A new response to Wray and an attempt to widen the conversation

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Abstract

This article begins by examining a recent claim by Brad Wray that the discovery of atomic number and isotopy constitutes a scientific revolution in the sense of the later writings of Thomas Kuhn. I argue that although Kuhn's criteria may apply to the change from the Ptolemaic to the Copernican model of the universe, they do not apply in the above chemical or atomic case. I also examine the wider issue of Kuhn's turning away from internal scientific issues to a consideration of lexical issues. I conclude, as others have done before me, that this may have been a wrong turn in view of the emphasis being placed on questions of sense rather than reference.

1. Introduction

In teaching introductory philosophy of science, one makes a distinction between Popper and Kuhn and the fact that for Popper a decisive refutation such as the discovery of black swans is supposed to lead to the abandonment of the 'law', that all swans are white, provided there are no non ad-hoc moves that can rescue the theory.

By contrast Kuhn's account is said to be more permissive because it allows for the occurrence of anomalies, although these events do not cause the sudden downfall of the paradigm. One needs to wait for more anomalies, which eventually lead to a crisis, a revolution, and eventually the establishment of a new paradigm. An important aspect of this scenario is that there need to be several anomalies.[1]

In the case of the periodic table there were just two anomalies in which ordering the elements according to their atomic weights failed to classify a total of four elements in their correct groups, as revealed through their chemical properties. These so-called pair reversals consisted of the more significant case of the elements tellurium and iodine with an atomic weight weight difference of 0.7 atomic weight units and the nickel cobalt anomaly (0.2 units). This situation clearly did not constitute a scientific crisis, on its way to becoming a scientific revolution in the sense of Kuhn's early account.[2]

The modification made by Mendeleev and other discoverers of the periodic table of reversing the positions of tellurium and iodine as well as of cobalt and nickel was not ad-hoc, since it accommodated the known properties of these elements. The paradigm of the periodic table was rescued successfully, in that all the other elements could still be ordered according to increasing values of atomic weight.

The anomalies that Mendeleev and his contemporaries experienced eventually led others to discover isotopes, rather than refuting or revolutionizing the periodic table. Similarly, the discovery of numerous radioisotopes in the early 20th century did not lead to the downfall of chemistry's central paradigm of the periodic table.

In the Copernican revolution however, it was not just a matter of one or two planets not orbiting as they should, but a major turning inside-out of the prevailing geocentric paradigm. Later on, one or two planets were found to have anomalous orbits. This fact did not lead to an overthrow of the Copernican paradigm but indeed to the successful prediction of the planet Uranus. So much, for the time being, for the way that Kuhn originally envisages scientific revolutions.

The philosopher of science Brad Wray has proposed that the discovery of atomic number and change in the manner that elements were defined represented a scientific revolution (Wray, 2018).[3] However, as I previously responded, once the focus had been narrowed from protons and neutrons to just protons (from atomic weight to atomic number) everything fell into place and there was no revolution to speak of (Scerri, 2021).[4] As I see it, science develops via a process of greater focus, greater specialization[5] and looking at increasingly more microscopic components. For example, the major changes in modern biology and chemistry have come about due to a focus on DNA and the electron, in biology and chemistry respectively. Science does not progress by merely changing the manner in which scientific entities like planets and elements are defined. Science is more about ontology than about the manner in which human beings classify the world. Of course, our concepts can prejudice what we observe, experiments are theory-laden and so on. But one need not go overboard in thinking that scientific discoveries cannot occur until the appropriate terminology is available.

I suggest that Kuhn may have been wrong to place such a big emphasis on scientific lexicon in his later work.[6] Such a step may have been motivated by needing to respond to his many critics, but he may have thereby taken a step away from what matters most in scientific practice.[7]

Regardless of whether it may be a revolution in the later Kuhnian sense, what is more important, or perhaps more interesting, is the question of whether the change from atomic weight to atomic numbering ordering and the related change in the definition of an element is a revolution in a broad sense that other philosophers of science or even scientists themselves might accept. The answer to this latter question must be a resounding no, in my view. Neither the change from the use of atomic weight to using atomic number for ordering the elements, nor the way that the term “element” should be understood, represented a scientific revolution in this broader sense.[8]

2. Are there *any* revolutions in chemistry?

In the field of physics there have clearly been some developments which one might want to identify as being of a revolutionary nature. One need only think of Einstein’s special and the later general theory of relativity. In addition, the development of quantum mechanics can rightly be considered to have been a major scientific revolution in many respects. In biology one may speak of the Darwinian revolution whereby all living creatures, and indeed also plants, became regarded as having descended from a common origin.

Has the field of chemistry experienced anything as remotely momentous as these revolutions? I believe not, apart from what is generally called the Chemical Revolution, which is mainly attributed to the work of Lavoisier, although even in this case there are many who doubt whether it may have been a genuine revolution (Blumenthal, 2013).

Indeed, the lack of the existence of a philosophy of chemistry, which persisted until relatively recently, can perhaps be attributed to the lack of any major revolution that could compare with the above-named examples from physics and biology.[9] The periodic table, which is undoubtedly one of the paradigms of modern chemistry, has stood for over 150 years since its discovery in the 1860s. There has yet to be, I claim, anything resembling a revolution in post-Lavoisier modern chemistry.

I believe this general background is important when weighing Brad Wray’s proposal that the discovery of atomic number, isotopy and the new way of identifying elements, that took place in the 1910s and 1920s should be regarded as any kind of scientific revolution.

3. Wray's attempt to draw an analogy between the Copernican revolution and the events that took place in chemistry in the 1910s and 1920s.

Wray begins by explaining that before Copernicus, all bodies observed in the night sky were regarded as stars, except those that wandered, which were said to be planets. Following the Copernican revolution, the Earth, Sun and Moon ceased being identified as planets. Of course, they still continued to 'wander' but they became deprived of their planetary status.

In Mendeleev's time there were about 60 elements which shared the characteristic of each possessing a unique atomic weight. Notice that there is no analogous contrast between stars and planets in the chemical case in question. All the observed microscopic entities were classified were classified as belonging to one kind, namely elements. This is the first of what I take to be dis-analogies to the astronomical case that was just discussed.

Following the discovery of atomic number by Moseley, and of isotopes by Soddy, some observed chemical entities with particular atomic weights were no longer classed as elements. This episode is taken by Wray as being a significant analogy to the change accompanying the Ptolemaic and Copernican view of planets. However, this attempt fails in the chemical case because one could equally well say that all isotopes were now regarded as having unique atomic weights, while some of these weights also corresponded to the weights of elements. I am referring to the not insignificant number of elements which are mono-isotopic.[10] For example, the element iodine only has one isotope. The atomic weight of this isotope thus succeeds in identifying this element and in distinguishing it from all other elements.

Returning to the astronomical case, some of the observed objects, namely the Sun, Moon and Earth changed their status and ceased being planets. In the chemical case some of the detected microscopic entities characterized through their atomic weights ceased being identified as elements. However, in the astronomical case the status of planethood and non-planethood are mutually exclusive. In the chemical case, some of the thousans of microscopic entities whose weights have been determined ceased being regarded as distinct elements, but by no means all of them. Being an isotope and being a distinct element are not mutually exclusive. The isotopes of monoisotopic elements are both members of the general class of isotopes but also members of the class of isotopes which happen to also count as elements in their own right.

This is precisely the kind of overlap that Wray does not seem to be aware of when he claims that this chemical case represents a violaion of Kuhn's no-overlap principle. As I have just explained, it is simply not the case that atomic weight per se fails to identify all elements. A single isotope of iodine, to return to the same example, can be identified with the only microscopic particles of the element iodine that exist.

Yet a third dis-analogy has to do with the fact that the term planet is not a natural kind but more of a conventional label assigned by popular consent. One only needs to consider the notorious 'Pluto affair' that took place in the year 2016, when the International Astronomical Union ruled that Pluto was no longer a planet because of some of the characteristics of its orbital motion (Bokulich, 2014).

No similar ambiguity exists regarding what is, or is not, an isotope of any element. If a microscopic atomic entity has a unique mass, it counts as an isotope. Similarly, each element has a unique atomic number. If an atom is found to have a particular atomic number this identifies it as one of the currently 118 known elements. Conversely, the identification of any given element, such as gold for example, is uniquely associated with having an atomic number of 79. Said otherwise, the possession of a particular atomic number is both necessary and sufficient for identifying any particular element. None of this kind of precision applies to the conventionally stipulated term of planet. Simply put, elements are natural kinds whereas planets are not.[11]

4. The original Kuhn and the later Kuhn

The refinement in the meaning of a paradigm that took place in Kuhn's later work is not supposed to dismiss the original view, a feature that Wray seems to agree with. The two Kuhnian senses of what constitutes a scientific revolution are not radically different. Kuhn's later understanding of a revolution, as Brad Wray concedes, is only meant to be a refinement of his earlier one.

> In reconceptualizing the notion of a scientific revolution, Kuhn was not intending to change his view fundamentally. Rather, he regarded his later reconceptualization as a refinement of the view presented in Structure. Thus, he thought of the new definition as picking out the same sorts of changes that he identified as “paradigm changes” in Structure (Wray, 2022).

However, the way that Wray portrays matters suggests that there is a little by way of intersection between the earlier and later Kuhn views, except perhaps for the case of the Copernican revolution. And even in this case, on Wray’s reading we are invited to believe that the real revolution is not the simple fact that the earth and other planets circle the Sun, but rather the far less important point that the Earth, Sun and Moon and no longer classified as planets.

As some Kuhn scholars have written, the more important difference between the Ptolemaic and the Copernican paradigms had more to do with comets than with the reassessement of whether any particular celestial body was a planet or not (Andersen, Barker, Chen, 2009).

There is presumably no sense in which scientific revolutions according to the early and the later Kuhn can be considered as incommensurable or said to be populating different worlds. I take it for granted that Kuhn did not wish to claim that his youthful and later selves inhabited radically different worlds.

More importantly perhaps, it appears that for the later Kuhn, the paradigm no longer concerns the ontological question of what objects moves around which other object, but a terminological question of whether to call the sun, for example, a planet or not. But the question of terminology belongs in the realm of human construction, regardless of whether we are speaking of planets or elements. What matters more is the behavior of these entities. In the case of atomic weight and atomic number what matters is whether one concentrates on the proton (atomic number) or on the whole atom (atomic weight). It is more a matter of reference than of sense, or a matter of extension rather than of intension.

5. Specific responses to Wray's recent article

In an article published in 2022 Brad Wray returns to our debate concerning whether the discovery of atomic number and isotopes constitutes a scientific revolution in the sense of Thomas Kuhn's later views. In his opening remarks Wray writes,

> …one reason Scerri and I have different views about this particular case in the history of chemistry is that we are not attending to the same Kuhnian account of scientific revolutions (wray, 2022).

I find this statement rather odd, given that I went to great lengths to examine Wray's claim in the light of Kuhn's later, as well as his earlier accounts of scientific revolutions and concluded that he was referring to the later view (Scerri, 2021)

Wray returns to this point a little later and says,

> I have said that the revolution in twentieth Century chemistry is a "classic" Kuhnian revolution, and Scerri is critical of this claim (see Scerri 2021, 7.3). This, I think, is simply a verbal dispute. Kuhn's later account of scientific revolutions (see Kuhn 2000), the one I draw on, is somewhat different from the account he presents in his 1962 classic, *The Structure of Scientific Revolutions* (see Kuhn 1962/2012). There, as noted above, Kuhn characterized scientific revolutions as paradigm changes. Perhaps Scerri is correct to insist that the "classic" Kuhnian view is the view expressed in The Structure of Scientific Revolutions, not the view Kuhn later developed, which is the one I draw on. By "classic" I merely meant typical (Wray, 2022)

In any case I am glad that Wray and I appear to be focusing our debate a little more closely on Kuhn's later view and that Wray seems to regret his use of the term "classic" in this context. Before moving on I would just like to remark that this new qualification by Wray, to mean typical cases, raises some new problems, since I am not aware that Kuhn or any other authors have re-examined many of his earlier revolutions such that one may speak of typical cases in the later sense. As far as I am aware Kuhn speaks of the Copernican revolution and the Chemical Revolutions but no other specific examples after his lexical turn.[12]

Returning to Wray, he also writes,

> Kuhn classified the change from the Ptolemaic Theory to the Copernican Theory as a scientific revolution, and most philosophers of science would agree with Kuhn's assessment.

Here, after assuring us that he only wishes to consider Kuhn's later view, Wray appears to be returning to the more general claim concerning revolutions or the earlier Kuhnian view. Yes, it may indeed be the case that most philosophers of science would agree that this astronomical example constitutes a revolution, but especially not for the reasons that the later Kuhn claims it to be so.

Most philosophers and indeed scientists too, would consider this case to be a revolution because it involved an almost literal 'turn-around' or inversion of the previously held view. Whereas the Ptolemaic universe holds that the earth is the focal point around which everything revolves, the Copernican view involves an inversion such that everything revolves around the sun. Philosophers and scientists do not regard this case as a revolution because of the lexical changes that may have taken place and because a few astronomical bodies were no longer considered as planets as a result.

But Wray's regression to speaking of revolutions in the more general sense is rather inevitable, given that the later Kuhnian view is supposed to generalize his earlier one, and not intended to provide an altogether different sense. In the final analysis, it may not be possible to divorce the early from the later Kuhnian view of revolutions, since the later view was meant only as a refinement of the earlier one.

In his recent response, Wray also claims that my use of a Venn diagram in which I aimed to show the relationship between atomic weight and atomic number is misleading,

> Scerri's diagram for the chemical revolution has circles representing the parts of an atom—proton, neutron, and electron. This diagram masks over the revolutionary nature of the change that occurred in chemistry. Indeed, this diagram is focusing on the wrong concepts, specifically, atomic weight and atomic number. In order to understand the revolutionary nature of the change, we need to focus on the change in the extension of the term "chemical element." The extension of the term is significantly different before and after the discovery of atomic number.

While I accept that part of the alleged revolution in the sense of the later Kuhn is supposed to be concerned with the term element, I must insist that the question of the relationship between atomic weight and atomic number is crucial to the discussion. The way in which certain isotopes ceased to be regarded as elements was precisely due to their having a distinct atomic weight, while sharing the atomic number of an element that was already recorgnized as such. Moreover, I am claiming that these two concepts show a great deal of ovelap rather than standing side by side as distinct ontological categories in the manner that Wray appears to conceive of them, in his own Venn diagram that he proposes in his most recent contribution.

Wray dismissal of my Venn diagram which seeks to clarify the relationship between atomic number and atomic weight is puzzling, given that his initial article on this subjected treated two issues, (1) change from atomic weight ordering to the use of atomic number and (2) the discovery of isotopes of elements on a par. For example, the opening words of his original article were,

> The 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 (Wray 2018, 209).

In the same article he also writes,

> Perhaps most significant in this process was the discovery of atomic number.

as well as,

> Contemporaneous with this research on atomic number was another research program examining the various anomalous chemical elements that shared the same chemical properties but differed with respect to atomic weight (Wray, 2018).
>
> These two discoveries complimented each other. Once chemical elements were thought of as essentially defined by their atomic number, the notion of an isotope was no longer a conceptual impossibility.

Another problem with Wray's account is his constantly referring to the discoveries of atomic number and isotopes as bringing about a change of theory in chemistry. However, these specific anomalies did not contribute to bringing about a radical change of theory in chemistry. As I already pointed out in my earlier response, the discoveries of atomic number and of the phenomenon of isotopy did not bring about any change whatsoever to the prevailing chemical theory. The discovery of a better means of ordering the elements does not constitute a theory by any stretch of the imagination and nor does the realization that atoms of the same element may differ in their weights. Theories are generally understood as being explanatory frameworks such as quantum theory or the theory of relativity in physics, and not as specific discoveries that resolve equally particular anomalies in any particular discipline.[13]

6. Kuhn and the violation of the no-overlap principle

Kuhn's later discussions of scientific revolutions is centered around his principle of the violation of no-overlap. Kuhn wrote very little on this principle which he first introduced in an article of 1987 titled, 'What are scientific revolutions? (Kuhn, 2000). He revisited this theme in 1990 while giving a presidential address to the Philosophy of Science Association (Kuhn, 1990).

In the course of these writings Kuhn gave very few examples, and of the few that he did provide, only one was a scientific case, namely the turn from the Ptolemaic to the Copernican universe. I am not aware of whether he ever returned to elaborate fully on this 'principle'. In the course of his speech to the PSA Kuhn alludes to a book that he is in the process of writing to finally answer his critics but, as is well known, such a book has never materialized.[14]

Given the rudimentary and underdeveloped nature of this principle, I suggest that it may be a little risky for commentators like Wray to connect their claims for new revolutions quite so firmly with it.

It should also be noted that Kuhn's use of the double negative in the concept of violation of no overlap is rather confusing. Such a double negation could amount to saying that there is in fact overlap. And if this state of affairs does exist between two paradigms, or two competing scientific lexicons, there seems to be no reason whatsoever for claiming any form of incommensurability.

If the manner in which the Earth, Sun and Moon was classified did show overlap between the Ptolemaic and Copernican paradigms, there would be no lack of agreement as to whether they were planets or not.

Clearly such a reading of the violation of no-overlap is not what Kuhn had in mind. What then did Kuhn mean to say regarding which heavenly bodies were considered to be planets before and after the Copernican revolution in connection with his principle? For a more correct, although I still claim rather convoluted use of his principle, I am grateful to Vincenzo Politi for providing the following passage.

> Ptolemy's and Copernicus's cosmologies are taxonomically incommensurable, because there cannot exist a conceptual taxonomy in which the moon is both a planet (as in the Ptolemaic classification) and a satellite (as in Copernicus's): such a taxonomy would clearly violate the no-overlap principle (Politi, 2022).

In other words, if the principle was not violated, there would be overlap between the two paradigms since the Moon *would* be a member of both natural kinds. If that were so there would be no incommensurability. But of course, Kuhn wants to claim that such a lack of overlap implies incommensurability and consequently the occurrence of a scientific revolution.

Or as James Marcum writes,

> Another important property of kind terms is conceptual, regarding the relations between kind terms and referents. These relations are governed by a non-overlap principle. Kuhn notes that "no two kind terms, no two terms with the same kind label may overlap in their referents unless they are related as species to genus" (Ibid.). For example, there are no gold rings that are also silver rings, but there are red things that are also beautiful. If two kind terms do have overlapping referents in a speech community, communication failures are inevitable: people simply do nt know how to name those referents in the overlapping region (Marcum, 2018).

In the case of atomic particles, the objects in question can be characterized by their masses, with each object having a unique mass. In former times such massive particles were all classified as elements. However, since Moseley's work they can be classified as isotopes of a particular element, but some such particles can be classified as both. An isotope of iodine, to return to my earlier example, is an example of a unique isotope but also a case of an atomic particle of a unique element.

Such isotopes provide examples of where there is overlap and therefore no violation of Kuhn's no-overlap principle. The paradigm which dealt only with elements and the later paradigm which deals in isotopes as well as elements, are not taxonomically incommensurable in the case of monoisotopic elements. Kuhn would therefore have to conclude that there is no scientific revolution involved in the change of taxonomy that was brought about by the discovery of atomic number and of isotopes. There are approximately 15 elements that only have one isotope in the same way that iodine has.

Isotope and element are not at the same taxonomic level, in the same way that cat and dog, two of Kuhn's favorite examples are. An isotope is a subclass of the concept of element in the majority of cases, namely all the elements that are not monoisotopic. It would appear that Wray is not aware of these points, otherwise he would not be suggesting that the astronomical case is analogous to the atomic case. The discovery of isotopes does not represent a revolution in the sense of the later Kuhn in the same way that the change from the Ptolemaic to the Copernican model may do.

If one places more attention on the reference of the terms planet ,or isotope, I believe that the alleged incommensurabiity dissolves. The fact that the moon circles the earth leads to the moon being classified as a satellite in the Copernican model rather than as a planet. But this change only concerns how this astronomical body is being classified. The referent is still that unique astronomical body which waxes and wanes in the course of each month and that we are all familiar with.

Of course, the situation is a little more complicated than I have just implied since the manner in which natural kinds are identified appears to have undergone an almost cyclic change in the history of philosophy (McCulloch, 1989). Very briefly, according to Frege, natural kinds were identified by means of sense or through a description of their attributes. In the 1970's Kripke and Putnam famously posited their causal theory of reference in which natural kinds were to be picked out according to their intrinsic properties such as the fact that the element gold was and substance whose atoms have atomic number of 79. It is significant that the causal theory of reference was also used to counter Kuhn's talk of incommensurability and to restore the common-sense view that descriptions may change as science develops but the entities in question do not.

More recently the Kripke-Putnam view has been subjected to a good deal of criticism since it seems to completely exclude any form of interest dependence on the part pf scientists. In response, Richard Boyd has introduced his homeostatic property cluster theory (HPC). Boyd postulates the existence of a homeostatic mechanism capable of explaining why those properties are statistically associated with each other and shared by the members of a given kind (Boyd, 1991). But none of these recent developments in the study of natural kinds represent a rejection of the attention that contemporary philosophy of science places upon matters of scientific ontology.

7. The wider question

One can only hope that Thomas Kuhn might have approved of Wray's desire to find further examples of scientific revolutions in the later sense of Kuhn. Moreover, Wray's defence of Kuhn in this way appears to be a form of 'normal Kuhnian philosophy of science', to coin an analogous term to Kuhn's talk of normal science, within which scientists do not challenge the prevailing paradigm. Brad Wray, who has carried out much work on the views of Kuhn, appears to be working only within the limitations of Kuhn's views, albeit the later and supposedly more refined view. Wray does not seem to want to pose the question of whether the discovery of atomic number and of isotopes constitute a revolution in the way that other philosophers of science might view the concept.

I propose to now take an alternative view of the situation, and one that I believe many of Kuhn's critics might also share. It is well known that Kuhn's original position received a great deal of criticism from historians as well as philosophers of science especially on the question of incommensurability (Shapere, 1964; Hacking, 1981; Scheffler, 1967; Putnam, 1981; Davidson, 2001; Kitcher, 1978).

As a more recent critic writes,

> He [Kuhn] argued that these criticisms depended on the "literally correct but regularly overinterpreted assumption that, if two theories are incommensurable, they must be stated in mutually untranslatable languages." Now, if the two theories could not be stated in a single language, they could not be compared. Furthermore, these critics claimed that if Kuhn were right, then archaic scientific theories could not be translated into modern language. But in *Structure* and elsewhere, that is exactly what Kuhn did: he both compared supposedly incommensurable theories with one another and he translated them into modern language. In both cases, his practice would seem to be inconsistent with his conception of incommensurability. (Garber, 2012, 505)

and in another article,

> Kuhn's extended attempt to answer the philosophers has always struck me as one of the great tragedies in the history and philosophy of science. It didn't have to be this way. There is much that was right in Kuhn's idea of the incommensurability of paradigms at the very beginning, in *Structure*. The history of his later struggles with incommensurability is a sad story of a great thinker who allowed himself to be led down a dead end (Garber, 2012, 506)

Indeed, Kuhn spent the remainder of his working life in attempting to explain what he had really meant, as well as in modifying what he had originally stated.

Here is how Kuhn expressed himself of this process,

> My own encounter with incommensurability was the first step on the road to *Structure*, and the notion still seems to me the central innovation introduced by the book. Even before *Structure* appeared, however, I knew that my attempts to describe its central conception were extremely crude. Efforts to understand and refine it have been my primary and increasingly obsessive concern for thirty years (Kuhn, 2000, 228).

One of the main qualifications, if not an outright departure from Kuhn's original position, was his turn to an analysis of scientific lexicon and the nature of language more generally. Would it be so preposterous to suggest that Kuhn's program began to degenerate from the moment when he started to alter his original bold and startling claims, which so caught the professional and public attention when they were first published?

As several authors have written, the main reason why Kuhn was mistaken in devoting so much attention to the language of science was that it diverted attention from ontological aspects to terminological ones. Said differently, Kuhn appears to be taking sides with those philosophers who place greater importance on sense rather than on reference in the long-standing philosophical debate that dates back to Frege and even earlier (McCulloch, 1989).

Kuhn's move from a concern from matters of scientific ontology to an emphasis on sense has of course been eloquently criticized by Alex Bird,

> Whereas The Structure of Scientific Revolutions is naturalistic in approach, drawing upon empirical, scientific discoveries where appropriate, his later work is much more philosophical in style and a priori in method. For example, in The Structure of Scientific Revolutions Kuhn's explanation of the relationship between observation, theory and reality was informed by gestalt psychology and by the results of research carried out by his Harvard colleagues, the experimental psychologists Bruner and Postman. Later, by contrast, Kuhn supported his view with quasi-Wittgensteinian considerations from the philosophy of language, while he characterized that view in terms of Kantianism (Bird, 2002).

Bird continues by claiming that Kuhn's earlier views would have benefited from a continued naturalistic development and suggests that his later, philosophical approach was not only a failure, but what Bird calls a "wrong turning" which contributes to a lack of significance in contemporary mainstream philosophy and even philosophy of science. Some of this wrong turning is also attributed to Kuhn's lack of philosophical training.

Whereas the early Kuhn drew many examples from the history of science, he abandoned his use of empirical science for a more a priori approach that was initially motivated by the writings of Quine. While the early Kuhn focused on the development of science while drawing from many historical episodes, his later output turned almost exclusively to the nature of the language that is used in science. Moreover, Kuhn later denied that an evolutionary epistemology need be a form of naturalised epistemology, and even regretted an overemphasis on the empirical aspect of his earlier writings.

Kuhn's attempt to cast incommensurability within the philosophy of language had begun in the 1960s, when he drew inspiration from Quine's indeterminacy of translation thesis (Quine, 1960). Furthermore, Kuhn seems to have also drawn from Quine the notion that what differs between incommensurable languages is the way they divide the world into kinds of thing, or in other words the notion of natural kinds.

But a result of Kripke and Putnam's work in the 1970s, philosophers have tended to downplay the fact that natural kind terms are picked out by their sense but have focused on a reference that is fixed by a causal connection between the use of the term and the reference itself. The claim is that water refers to that familiar transparent liquid because it has a causal connection to the substance that was baptized as water in the remote past and not because of any description of the liquid. In this respect the later Kuhn is very much out of step with contemporary philosophical thinking. Stated otherwise, wheras Kuhn's earlier work was very much focused on actual scientific matters or one might say ontological aspects, his later work is seen by many to consist of a retreat to an analysis of language, a shift from reference to the world itself to an analysis of how one describes and categorizes the world. It is for these further reasons that I too believe that Kuhn's reformulation of scientific revolutions may have been misguided.

Meanwhile Brad Wray is attempting to have things both ways, since he does plunge into a considerable amount of scientific detail concerning atomic weight, atomic number and isotopy while using Kuhn's later approach to the analysis of scientific change with its emphasis on the language of science and all that this entails.[15] I have to conclude that Kuhn might not after all have approved of Wray's attempt to support his later writings.

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Notes

[1] I am referring to the original account by Thomas Kuhn as stated in his classic book, *The Structure of Scientific Revolutions.*

[2] At later stages in the history of the periodic table two further atomic weight anomalies of this kind also emerged. One of them followed the discovery of the noble gas argon, which has an atomic weight that is lower than the element potassium although their ordering is such that argon is placed before potassium. The fourth atomic weight anomaly concerns thorium and protactinium, the latter of which was only discovered in 1917. Although protactinium is a whole atomic unit lighter than that of thorium, its place in the periodic table follows that of thorium. This fourth example represents the largest atomic weight anomaly of the four known cases. Neither of these further examples were known to exist at the time of the discovery of the periodic system.

[3] One of the reviewers of this article reminds me that Wray's suggested revolution concerning the discovery of atmomic number and isotopes is not especially original, since it had previously been discussed by Jensen in 1998.

[4] This statement is somewhat ahistorical in that neutrons were not identified until the year 1930.

[5] Kuhn makes precisely this point about increasing specialization as science develops in his later writing (Kuhn, 1990).

[6] At the same time, I do not wish to neglect the relevance of lexicon and linguistic aspects in general in the development of scientific ideas. I am only suggesting that Kuhn may be placing too much emphasis on these factors.

[7] Others have even suggested that this was Kuhn's biggest mistake (Garber, 2016).

[8] My view is supported in a recent article by Pieter Thyssen who emphasizes that Paneth emphasized the continuity with the older definition of elements while providing a new definition in terms of atomic number which was adopted by IUPAC (Thyssen, in press).

[9] The philosophy of chemistry as an academic discipline came into being in the mid 1990s and has continued to develop since them. For example, the International Society for the Philosophy of Chemistry has held an international meeting during each of the previous 26 years while the official journal for this society, *Foundations of Chemistry*, began publication in 1999.

[10] Perhaps Wray is not aware of the existence of many mono-isotopic elements which include, beryllium, fluorine, sodium, aluminum, phosphorus, scandium, manganese, cobalt, arsenic, yttrium, niobium, rhodium, iodine, caesium, praseodymium, terbium, holmium, thulium and gold.

[11] Indeed, elements are perhaps the epitome of natural kinds and have served as the prime example of such in innumerable philosophical articles on the subject (Kendig, 2016; Scerri, 2020).

[12] Indeed, it would be rather useful is somebody were to undertake the task of re-examining Kuhns earlier revolutions to see whether they stand up in the light of his new criteria having to do with lexical changes, diversification of disciplines and the no-overlap principle.

[13] Needless to say, I do not deny the epistemic significance of the discovery of isotopes in the development of our knowledge of the structure of atoms and its relevance to understanding the periodic table in a more profound manner than was previously available. I thank a reviewer for suggesting this qualification.

[14] It appears that Kuhn gave the text of the book to James Conant so that he might complete it, something that has not yet occurred.

[14] As a reviewer of the present article also points out, Wray is incorrect in claiming that Cannizzaro proposed using the atomic weights of the elements as a means of classifying them.

The use of Cannizzaro's atomic weights to classify the elements was rather carried out by at least six discoverers of the periodic system, of whom Mendeleev is the best known.

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