A PHILOSOPHICAL COMMENTARY ON GIUNTA'S CRITIQUE OF NEWLANDS' CLASSIFICATION OF THE ELEMENTS

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Introduction and Motivation

In recent years there has been a resurgence in interest in philosophical aspects of chemistry, dating from the early 1990s (1-3). It is gratifying to see that this development has not been confined to analytical philosophy but has frequently spilled over into issues concerning the history of chemistry. Giunta's recent article (4) is therefore of great interest because it represents an example of work that approaches philosophy of chemistry from the historical direction. Giunta's article on Newland's periodic system, or whether indeed it can be called a system, is a bold attempt by a chemist-historian who is willing to venture a philosophical analysis based on an episode in the history of chemistry.

There has been a good deal of discussion in the literature over whether chemical periodicity should be referred to as the periodic table, periodic system, or periodic law. In addition there have been articles in philosophy of chemistry which have attempted to clarify the terms theory, model, or law in the context of chemistry as distinct from physics (5, 6). Giunta's article, which provides an analysis of these terms, should therefore be of interest to philosophers as well as historians of chemistry.

While accepting that a chemical audience may not wish to agonize over the use of terms like system and self-consistency, I believe that the clarification of terms is one area in which a philosophical analysis of chemical concepts has a powerful role to play. In addition, it cannot be denied that Giunta's main intention is to examine whether Newlands produced a "system" or not. I therefore make no apologies for undertaking an analysis of precisely what Giunta means by the word "system" in this context. After doing so I turn to Newland's work and conclude that he deserves more credit than he has been accorded by Giunta.

Commentary

I believe that Giunta makes some important points about Newland's overall role in the discovery of the periodic system but also that he introduces some misconceptions with which I shall express some friendly disagreement. Although Giunta states, at the outset, that he intends to examine the work of Newlands "from a contemporary point of view," I think there may be some problems with this proposal as I hope to show. Giunta also says that he is not concerned with reconstruction of the process of Newlands' discoveries but only with appraising the validity of his writings. He appears to want to concentrate on the logic of discovery, rather than the context of discovery, to use a distinction that was once popular in philosophy of science. Such a goal is of course laudable, especially given the excessive emphasis on context, and in particular the social context, of scientific discoveries that one finds in recent science scholarship.

System, Organization, and Self-consistency

Although chemists might have an intuitive feel for terms like system, organization, and internal consistency, I ask for their indulgence in pausing to analyze these notions. I feel justified in doing so since Giunta has made them major criteria in his critique of Newlands.

Giunta begins his critical analysis by stating that whereas Mendeleev and Newlands referred to a "periodic law" he, Giunta, intends to follow the author van Spronsen in preferring to use the term "periodic system (7)." However, Giunta believes that van Spronsen's definition of periodic system is inadequate and in need of strengthening. He tells the reader that van Spronsen defines a periodic system as (4):

> …a system of all the (known) elements arranged according to increasing atomic weight in which the elements with analogous properties are arranged in the same group or column.

but that earlier in his work van Spronsen refers to (7):

> "facets of a true periodic system" including additional criteria, for example a distinction between main groups and sub-groups, and provision of vacant spaces for undiscovered elements.

Giunta proposes to define a periodic system as something lying between these two versions, given by van Spronsen, namely (7):

> a periodic system of the elements consists of a self-consistent arrangement by atomic weight of all the known elements, which systematically displays groups of analogous elements.

Giunta claims that his own definition places considerable emphasis on "organization" and "internal consistency" although he fails to provide any additional criteria to indicate just what these features might mean in this context. Finally, he asserts that he does not require his own sense of system to be one "free from error."

Giunta's attempt to improve on van Spronsen's definition(s) of the term periodic system is, I believe, somewhat problematic (8). Whereas Giunta implies that his own definition is stronger than van Spronsen's first definition, it is, in fact, weaker. By failing to include the word "increasing," as a qualifier for atomic weight, Giunta unwittingly admits even earlier systems such as that of Gmelin. In 1843, a remarkable 26 years prior to the first of Mendeleev's published systems, this chemist classified all the then known elements and obtained a very successful grouping of analogous elements (9). Gmelin's only failing was that he did not strictly adhere to increasing atomic weights, something which Giunta does not explicitly specify as an important criterion, although this omission may well have been accidental.

On the other hand, the claim by Giunta that his own definition of "system" is weaker than van Spronsen's second definition also appears to be mistaken because van Spronsen does not require a system to be free from error. Had van Spronsen done so, he would have excluded many of the precursors of the modern periodic system, which he has so painstakingly documented in his book while considering them as genuine systems.

I turn to considering what Giunta claims to have added to van Spronsen's definitions. Giunta requires that the qualities of being "self-consistent" and "systematic" should be present in a system displaying all the known elements. However, we are not told what self-consistency actually implies in the context of a classification of the elements.

As for the second requirement, I believe this may be circular. Since the definition given by Giunta was intended to define "system," it can hardly be illuminated by the statement that a system shall be systematic! The further stipulation that his own definition "places considerable emphasis on organization and internal consistency" does not appear to clarify his position since nothing in the definition actually states how this claim is to be realized.

Is the Periodic System a Theory?

Giunta then introduces a further requirement, namely that a periodic system should also fulfill the criteria given by one George Lachman of what constitutes a theory. Whereas Giunta promises, in his title, to analyze why the work of Newlands does not represent the discovery of a system, I believe he proceeds to cloud the issue by invoking a further set of criteria which are not intended for "systems" but for scientific theories. Whereas up to this point the discussion had focused on whether Newlands had produced a system, Giunta then appears to suggest that "theory" and "system" are synonymous terms. I would like to explain why I believe these terms to be far from synonymous, especially in the context of chemical periodicity.

The term system is very frequently used to describe chemical periodicity, but to the best of my knowledge chemical periodicity has never been regarded as a theory. In fact, according to some authors, chemistry does not possess any genuine theories of its own (10). This incidentally is a reason sometimes given for the lack of interest in chemistry shown by philosophers of science. Chemical periodicity is instead referred to almost exclusively as a system because it is essentially a classification system and, as such, does not depend upon any theoretical underpinning for its success or otherwise. This is why I believe that Giunta may be mistaken in invoking Lachman's criteria for theories in a context where one is simply not dealing with any theory.

Indeed Giunta then introduces into the reckoning some even further criteria, due to Thomas Kuhn, without discussing whether they are consistent with the views of Lachman as well as his own previously stated definition of "system." Having thus set up at least three types of criteria, and without, I would claim, *any* supporting arguments involving examples from Newlands work, Giunta is prepared to declare (4):

> Although Newlands' work does not meet the criteria for a periodic system set out above, his contributions were substantial.

Perhaps the most charitable interpretation for this conclusion would be that Giunta intends to show later in the article how Newlands fails to meet the criteria for a system, but this turns out not to be the case. Instead of giving any form of analysis of why Newlands fails to meet his own criteria or those of Lachman and Kuhn, Giunta begins pursuing what he himself states as being of secondary importance, namely the fact that Newlands' "contributions were substantial." I will return to this analysis which takes up the next two pages of Giunta's article, in due course; but first let us turn to the main purpose of the article, namely whether Newlands did or did not produce a "system."

Predictions

In returning to the promised main theme, Giunta begins by stating that it is not so much that Mendeleev produced a better system than Newlands, but rather that Newlands failed to produce anything that might warrant the label of a "system." First of all Newlands' rather remarkable prediction of the existence of an unknown element, which subsequently became known as germanium, is dismissed by Giunta. This is done on the grounds that the prediction was made before Newlands had formulated his law of octaves and that it was carried out with atomic weights instead of ordinal numbers. In doing so Giunta seems to overlook the fact that Mendeleev's spectacular predictions of germanium, gallium, and scandium were also based on atomic weights. Contrary to Giunta's reading, and regardless of whether or not Newlands called his earlier classification a law of octaves or not, it cannot be denied that he did in fact predict germanium a number of years before Mendeleev, as many historians concur.

In any case it is difficult to see why Giunta is placing so much importance on predictions when he had promised earlier to concentrate on the criteria of "self-consistency" and "organization" in order to assess the worth of a periodic system. Of course, it may well be that Newlands' law of octaves is inconsistent with his prior prediction of the element germanium, but this is a quite separate issue from whether the system itself is self-consistent. As I see it, self-consistency, in any form of system, such as a mathematical system, for example, does not necessarily imply predictive power.

Giunta then proceeds to criticize Newlands on the grounds of failing to accommodate newly discovered elements into his classification. Once again this is a separate criterion that is covered neither by self-consistency nor organization of any system since the latter criteria are not necessarily connected to the possible discovery of new elements. The failure of Newlands' classification to allow for accommodation of new elements is an important drawback but one which I believe is mischarachterized by Giunta's analysis which supposedly hinges on "self-consistency and organization."

Giunta then claims that Newlands' "attempts of systematization" made in 1878 and 1884 came too late. What features make these attempts more systematic, in Giunta's view, is not something that he discusses, except to say that Newlands was now "providing a check-list of specific instances in which he was applying the law." I suggest that Giunta has once again shifted ground in that now an attempt to apply the law of octaves is taken to represent another criterion for deciding whether or not Newlands' classification represents a "system" (11).

Giunta then moves on to praise Mendeleev's superiority over Newlands for making an (4):

> ...extensive list of deductions which accompanied his predictions from the start.

This is unfortunately not quite the case. Admittedly, the three famous predictions of Mendeleev are hinted at in his original paper of 1869, by the fact that he leaves empty spaces for these elements. But it was not until two years later that Mendeleev was prepared to make detailed predictions on the properties of these elements and their compounds in his system of 1871 (12, 13). One might argue that two years is not a long time between Mendeleev's vague predictions of 1869 and his detailed version of 1871, but Giunta's remark suggesting that Newlands is the only person whose views on classification of the elements evolved over time seems to be excessive.

There is no denying Giunta's statement that some of Newlands' published ideas showed a deterioration as time progressed. But there are some not too well known aspects of Mendeleev's work which, if examined in isolation, would also lead the reader to realize that the much lauded Russian chemist also falls short of the adulation he is usually accorded. One example is the case, seldom discussed in the literature, concerning the element gallium. In his paper of 1871 Mendeleev predicted that eka-aluminum, subsequently known as gallium, would "in all respects" have properties intermediate between those of the elements above and below it, namely aluminum and indium. However, the melting point of gallium (30ºC) is nowhere close to being intermediate between those of aluminum (660ºC) and indium (155ºC). In 1879 Mendeleev gave the following *ad hoc* rationalization of the anomalously low melting point for gallium (14):

> ...we should pay heed to the fact that the melting point of gallium is so low that it melts at the temperature of the hand. It might appear that this property is unexpected; but this is not so. It suffices to look at the following series -

Mg	Al	Si	P	S	Cl
Zn	Ga	...	As	Se	Br
Cd	In	Sn	Sb	Te	I

> It is evident that in the group Mg, Zn, Cd, the most refractory metal has the lowest atomic weight; but in the groups beginning with S and Cl, the most difficultly fusible simple bodies are, on the contrary, the heaviest. In a transitory group such as Al, Ga, In, we must expect an intermediate phenomenon; the heaviest (In) and the lightest (Al), should be less fusible than the middle one, which is as it is in reality. I turn attention to the fact that properties such as the melting point of bodies depend chiefly upon molecular weight, and not on atomic weight. If we were to have a variety of solid sulphur not in the form of S_6 (or, perhaps, of still heavier molecules S_n), but in the form S_2, which it assumes at 800ºC, then its temperature of melting and of boiling would undoubtedly be much lower. In just the same way, ozone, O_3, condenses and solidifies much more readily than does ordinary oxygen, O_2.

Not only had such an argument never been give before by Mendeleev, as a means of predicting trends in properties, but it also runs contrary to the spirit of his method of simple interpolation which he used so successfully in many other instances. The completely *ad hoc* nature of the argument is compounded by the fact that it is by no means clear that this truly represents "an intermediate phenomenon" to those in the other groups mentioned and indeed why this somewhat contrived trend should begin at this particular place in the periodic table. In spite of his use of the word "must" there is nothing in the least bit compelling about Mendeleev's argument.

Nobody would consider denying Mendeleev his triumphs because of such indiscretions. Furthermore these *ad hoc* moves by Mendeleev would seem to be more serious than Newlands' desperate bid to assert his claim to priority in the case of germanium by referring to all his articles rather than, as Giunta would seem to wish, just those published following the announcement of his law of octaves.

Back to Newlands

Contrary to the message in Giunta's title, I believe that Newlands did indeed produce a good periodic system and more importantly perhaps, that he was the first to emphasize the importance of the periodic law, or the law of octaves as he termed it. Indeed, as Giunta points out and documents, the often heard dismissals of Newlands on the grounds that he mistook the repeat distance to be eight elements instead of nine, in the short periods, is something that Newlands himself fully anticipated.

Newlands' contribution lies in having been the first to recognize that the crucial feature lay in the approximate repetition, or periodicity, of the elements and that this behavior is law-like. Whether this repetition occurs after seven, eight, or even nine elements is beside the point. I believe that Giunta's arguments for criticizing Newlands' system because of what he regards as inconsistencies have missed this important aspect. But I agree with Giunta's drawing attention to the fact that some of Newlands' later systems did not leave any gaps for undiscovered elements and thus negated his periodicity of eight.

This mention of leaving gaps raises the vexing question of just how important predictions are in science, something that Giunta does not discuss in spite of the extensive literature on the subject and the fact that this debate is still being actively pursued and precisely in the context of the periodic system (15-20). Whether or not prediction is an especially important aspect of scientific developments is open to question, as the current debate continues to show. On the other hand, the discovery of laws as a very important scientific activity is accepted with less controversy. To return to a theme I alluded to earlier, Newlands deserves perhaps more credit than Giunta is giving him, precisely because he was the first to recognize a law-like behavior in the way that elements seem to recur after certain intervals.

Although Giunta recognizes that law-likeness is important, he seems to be prepared to ignore this aspect in the course of pronouncing judgment on Newlands' scientific contributions. Instead, as the title of the article in question indicates, Newlands is being criticized for failing to discover a "system," according to Giunta's rather idiosyncratic criteria for what constitutes a system.

I suggest that in attributing merit it is not the ability to capture the small details that should be valued most, but rather to grasp the existence of a general law. If this is accepted then, contrary to Giunta's position, Newlands should be lauded rather than faulted. Admittedly, Newlands was mistaken in not realizing that this repeat distance was variable. But in terms of announcing the existence of a law of regularity, which would have very important ramifications, he was the first to do so.

Atomic Number

Finally, I turn to Giunta's critique of Newlands over the question of atomic number since I believe that the arguments proposed are to some extent misplaced and rather Whiggish. Giunta contradicts Wendell Taylor's statement (21), that Newlands might have been "a pioneer in atomic numbers" because as Giunta puts it (4):

> For several reasons that number is not the same as the atomic number known today.

The first such reason for Giunta is that the discovery of elements unknown to Newlands would cause some of Newlands' higher atomic numbers to be too low. Although this is indeed the case, I believe it to be a trivial objection to the general principle of using an ordinal number to order the elements rather than their individual atomic weights. Clearly, Newlands could not have known the correct atomic numbers of all the elements at the time at which he was writing.

Giunta's second reason, the fact that Newlands assigned the same ordinal number to some elements, is a more serious problem although it only occurs six times in as many as 56 entries in Newlands' table of 1866.

The final reason given by Giunta for rejecting the notion that Newlands foresaw atomic number is also disputable (4):

> Finally, Newlands was not aware of the physical basis for atomic number first elucidated by Moseley more than half a century later.

If the issue is whether Newlands in some sense anticipated the notion of atomic number, then he could only have done so in the absence of the knowledge of its physical basis. One cannot help wondering whether Giunta might also want to diminish Mendeleev's discovery of chemical periodicity itself because he was not aware of its "physical basis" until this was provided by Niels Bohr, in the form of electronic configurations of atoms, also about half a century later.

Conclusion

Giunta is to be applauded for trying to bridge the unfortunate gap between the study of historical and philosophical aspects of chemistry. He has begun to analyze the term "system" in the work of John Newlands, while drawing on the historical record. I hope the comments raised here will stimulate a deeper analysis of the issues involved.

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