinterpretation and rationalization of the isotopes that were being discovered. Moreover, Paneth realized that the periodic table could safely ignore this apparent proliferation since it was focused on elements as basic substances, as characterized by their atomic numbers.

Let me turn to another aspect that I believe Wray fails to address. When Kuhn revised his earlier views on scientific revolutions in order to focus on the lexicons in science, he also turned from the consideration of diachronic changes to those that were synchronic and involving the diversification of subdisciplines. What Wray appears to be doing is adopting Kuhn's later definitions of paradigm and revolution while still considering the development of science across time. For the later Kuhn, paradigms take on a positive role in the form of the development of subdisciplines and specialties, as Wray has eloquently written about in his book on Kuhn (Wray 2011). Kuhn compares the diversification of scientific fields to that of biological speciation. As Kuhn sees it, the form of incommensurability that develops when two disciplines branch off from each other is actually productive. Divergences in lexical taxonomies are supposed to lead to communication difficulties among members of rival paradigms and the later Kuhn believes that such isolation is favorable to the growth of science.

It is by no means obvious how this aspect of the later Kuhn's thinking is supposed to apply to Wray's example since, as I have stated earlier, it was not so much a matter of changing the meaning of "element" as one of favoring a meaning that had been largely neglected since the time of Mendeleev. It was also a matter of focusing on the nucleus of any element rather than the atom as a whole, as I explained earlier.

For the later Kuhn speciation, and the resulting isolation, enables scientific disciplines to specialize and science to grow as a result. Wray makes no such argument for what he is claiming to be a scientific revolution, in the sense of the later Kuhn, regarding the change from atomic weight to atomic number. It would seem that he has concentrated on one aspect of the later Kuhn's view, namely lexical changes, while not also adopting the focus that Kuhn recommended on synchronic changes such as those involving the diversification of scientific subdisciplines, that is to say, the positive slant that Kuhn gives to his new interpretation of incommensurability. In this respect Wray may be supporting a rather outdated Kuhnian view while correctly reporting that the newer conception of elements represents an improvement on the view in which elements were characterized by their atomic weights.

7.7 Kuhn's Non-Overlap Principle

In order to explain his revised view of incommensurability and the new sense of scientific revolutions in terms of changes in lexicon, Kuhn also proposed his no-overlap principle in the course of an address delivered to the Philosophy of Science Association in 1990 (Kuhn 1990).

According to Kuhn there is a revolution if there is no "overlap" between two kind terms such as the term "planet" as it was used by Ptolemaic and Copernican astronomers respectively. For Ptolemy, and his followers, the Sun was regarded as a planet in view of its apparent wandering motion in the sky. On the other hand, the Earth was not regarded as a planet. For Copernicus, and his supporters, it was the other way around since the Sun was not thought of as a planet whereas the Earth was.

Although Kuhn never presented diagrams to depict this state of affairs I will attempt to do so later for the sake of clarity (Figure 7.1).[10]

The intersection of the two sets contains the celestial bodies that both groups of astronomers regarded as planets. In the everyday sense of the word, one could say that there is considerable overlap among the members of these two sets of celestial bodies. The problematic cases clearly consist of the Sun, the Earth, and the Moon. However, I do not see that these three anomalies would present serious difficulties.

On a strong reading of Kuhn, a lack of overlap would seem to indicate a lack of complete overlap since the kind terms involved in the taxonomy can only stand in two relations, namely exclusion or inclusion. Partial overlap between contrasting kinds is forbidden in that something is either a planet or a star but not both.

I now turn to how Wray puts Kuhn's no-overlap principle to use in arguing for his claim that the change in the definition of an element represented a scientific revolution. Wray says that if the defining feature of a chemical element is its atomic weight, then isotopes are strictly speaking impossible or forbidden. According to Wray, the earlier lexicon regarding elements was incompatible with the very possibility of two

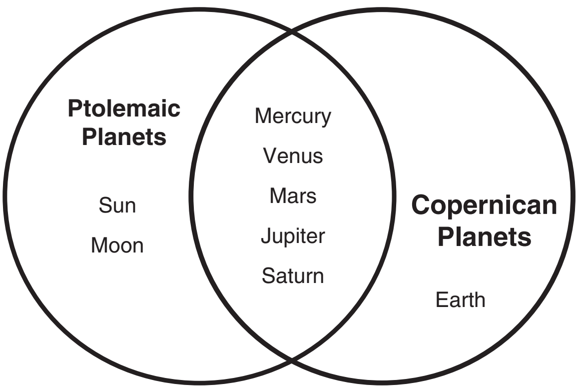

Figure 7.1 Comparison of the extension of the term "planet" in the Ptolemaic theory and the Copernican theory. Both groups of astronomers regarded Mercury, Venus, Mars, Jupiter, and Saturn as planets.

[10] As Wray tells me, there is very little written by Kuhn on the no-overlap principle. Many authors and commentators on Kuhn appear to merely repeat what Kuhn said using the same examples that include cats and dogs as well as gold and silver rings, between which pairs Kuhn also claims that no-overlap exists.

samples of the same element having different atomic weights. The only way to accommodate the possibility of isotopes, continues Wray, is to radically change the lexicon by defining elements through their atomic numbers instead of their weights. As a result, samples of the same element need not necessarily have the same atomic weight.

However, I do not think that the two cases, Kuhn's example of planets and Wray's example of elements, are analogous, because any substance that has an atomic weight also has an atomic number. Consider the classic example of the two major isotopes of chlorine, namely ^{35}Cl and ^{37}Cl. Any atom of ^{35}Cl contains its atomic number within itself, as it were, since the nucleus of this isotope contains seventeen protons. Similarly, every atom of the other isotope ^{37}Cl also contains a nucleus with seventeen protons. An atom cannot be an isotope of any particular element without containing within itself the feature that identifies it as an isotope of that particular element, namely its requisite number of protons.

The simple reason for this state of affairs is that ontologically speaking atomic number is fully contained within the weight of any particular isotope. Stated otherwise, atomic number is associated with just the protons in an atom, whereas atomic weight is due to the number of protons plus the number of neutrons and electrons. Focusing on atomic number, as is the case in the modern definition of an element, implies focusing on a more specific aspect of the same ontological entities, or the atoms themselves, regardless of whether they might be the same or different isotopes of any particular element.

I now return to my Venn diagrams, this time applied to elements, atomic number and atomic weight, and the relationship between them. One way to represent this relationship is shown in Figure 7.2.

So even if Kuhn may be correct in insisting that there is no *complete* overlap in the case of planets as viewed through the two lexicons, Wray may not be entitled to make an analogous argument in the case of elements viewed through the lexicon of atomic number as opposed to atomic weight.

Not only is there overlap in the element case but there is also complete containment of one set within the larger one, ontologically speaking. To cope with the discovery of isotopes scientists were required to sharpen their focus and their definition of element-hood. They were not required to exclude certain instances of previously regarded elements. There was no counterpart to Ptolemy regarding the Sun as a planet that then became reclassified as a star in the subsequent lexicon. This is why I believe that the two situations are not analogous. As a result, and in

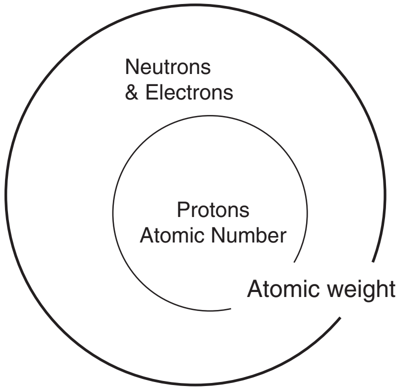

Figure 7.2 Venn diagram of the relationship between atomic number and atomic weight. Atomic number (protons) is fully contained within atomic weight (protons plus neutrons and electrons) for any particular isotope.

the terms of the later Kuhn, the change in lexicon from using atomic weight to using atomic number to define elements does not, in my view, constitute a Kuhnian revolution, neither classic nor in the sense of the later Kuhn.

7.8 Conclusion

Contrary to Wray's ingenious proposal I do not share the view that the discovery of atomic number as the ordering principle for the elements represented a scientific revolution. As I have argued, it was certainly not a revolution in the sense of Kuhn's original account in SSR. Moreover, I do not believe that it was a revolution in the sense of the later writings of Kuhn in which attention shifts to the scientific lexicon, the diversification of scientific fields, and the no-overlap principle.

Finally, I must raise another issue that occurred to me after I had essentially completed this article. In all that I have written here, I have examined Wray's view that the adoption of atomic number by the chemical community represented a scientific revolution. But there are really two issues at play here. First there is the question of whether Wray has correctly identified a historical episode that has the characteristics of what Kuhn might consider to be a scientific revolution. Broadly speaking I believe he has done so. The other question is whether this episode does indeed represent a scientific revolution *tout court*, regardless of Kuhn's views. I am a little worried that I may have allowed my deep-seated disagreement with Kuhn, specifically on revolutions, to be transferred to one of his followers, namely Brad Wray (Scerri, 2016). Needless to say, this is somewhat inevitable since Kuhn is no longer with us, and all I can do is to direct my comments at one of his finest living expositors. Thank you, Brad Wray, for graciously allowing me to contribute to this volume. If my chapter appears like a fish out of water, it may be because I choose to focus on the scientific details. In the final analysis I would hope that Kuhn might approve of this course of action.