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Some comments on the views of Niaz, Rodriguez and Brito on Mendeleev's periodic system

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ABSTRACT
Many articles have been written about the value of incorporating an understanding of history and philosophy of science into science education and this has included the teaching of chemistry. Given the immense role that the periodic table plays in chemistry it is important to be clear about a historical and philosophical perspective on the periodic table and its possible ramifications for the way in which chemistry is presented. The article presents a critique of a paper by Niaz, Rodriguez and Brito who have addressed the relevance of historical and philosophical aspects of the periodic table in an article in which they have claimed that Mendeleev's periodic table should be regarded as a theory. In addition they have claimed that Mendeleev was a supporter of the atomic theory and have addressed some general philosophical questions concerning inductivism and the role of prediction and accommodation in the acceptance of scientific discoveries.

Introduction

The purpose of the present article is to consider several notions that have been argued by Niaz, Rodriguez and Brito concerning Mendeleev's periodic table (Niaz, Rodriguez, Brito, 2004). Among the several claims made by these authors the main one seems to be that contrary to most experts in the field, Mendeleev's periodic table was a theory rather than a law, or a classification. In addition there are many claims made regarding what the authors believe to be a naïve inductivist stance taken by historians of science. Instead the authors propose that taking a Lakatosian view of the development of science adds further support to their claim whereby Mendeleev's periodic table is a theory rather than a law. In arriving at this conclusion the authors canvass what they take to be support from a wide variety of sources including some physicists, chemists and some contemporary philosophers of science including Cartwright and Giere. It is my contention that the majority of such samplings may have been taken out of context and that betray some confusion over the central issues under discussion.


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An appraisal of Mendeleev's contribution to the development of the periodic table

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Abstract

Historians and philosophers of science generally conceptualize scientific progress to be dichotomous, viz., experimental observations lead to scientific laws, which later facilitate the elaboration of explanatory theories. There is considerable controversy in the literature with respect to Mendeleev's contribution to the origin, nature, and development of the periodic table. The objectives of this study are to explore and reconstruct: a) periodicity in the periodic table as a function of atomic theory; b) role of predictions in scientific theories and its implications for the periodic table; and c) Mendeleev's contribution: theory or an empirical law? The reconstruction shows that despite Mendeleev's own ambivalence, periodicity of properties of chemical elements in the periodic table can be attributed to the atomic theory. It is argued that based on the Lakatosian framework, predictions (novel facts) play an important role in the development of scientific theories. In this context, Mendeleev's predictions played a crucial role in the development of the periodic table. Finally, it is concluded that Mendeleev's contribution can be considered as an "interpretative" theory which became "explanatory" after the periodic table was based on atomic numbers.
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1. Introduction

From a history and philosophy of science perspective the periodic table of elements has generally been considered as a classification, system, table, law, and very

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Periodicity in the periodic table and atomic theory

The authors devote considerable space to a discussion of Mendeleev's writings on atomic theory and argue that his views were ambivalent. They claim that most historians take a naïve inductivist approach to the development of the periodic table and that they consider that Mendeleev proceeded on the basis of empirical observations rather than the atomic theory. However, there is no reference to, or mention of, the work of Popper who was the most significant critic of inductivism and whose work Lakatos freely acknowledged to be the starting point to his own contributions (Popper, 1959; Larvor, 1998). In addition, Niaz et al. may be misconstruing the notion of naïve inductivism while wanting to convince their readers of the virtue of a Lakatosian view, with all the zeal of new converts. Niaz et al. do not seem to appreciate that Popper, and Lakatos for that matter, have exerted a considerable influence on historians as well as philosophers of science and they often fail to quote any historian or philosopher when accusing them in general of still operating within an inductivist framework.

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For example, the thrust of the argument in section 2 of the paper under discussion consists of Niaz et al. claiming that historians of science believe that Mendeleev arrived at his periodic system merely on the basis of observations. Moreover the authors wish to claim that Mendeleev drew on atomic theory rather than mere observational data and that this essentially shows that we need a post-inductivist approach to understand the issues. Inductivism is therefore reduced to the view that scientific developments are based on observations rather than on the use of theories. Be that as it may, I would like to examine some of the detailed claims that are made.

The authors cite historian of chemistry, van Spronsen, in saying that the catalyst for the development of the periodic system was the Karlsruhe meeting of 1860, which clarified the distinction between atom and molecules and defined such concepts as valence. Niaz et al. immediately raise the following question,

> In spite of this fairly categorical statement with respect to the role played by the atomic theory by a major historian of the periodic table, many historians attribute its success primarily to empirically observed properties of the elements (inductive generalization). (Niaz et al., 2004, p. 273)

In fact almost every source on the periodic table seems to agree on the "catalytic power" of the Karlsruhe meeting in the discovery of periodicity not just in the case of Mendeleev but also Odling and Lothar Meyer. But whether or not this conference did indeed have any such catalytic effect does not immediately imply that van Spronsen, or anybody else, is claiming that atomic theory *per se* might have been essential to the discovery of chemical periodicity. One could distinguish between the terms atom and molecule for example, regardless of whether one believed in the literal existence of physical atoms. There is a considerable literature on the question of chemical atomism, which treats 'the atom' as the smallest amount of matter, which could enter into chemical combination (chemical atomism), as opposed to the belief that atoms were microscopic physical entities with a 'real' existence (physical atomism) (Fleck, 1963). Niaz and his co-authors are either unaware of this literature or have mysteriously chosen to ignore it.

Instead Niaz et al. pose the following rhetorical question,

> So how could Mendeleev conceptualize periodicity as a function of the atomic theory? An answer to this question will precisely show Mendeleev's ingenuity, farsightedness, creativity, and the ability to "speculate". (Niaz et al., 2004, p. 273)

It would appear that Niaz et al. believe that if they can show that Mendeleev indeed possessed the ability to "speculate" then they can oppose the vast majority of historians of science who apparently wrongly hold that Mendeleev was not a speculator but merely followed the observational evidence like a good naïve inductivist. Now one of the major problems that Niaz et al. face in this task is that there is ample evidence that Mendeleev largely rejected the atomic theory of his day or, as the authors correctly report, was ambivalent about the role of atomic theory in his writings.[1]

The authors proceed to enumerate a series of what they term "steps" that are presumably intended to provide evidence for Mendeleev's surreptitious use the atomic theory. The first of these steps is,

> Step 1: Even in his first publication Mendeleev referred to the relationship, albeit implicitly, between periodicity, atomic weights and valence: "The arrangement according to atomic weight corresponds to the valence of the element and to a certain extent the difference in chemical behavior, for example Li, Be, B, C, N, O, F". (Mendeleev, 1869, p. 405, original emphasis). (Niaz et al., 2004, p. 273).

One can only presume that the authors are drawing attention to Mendeleev's use of the term "atomic weight" as evidence of his support for the atomic theory. Similarly, the second step is announced without any comment on how it is supposed to be supporting the main thesis that, unbeknownst to himself, Mendeleev was in fact a physical atomist,

> Step 2: After the discovery of gallium and scandium, Mendeleev expressed the relationship between atomic weight* and atomic theory much more explicitly: 'It is by studying them [atomic and molecular weights], more than by any other means, that we can conceive the idea of an atom and of a molecule. By this fact alone we are enabled to perceive the great influence that studies carried on in this direction can exercise on the progress of chemistry… The expression atomic weight implies, it is true, the hypothesis of the atomic structure of bodies' (Mendeleev, 1879, p. 243, emphasis added. The asterisk leads the reader to the following footnote: 'By replacing the expression of atomic weight by that of elementary weight, I think we should, in the case of elements, avoid the conception of atoms'). (Niaz et al., 2004, p. 273-274)

This surely is a clear indication that to the extent that Mendeleev mentioned the term "atom" he was doing so in the spirit of chemical atomism and not physical atomism and again raises the distinction that the authors seem reluctant to examine.

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[1] See Scerri (2006), for a number of quotations from Mendeleev that argue against his being a supporter of the atomic theory.

Step 3 offers more textual evidence of Mendeleev's ambivalence concerning atomic theory,

> Step 3: Another example of Mendeleev's ambivalence can be observed from the following: 'I shall not form any hypotheses, either here or further on, to explain the nature of the periodic law; for, first of all, the law itself is too simple*' (Mendeleev, 1879, p. 292. The asterisk leads the reader to the following footnote: 'However, I do not ignore that to completely understand a subject we should possess, independently of observations (and experiences) and of laws (as well as systems), the meanings of both one and the other'). (Niaz et al., 2004, p. 274)

Finally in their step 4, the authors begin to offer what they regard as positive evidence for what they believe is Mendeleev's support for atomic theory,

> Step 4: Although Mendeleev stated in 1879 that he would not formulate an hypothesis, ten years later in his famous Faraday lecture, Mendeleev (1889) not only attributed the success of the periodic law to Cannizaro's ideas on the atomic theory (pp. 636–637), but went on to explicitly formulate the following hypothesis: 'the veil which conceals the true conception of mass, it nevertheless indicated that the explanation of that conception must be searched for in the masses of atoms; the more so, as all masses are nothing but aggregations, or additions, of chemical atoms' (Mendeleev, 1889, p. 640, emphasis added). (Niaz et al., 2004, p. 274)

Needless to say, this passage does not provide very compelling ammunition for Niaz et al. for at least a couple of reasons. Firstly it is a statement made by Mendeleev a full 20 years after the discovery of chemical periodicity. Secondly it is a statement made to a general audience at an award lecture by a scientist looking back at his achievements. Such statements are notoriously prone to grandiose generalizations which may, or may not have, in fact contributed to the discovery in question. It can still be argued that Mendeleev took "the masses of atoms" to be elemental masses and that he was not even now admitting to the influence of atomic theory as such.

Finally we have step 5, which is reproduced here in its entirety in order to avoid any possible distortion to what Niaz et. al. might be arguing for.

> Step 5: Again, at the Faraday lecture, Mendeleev (1889) took extreme care to explain the periodicity of properties of chemical elements on the basis of atomic theory. We cite at length:
> The periodic law has shown that our chemical individuals [atoms] display a harmonic periodicity of properties, dependent on their masses… An example will better illustrate this view. The atomic weights–
>
> Sb = 120 Ag = 108 Cd = 112 In = 113 Sn = 118 Te = 125 I = 127
>
> steadily increase, and their increase is accompanied by a modification of many properties which constitutes the essence of the periodic law. Thus, for example, the densities of the above elements decrease steadily, being respectively–
>
> 10.5 8.6 7.4 7.2 6.7 6.4 4.9
>
> while their oxides contain an increasing quantity of oxygen:–
>
> Ag_2O Cd_2O_2 In_2O_3 Sn_2O_4 Sb_2O_5 Te_2O_6 I_2O_7
>
> But to connect by a curve the summits of the ordinates expressing any of these properties would involve the rejection of Dalton's law of multiple proportions. Not only are there no intermediate elements between silver, which gives AgCl, and cadmium which gives $CdCl_2$, but, according to the very essence of the periodic law there can be none; in fact a uniform curve would be inapplicable in such a case, as it would lead us to expect elements possessed of special properties at any point of the curve. (Mendeleev, 1889, pp. 640–641)
>
> This is a clear acknowledgment of the role played by the atomic theory to explain periodicity in the periodic table (Niaz et al., 2004, p. 274-275)

Contrary to what the authors conclude in the final line quoted above, this statement is not an acknowledgement of any role played by atomic theory. Mendeleev's well known reluctance to connect data points reporting properties of the elements merely shows that he regarded the elements to be strictly individual rather than their all being made of the same substance. Mendeleev consistently argued against the unity of matter and against Prout's hypothesis to that effect. I suggest that the quotation above is not necessarily a reflection of Mendeleev's views specifically on atoms of the elements.

Mendeleev as a positivist

If Mendeleev was so clearly in favor of atomic theory, and used it in the course of discovering chemical periodicity as Niaz et al. argue, it behooves them to explain why Mendeleev himself should have gone to such lengths to conceal this fact. Niaz et al. are clearly aware of this problem since they turn to a discussion of positivism in order to provide a possible answer,

> Throughout the 19th century positivism was the dominant philosophy, which led all scientific work to be based strictly on experimental observations and all hypothetical

propositions were considered speculative and hence non-scientific (Brush, 1976; Gavroglu, 2000; Holton, 1992). Mendeleev was clearly aware of this and on many occasions went out of his way to emphasize that the perioidic 'law itself was a legitimate induction from the verified facts' (Mendeleev, 1889, p. 639). In the Faraday lecture, Mendeleev emphasized the inductive aspect of the periodic law in the light of the antiatomist Marcellin Berthelot's (1827–1907) criticism: 'the illustrious Berthelot, in his work Les origins de l'alchimie, 1885, 313, has simply mixed up the fundamental idea of the law of periodicity with the ideas of Prout, the alchemists, and Democritus about primary matter. But the periodic law, based as it is on the solid and wholesome ground of experimental research, has been evolved independently of any conception as to the nature of the elements' (Mendeleev, 1889, p. 644, emphasis added). Apparently, Mendeleev's dilemma was that on the one hand he could rightly claim that the periodic law was based on experimental properties of the elements (an aspiration of scientists in the late 19th century), and yet he could not give up the bigger challenge, viz., the possible causes of periodicity, and hence the importance of atomic theory. (Niaz et al., 2004, p. 275)

In previous publications I have argued that Mendeleev was not in fact a positivist given his willingness to make speculations about the nature of the elements, to make predictions and to correct the atomic weights of several elements. As I have suggested, had he been a positivist Mendeleev might have adhered more closely to the experimental facts rather than allowing himself such flights of fancy as contemplating yet unknown elements (Scerri, 2007). As I have also argued, Mendeleev had some well-developed views on the dual nature of elements as 'basic substances' that were abstract and the seat of all properties on one hand, and elements as 'simple substances' which could manifest as various allotropes on the other hand.[2] Mendeleev also stressed that his periodic system was primarily a classification of the elements as abstract basic substances and not as simple substances (Paneth, 1962; Scerri, 2006). Again, such a view does not appear to be typical of a positivist who might be inclined to pay greater attention to the observable sense of an 'element' rather than its more abstract counterpart of an element as a basic substance.

Whereas the authors imply that Mendeleev's public statements were made for 'political reasons' and that he was falsely trying to pass himself off as a positivist such an interpretation seems a little far-fetched. We propose that Mendeleev was primarily expressing his disdain for the notion of the unity of matter rather than trying to assert his allegiance to positivism. Mendeleev did in fact eschew positivism but not necessarily because he supported the importance of atomic theory.

Niaz et al. continue by claiming that their task has been accomplished in beginning their section 3 with the statement,

> After having provided evidence for the relationship between periodicity and atomic theory in the development of the periodic table by Mendeleev, in this section we present arguments as to how predictions play an important role in scientific theories. (Niaz et al., 2004, p. 276)

Contrary to this claim we propose that the authors have not in fact adduced any evidence to support such a relationship.

Prediction, novel and otherwise

Niaz et al. proceed by elaborating on how Lakatos considers that predictions made by any scientific theory are of paramount importance. Although they note in passing that Lakatos wrote a footnote to say that post-diction should be regarded as a variety of 'prediction' the authors seem not to grasp the full worth of this concession (Worrall, 1998). Rather than being a mere footnote, the question of widening the meaning of 'prediction' to include post-diction, or accommodation, as it is sometimes termed, formed a major theme in Lakatos' work. This first came about when his then PhD student Zahar pointed out to Lakatos that Einstein's general theory of relativity had been accepted as much for being able to calculate the advance of the perihelion of mercury (an accommodation) as for the strictly novel prediction of the bending of starlight which was subsequently confirmed (Zahar, 1999).

In addition many articles have sought to explore this issue more deeply in the context of the periodic table (Scerri, 1996). Although Niaz et al. cite some of these papers they seem to miss the central point since they immediately return to discussing Lakatos and his view of prediction in the narrower sense of novel prediction. Each time that predictions are referred to on the same page, Niaz et al. explain that they are discussing *novel* predictions.[3] While still avoiding the central issue of the meaning and role of prediction in the work of Lakatos, Niaz and colleagues claim that they can cast light on the debate between Brush on one hand and Scerri and Worrall on the other.

> 'The time-honoured empirical criterion for a satisfactory theory was agreement with the observed facts. Our empirical criterion for a series of theories is that it should produce new facts. The idea of growth and the concept of empirical character are soldered into one' (p. 119, original

[2] I have used the terminology due to Paneth (Paneth, 1962). It is remarkable that Niaz et. al. never once refer to this all-important philosophical aspect of Mendeleev's work in the article under discussion.

[3] There are four such uses of prediction in the narrow sense of novel prediction just on page 276 of Niaz et al.

> emphasis). This helps to understand the controversy between Brush (1996) and Scerri & Worrall (2001) with respect to the role played by accommodation (agreement of observed facts with the theory) and prediction of new elements by Mendeleev. Following Lakatos, it appears that both accommodations and predictions are equally important for progress in scientific theories. (Niaz et al., 2004, p. 227).

In fact Brush and Scerri and Worrall are all perfectly aware of Lakatos' view that predictions and accommodations are equally important. Apparently unbeknownst to Niaz et al., Brush has for many years pursued the relative virtue of novel predictions and accommodations in a number of scientific theories from different fields (Brush, 1995). Swimming against the general consensus, Brush has claimed that many theories, especially in physics, were accepted as much for their successful accommodation of already known data as they were for making novel predictions. But when it came to chemistry, Brush was at first reluctant to believe that the equal importance of novel prediction and accommodation had a role to play in the discovery of the periodic table. For some time Brush claimed that the acceptance of the periodic table was the one important case in which novel predictions had in fact played the decisive role (Brush, 1996). The article by Scerri and Worrall argues that Brush should not draw back from even applying his view to the acceptance of the periodic table. More recently Brush signaled his change of mind while concluding thus,

> While chemists differed on the relative importance of prediction and accommodation, it seems fair to approximate the consensus as follows. The reasons for accepting the periodic law are, in order of importance: it accurately describes the correlation between physicochemical properties and atomic weights of nearly all known elements; it has led to useful corrections in the atomic weights of several elements and has helped to resolve controversies such as those about beryllium; and it has yielded successful predictions of the existence and properties of new elements (Brush, 1996, 612).

So rather than Lakatos explaining the disagreement between Brush and Scerri-Worrall, all parties have come to agree on this point to varying degrees.

After this brief 'nod' towards the current literature on the role of prediction, Niaz quickly return to discussing prediction in the narrow sense of novelty. Now they argue that Mendeleev's work must be regarded as a theory since theories are confirmed by novel predictions and, as is well known, Mendeleev made several predictions of new elements that were indeed confirmed. Again Niaz et al. allude to the controversy in the literature concerning *novel* predictions and accommodations but quickly cast this issue aside with the statement that,

> A detailed discussion goes beyond the subject of this study. (Niaz et al., 2004, 227)

Niaz et al. press on, once again returning to prediction of *novel* facts,

> It is important to note that Mendeleev's contribution, in contrast to many of his predecessors, cannot be considered as a mere accumulation of knowledge, but rather has the basic elements of a scientific theory. Similarly, van Spronsen (1969), in spite of his ambivalence with respect to Mendeleev's contribution, does recognize the role played by predictions (Niaz et al., 2004, 227)

It is quite remarkable in the view of the present author that van Spronsen of all people should be so painted as having begrudgingly recognized the role of predictions while largely concentrating on Mendeleev's "accumulation of knowledge".[4] The line taken by Niaz also continues to beg the question since even if we grant that prediction in the *novel* sense was all-important, this can equally well be achieved by a classification system or a law, rather than just by a theory, as Niaz et al. seem to suppose. Why should the only alternative to the inductive piling up of knowledge be just the use of theory? It would appear as though Niaz et al. regard the only alternative to inductivism to be the use of theory.

Even more worryingly, it appears that Niaz et al. believe that "hypothesis" or "conjecture", which is surely the natural alternative to naive inductivism, as emphasized by Popper, and later Lakatos, amounts to the use of a theory. But this is a very unfortunate conflation which runs the risk of encouraging such people as creationists and supporters of 'intelligent design' who notoriously confuse the term "theory" with the lay person's sense of "in theory" or guesswork, or mere conjecture.[5]

One can agree with Popper and Lakatos that naïve inductivism is an unreliable approach but this does not open the road to believing that all 'proper science' is only based on the use of "theory" as Niaz and colleagues seem to imply. The following section of the paper by Niaz faces the question more directly,

> Based on evidence provided in the previous two sections, here we present arguments as to whether Mendeleev's contribution was a theory or an empirical law. There seems to be considerable controversy among philosophers of science with respect to the nature of Mendeleev's contribution. Wartofsky (1968) clearly considers

[4] Van Spronsen is the author of the definitive book on the history of the periodic table (Van Spronsen, 1969).

[5] I am referring to creationists who are fond of saying that one should not believe Darwin's theory because it is only a theory.

> Mendeleev's contribution to be more than a simple empirical law:
>
> Mendeleev, for example, predicted that the blank space of atomic number 32, which lies between silicon and tin in the vertical column, would contain an element which was grayish-white, would be unaffected by acids and alkalis, and would give a white oxide when burned in air, and when he predicted also its atomic weight, atomic volume, density and boiling point, he was using the periodic table as a hypothesis from which predictions could be deduced. This was in 1871. (p. 203, emphasis added). (Niaz et al., 2004, p. 278)

Although there may indeed be some controversy among historians and philosophers concerning the nature of Mendeleev's discovery, it is rather misleading to imply that such controversy stretches as far as some authors considering Mendeleev's discovery to be of a 'theoretical' nature.[6] For example, in the above quote Wartofsky clearly states that he regards Mendeleev as having made a "hypothesis". Are we to understand that Niaz et al. are here even wanting to equate the notion of a hypothesis with that of a scientific theory?[7]

On the other hand Niaz et al. do not shrink from quoting several historians and philosophers who have stated quite categorically that they do not regard Mendeleev's approach to have been of a theoretical nature. This includes the philosopher Dudley Shapere who considers whether Mendeleev's achievement was a classification, a system, a table, or a law but concludes that it was more in the form of an 'ordered domain'. Conversely, the one possibility that Shapere excludes completely is precisely the notion that Mendeleev's discovery involves the use of a theory. But after quoting Shapere's view Niaz et al. say nothing to counter it. Similarly, they quote Bensaude as claiming that Mendeleev belonged to a positivist tradition. As the reader will recall Niaz et al. consider this to be an incorrect reading and yet they offer no argument against Bensaude.

Instead Niaz et al. seem to cast all philosophers and historians in the same light and declare them to be inductivists and presumably ignorant of Lakatos' insights into the nature of science,

> It appears that historians and philosophers of science generally conceptualize scientific progress to be dichotomous, viz., experimental observations lead to scientific laws, which later facilitate the elaboration of explanatory theories. On the contrary, Lakatos (1970) has argued that 'the clash is not "between theories and facts" but between two high-level theories: between an interpretative theory to provide the facts and an explanatory theory to explain them; and the interpretative theory may be on quite as high a level as the explanatory theory' (p. 129, original emphasis). (Niaz et al., 2004, p. 279)

Here Niaz et al. are finally revealing the reason for their rather unusual view that Mendeleev's work should be regarded as being of a theoretical nature. However, nowhere do they give a sustained argument in favor of the view that Mendeleev's work might have represented "an interpretative theory to provide the facts" in the sense of Lakatos. Furthermore this is quite a retreat from the view that has been put forward up to this point in the article which consists of a series of claims that Mendeleev's work was of the form of a "theory" *tout court*.

We believe that even this far more restricted sense of "theory", that now emerges, cannot be sustained since Mendeleev's scheme, for all it's value, did not present any form of interpretation of the form of the periodic table or why it was so effective at making successful predictions of new elements.

Niaz et al. on laws of science

It appears that Niaz et al. may regard their positive arguments in favor of the theoretical nature of Mendeleev's discovery to be insufficient to convince the skeptic because in the next section they proceed to adduce some negative arguments aimed at showing that Mendeleev's approach does not represent the use of a scientific law.[8] Of course one might want to ask whether it has to be one or the other, theory or law as Niaz seems to imply.[9]

In order to argue that Mendeleev was not operating with a scientific law, Niaz et al. appeal to the work of two contemporary philosophers of science, Nancy Cartwright and Ronald Giere. As is well known Cartwright, for example, has claimed that all laws of science 'lie' in the sense that they never apply to specific situations because they refer to

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[6] The only historian or philosopher of science to my knowledge that has argued for a 'theoretical' reading of Mendeleev's discovery is Michael Weisberg, whose article I have criticized in a recent publication (Scerri, 2012).

[7] Once again, creationists would no doubt welcome the notion that there is little difference between hypotheses and theories. Once again, creationists would no doubt welcome the notion that there is little difference between hypotheses and theories.

[8] In the final analysis the positive arguments by Niaz et al. for claiming that Mendeleev was acting as a theorist consist of one single quotation from the physicist Ziman who was perhaps not being too reflective and some statements about Lakatos and interpretative theory leading to explanatory theory.

[9] Other viable possibilities for the status of Mendeleev's work have already been mentioned. It could be regarded as a classification, an ordered domain rather than just either a theory or a law.

unrealizable idealizations. But in making such a move Niaz et al. immediately blunt their argument in a serious way. Here they are supporting philosophers who regard *all* scientific laws as being fundamentally flawed notions. Niaz et al. are no longer providing an argument that specifically applies to the case of Mendeleev. Of course if *all* laws are guilty of telling lies then Mendeleev's putative periodic law should be similarly regarded. This is too easy. If as Cartwright suggests all scientific laws strictly lie, then of course Mendeleev's law too cannot be considered as a strictly valid law of science. But even if this were the case it would not necessarily support the view championed by Niaz et al. that Mendeleev's approach should be regarded as a theory. Again it should be clear that here are more than just these two options on offer.

Is the periodic table an interpretative theory?

My final point concerns the concluding section in Niaz et al. in which the authors claim,

> Finally, it is concluded that Mendeleev's contribution can be considered as an 'interpretative' theory, which became 'explanatory' (Lakatos, 1970) after the periodic table was based on atomic numbers. (Niaz et al., 2004, p. 280)

It is very difficult for the present author to see how the periodic table is supposed to have changed its status from that of an "interpretative theory" to an "explanatory theory" as a result of the discovery of atomic number. One may well concede that the arrival of atomic number, due to van den Broek and Mosely, resolved an important problem in the periodic table, namely the existence of pair reversals such as the case of tellurium and iodine. If these elements were ordered according to atomic weight, as they were up to the discovery of atomic number, the two elements fell in the wrong order according to their chemical behavior. Ordering them according to their respective atomic number results in the correct placement of the elements in accordance with their chemistry. But this important advance did little to *explain* the periodic table. In no way did the periodic table *per se* become more explanatory after atomic number was discovered.[10]

Rather the periodic table still awaited an explanation, which was soon supplied by Bohr's discussion of the electronic configuration of many-electron atoms followed by more detailed configurations by Stoner and Pauli (Scerri, 2007). But even then the periodic table had not suddenly been transformed into an explanatory theory. The periodic table is the *explanandum* rather than the *explanans* and so it remains to this day. The periodic table, in and of itself, is not a theory but requires a theory in order to explain why it has been such a productive scientific discovery. It is important for these views to be debated further and clarified if history and philosophy of science are indeed to be imported into science education in order to improve teaching. It would be interesting to hear how Niaz et al. respond to the points that I have raised in this article.

[10] Atomic number provides a more accurate means of ordering the elements but it does not say anything about where sequences along this one-dimensional Mendeleev line should recur. Atomic number provides a one-dimensional ordering of the elements only. It does not address the question of at which point along the sequence of the elements there is recurrence or periodicity. The second dimension of the periodic table is provided by electronic structure which is quite independent of the number of protons in any nucleaus.

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Mendeleev and the periodic table: A response to Scerri

*Mansoor Niaz**

ABSTRACT
This is a response to some of the issues raised by Scerri, "Some comments on the views of Niaz, Rodríguez and Brito (2004) on Mendeleev's periodic system". We thank the Editor, Dr. Andoni Garritz, for inviting us to write this response. However, due to limited space in the journal I shall restrict myself to responding to only the important issues. In order to help the reader I have maintained the same section headings as those of Scerri.

KEYWORDS: response, review, Scerri

Periodicity in the periodic table and atomic theory

According to Scerri, "They [Niaz et al.] claim that most historians take a *naïve inductivist* approach to the development of the periodic table and that they consider that Mendeleev proceeded on the basis of empirical observations rather than the atomic theory" (p. 3, italics added). Once again, Scerri writes: "It would appear that Niaz et al. believe that if they can show that Mendeleev indeed possessed the ability to 'speculate' then they can oppose the vast majority of historians of science who apparently *wrongly* hold that Mendeleev was not a speculator but merely followed the observational evidence like a good naïve inductivist" (p. 4, italics added). Actually, nowhere in the manuscript do we refer to present day historians or Mendeleev as *naïve inductivists.* On the contrary, we referred to their approach as an inductive generalization. A student of philosophy of science knows well that the two are different things. Furthermore, nowhere in our article do we refer to the approach followed by the historians as *wrong*. We simply tried to present an alternative interpretation of Mendeleev's contribution, which was accepted as such by the reviewers of *Studies in History and Philosophy of Science*.

In response to our step 4 (Niaz et al., p. 274), Scerri noted: "Needless to say, this passage does not provide very compelling ammunition for Niaz et al. for at least a couple of reasons. Firstly it is a statement made by Mendeleev a full 20 years after the discovery of chemical periodicity. Secondly it is a statement made to a general audience at an award lecture by a scientist looking back at his achievements. Such statements are notoriously prone to *grandiose generalizations …*" (p. 6, italics added). Now, let us consider the following information:

a) According to Kaji (2003), as early as 1864, in a lecture on theoretical chemistry Mendeleev stated: "In fact, while the atomic theory was strongly supported by the law of definite chemical compounds, it was also challenged by the so-called indefinite compounds" (p. 194). This shows Mendeleev's ambivalence (which we stressed throughout our article) and also the acknowledgment of the relationship between the atomic theory and the law of definite proportions. Interestingly, these views were expressed by Mendeleev a full 25 years before the Faraday Lecture;

b) Van Spronsen (1969) considers Mendeleev's Faraday Lecture of 1889 as "highly influential" (p. 348). Furthermore, a review of the literature shows that most scholars cite Mendeleev's Faraday Lecture.

Readers will note that we have presented counter evidence with respect to the two arguments put forward by Scerri and hence our line of reasoning in Step 4 has been upheld.

In response to our Step 5 (Niaz et al., 2004, pp. 274-275), Scerri stated: "Contrary to what the authors conclude in the final line quoted above, this statement is not an acknowledgment of any role played by atomic theory … Mendeleev consistently argued against the unity of matter and against Prout's hypothesis to that effect" (p. 8). If we read once again Mendeleev's quote in Step 5, it will reveal that it was not the question of Prout's hypothesis (which Mendeleev denied and we noted in our article, p. 275), but rather Dalton's law of multiple proportions, which was at stake. Actually, Mendeleev (1889) himself explains the data presented with respect to the oxides in Step 5, in the following categorical terms: "The periodic law has clearly shown that the masses of the atoms increase abruptly, by steps, which are clearly connected in some way with Dalton's law of multiple proportions …" (p. 642). It is interesting to note that we cited this explanation by Mendeleev in Niaz et al (2004, p. 275) and for some reason Scerri decided to ignore it! Similarly, Weisberg (2007) has endorsed a similar thesis: "Mendeleev showed that the quantity of oxygen in the oxides was a periodic function of the element's group (column) on the Periodic Table … This can be accounted for by the Periodic Law, but would have remained mysterious otherwise" (pp. 214-215). Furthermore, Dalton's law of multiple proportions is considered as evidence to corroborate the atomic theory by the dean of modern chemistry: "The discovery of the law of multiple proportions was the first great success of Dalton's atomic theory. This law was not induced from experimental

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results, but was derived from the theory, and then tested by experiments" (Pauling, 1964, p. 26). The similarity between the explanation provided by Mendeleev in 1864, 1889 and its endorsement by Pauling in 1964 is striking indeed!

Mendeleev as a positivist

According to Scerri: "Whereas the authors [Niaz et al., 2004] imply that Mendeleev's public statements were made for *'political reasons'* and *that he was falsely trying to pass himself off as a positivist* such an interpretation seems a little far-fetched" (p. 9, italics added). This is a gross misrepresentation of our views, as nowhere did we use such expressions, either implicitly or explicitly. On the contrary, we argued that Mendeleev was rather ambiguous/ambivalent with respect to the atomic theory and its role in the periodic table.

Prediction, novel and otherwise

According to Scerri: "… Lakatos wrote a footnote to say that post-diction should be regarded as a variety of 'prediction' the authors seem not to grasp the full worth of this concession" (p. 10). This is surprising indeed! It was Lakatos who included a footnote and hence Scerri's quarrel is not with us.

With respect to novel predictions Scerri states: "In addition many articles have sought to explore this issue more deeply in the context of the periodic table. Although Niaz et al. cite some of these papers they seem to miss the central point since they immediately return to discussing Lakatos and his view of prediction in the narrower sense of novel prediction" (p. 10). Readers would certainly have liked to see some references that treat 'this issue more deeply'. However, Scerri provides not a single example.

According to Scerri: "The article by Scerri and Worrall argues that Brush should not draw back from even applying his view to the acceptance of the periodic table" (p. 11). Actually, Brush (2007) has not drawn back and stated clearly: "Having found little evidence for predictivism in physics, I did find it in chemistry, in particular in the case of Mendeleev's periodic law" (p. 257). So what is the argument!

Once again, Scerri asks a rhetorical question: "Why should the only alternative to the inductive piling up of knowledge be just the use of theory?" (p. 12). Readers would have liked to know some alternatives and Scerri provides none.

According to Scerri: "Are we to understand that Niaz et al. are here even wanting to equate the notion of a hypothesis with that of a scientific theory?" (p. 13). The relationship between hypotheses, predictions and theories is important in both science education and the philosophy of science. According to Lawson (2010): "Persons at Level 1 view science as an inductive and descriptive enterprise. Persons at Level 2 view science in terms of hypothesis generation and test. Persons at Level 3 see science as theory driven. That is, theories are generated and their postulates are tested via planned tests with predicted consequences and *theories are used to generate specific hypotheses*, which are in turn tested in a similar manner" (p. 257, italics added). Interestingly, Lawson considers our interpretation of Mendeleev's contribution as an example of Level 3 epistemology. According to Brush (2007): "It should be recognized that physicists (and some other scientists) use the word 'prediction' to mean 'deduction' (of an empirical fact from a hypothesis or theory) regardless of novelty" (p. 257, n. 1). This clearly shows that hypotheses, predictions and theories are intricately related. Consequently, Wartofsky's (1968) assertion that Mendeleev, "… was using the periodic table as a hypothesis from which predictions could be deduced" (p. 203), necessarily refers to a theoretical framework. On page 13, Scerri reproduced this quote, but without the necessary quotation signs, thus seeming to attribute this statement to Niaz et al (2004). We consider this to be a misrepresentation. In this context, it is important to note that Scerri in an endnote (n. 8) states, "… arguments by Niaz et al. for claiming that Mendeleev was acting as a theorist consist of one single quotation from the physicist *Ziman who was perhaps not being too reflective* …" (p. 17, italics added). A novice student of philosophy of science may wonder if this is how philosophers reason when the evidence goes against them.

In another endnote (n. 6), Scerri states: "The only historian or philosopher of science to my knowledge that has argued for a 'theoretical' reading of Mendeleev's discovery is Michael Weisberg, whose article I have criticized in a recent publication (Scerri, 2012). This makes interesting reading as Scerri's criticism of Weisberg was published in a journal whose Editor is Scerri. Once again a novice student would like to know if Weisberg was invited to respond. Furthermore, readers would like to know what exactly Weisberg (2007) asserted: "… Mendeleev had no empirical knowledge that there were any empty slots to be filled … He first needed to hypothesize the existence of the missing elements by analyzing the *theoretical structure he had created*. Then he was able to use the trends posited by the Periodic Table to make predictions about the properties of the 'missing' elements. This prediction was a theoretical, not merely classificatory, achievement" (p. 214, italics added). Now as Scerri claims that he criticized Weisberg's thesis, let us see what exactly was rebutted: "Unfortunately, Weisberg says nothing to support his claim that Mendeleev examined 'the theoretical structure that he had created'. This claim need to be motivated by some reference to Mendeleev's own writings, although I do not think this will be possible from my knowledge of the Russian chemist's writings" (Scerri, 2012, p. 277). Indeed, it would be interesting to see what Scerri found in the 'Russian chemist's writings'. For the time being we have Weisberg's (2007) elaboration of Menedeleev's theoretical structure, that was ignored by Scerri: "When the elements were properly ordered, Mendeleev argued, one could see the periodic dependence of elemental properties on their atomic weight. This principle, which Mendeleev called *The Periodic Law*, is one of the bedrock principles which organizes chemistry" (p. 213).

Niaz et al. on laws of science

According to Scerri: "If as Cartwright suggests all scientific laws strictly lie, then of course Mendeleev's law too cannot be considered as a strictly valid law of science" (p. 15). A student of philosophy of science knows well the context in which Cartwright suggested that scientific laws 'lie', namely the inclusion of *ceteris paribus* modifiers can make the laws to be better approximations. In our view, if laws are at best approximations, so are theories and hence Mendeleev's theoretical framework needs improvement. This idea has been explained by Weisberg (2007) in cogent terms: "While it is true that Mendeleev's periodic system is in need of further theoretical explanation, the same could be said of any theory that is not a fundamental physical one" (p. 215).

Conclusion

Scerri has gone to considerable length (5845 words) to critique our views about Mendeleev's periodic table. Nevertheless, we have demonstrated (despite limitations of space) that none of his criticisms can be considered as valid, and that at times he simply misrepresents or ignores our position. Interestingly, we have shown that at least three philosophers of science (Wartofsky, Weisberg and Ziman) endorse the view that Mendeleev's periodic table can be sustained by a theoretical framework.

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