A RESPONSE TO SCERRI'S COMMENTARY

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Dr. Scerri raises several thoughtful points, both historical and philosophical, in his commentary (1) on my paper (2) about J. A. R. Newlands' classifications of the elements. I would like to respond to several of those points, to agree with some of them and to highlight some which we view differently.

I agree that my definition of periodic system was ambiguous and somewhat circular as regards "system." Let me try again. What constitutes a periodic system? It must be periodic and it must be systematic. In my article, I believe I specified what I meant by periodic: arrangement by atomic weight and grouping of elements with common properties; blank spaces for new elements and main group/sub group distinctions were not necessary. I was much less definite on what it meant to be systematic, specifying only internal consistency. Clearly more was needed. What I had in mind but did not explicitly define can be described as clarity of exposition and of classification: a classification system ought to be clear about which elements constitute a group of related elements.

Several other criticisms of my criteria for what constitutes a periodic system are, I believe, less well founded. Scerri states that I introduced an additional requirement, that a periodic system fulfill the criteria of Sheldon Lachman about "what constitutes a theory." Lachman's criteria are for theory preference, not for what constitutes a theory (3). I did judge Newlands' work by Lachman's criteria in addition to my (ambiguous) criteria for a periodic system; these were separate analyses that addressed different questions. Scerri comments that I introduced even further criteria (due to Thomas Kuhn) without mentioning whether they are consistent with either Lachman's or my earlier criteria of periodic system. On the contrary, my purpose of listing Kuhn's criteria was not to introduce another set, but to show that Lachman's list—which I find to be particularly clear—is not anomalous in philosophy of science. Indeed, I quoted Kuhn: "Together with others of much the same sort, they provide the shared basis for theory choice (4)."

Dr. Scerri suggests that I implied that the periodic system is a theory. I must admit to having had no conscious intent to imply such a thing; however, I recognize that my paper can be fairly read as suggesting just that, or at least as blurring the lines between theories on the one hand and classification systems on the other. Upon reflection, I do not wish to make such a suggestion, and I recognize that distinctions between theories and classification systems can be useful. Still, I think that classification systems and empirical laws are amenable to analysis under criteria for theory preference such as Lachman's, Kuhn's, or the like.

This may be a fruitful point for discussion. Must a theory be explanatory? predictive? or may it be simply descriptive? Clearly, a report of raw observations or experimental results is not a theory; such a report is descriptive, but not well organized. Empirical laws and empirical classification systems are also descriptive, but in a more organized way, correlating the observations upon which they are based; however, they need not be explanatory (*i.e.*, state why a relationship holds). An empirical law asserts a relationship between quantities generalized from individual instances and at least implicitly predicts relationships that can be measured in the future between the same variables. A classification scheme is also assertive, if not at least implicitly predictive, if it claims that it is a natural classification; its groupings assert that in one respect or another, A is like B and unlike C. Empirical laws and classifications are susceptible, at least in principle, of being formulated in alternative ways. In that they are assertive generalizations susceptible of alternative formulations, empirical laws and classification systems are similar enough to theories that Lachman's or Kuhn's criteria are appropriate for preferring one to another. This is not to deny that a distinction between theories on the one hand and classifications or empirical laws on the other may well be useful.

One main point of disagreement between Dr. Scerri's analysis of Newlands' work and my own is over my treating all of Newlands' work as a whole, which I found incoherent. He seems to read Newlands' classifications as ideas evolving over time. This interpretation is not unreasonable; however, I believe that the historical record supports my interpretation (5). One can conceptually separate triad-based classifications (which include predictions of undiscovered elements, including one of the element now called germanium) from order number classifications (including the "Law of Octaves" and later classifications). If one were to consider these two phases separately, then the triad phase is certainly not a periodic system because it did not embrace all elements. The law of octaves is a periodic system by my definition, although it is not as clear as I would like about what elements constitute a group of related elements. (The law of octaves falls short of the more elaborate versions of Mendeleev's classifications on several of Lachman's criteria, but those criteria are for preference, not for periodic system.) In short, if I regarded order-number classifications as displacing triad-based classifications, then I would admit that the latter constitute a periodic system. I do not think the historical record supports such an interpretation, though. Newlands' monograph, written years after the classifications, seems to embrace both of these phases simultaneously. In addition, the first of the order-number papers (August 8, 1864) was written soon after the last of the triad-based papers (July 12, 1864) without any explicit break.

Beyond these differences in interpreting the historical record, I believe Dr. Scerri's analysis misses the mark in several particulars. For example, he asserts that I failed to show how Newlands fell short of my criteria for periodic system or the criteria of Lachman and Kuhn, and that instead I followed a summary judgment on this matter by pursuing secondary issues, namely Newlands' substantial contributions. In fact, the section to which Scerri refers was titled "The Case for Newlands," and it treated those aspects of Newlands' work that *satisfy* parts of my definition of a periodic system. The next section ("Why Newlands' Insights Do Not Constitute a Periodic System") asserted that Newlands' work is not a periodic system because it is not systematic. While I admit the inadequacy of my definition of periodic system, this section certainly addressed the definition, criticizing the internal inconsistency of Newlands' writings. Finally, the last section of the paper ("Assessment Using Lachman's Criteria") rated Newlands' work on all six of those criteria, one by one. Scerri seems to think that I unjustly dismissed Newlands' prediction of germanium, made on the basis of atomic weight relationships, and overlooked the fact that Mendeleev's predictions were also based on atomic weights. Far from dismissing the prediction of germanium, I emphasized it because predictions of new elements were logically inconsistent with the later periodic classification (the law of octaves) which left no room for new elements. Furthermore, the basis of Newlands' (and Mendeleev's) predictions was not wrong but it was not the basis of Newlands' periodic classification. Scerri contradicts my assertion that Mendeleev's system included an "extensive list of deductions ... from the start," characterizing predictions in his original 1869 paper as merely "hinted at" by leaving empty spaces in his table. In fact, the 1869 paper contained several explicit deductions, including explicit predictions of two new elements (6). Finally, Scerri finds my "critique of Newlands over the question of atomic number" to be "to some extent misplaced and rather Whiggish." I do not consider my words on atomic number to be a critique: I credited Newlands for the ordinal number concept and quoted without contradiction Taylor's assessment of Newlands as a "pioneer in atomic numbers (7)." I went on to describe the differences between the modern concept of atomic number and Newlands' ordinal number, with no criticism stated, intended, or implied (but apparently criticism was inferred). Of course this section is Whiggish in that it describes current understanding of a past proposal.

Dr. Scerri offers some observations that I believe would have improved my paper if I had incorporated them; he raises some issues that thoughtful scholars can fruitfully debate and over which they may disagree, but he makes some points that I believe are not well founded.

REFERENCES AND NOTES

1. E. R. Scerri, "A Philosophical Commentary On Giunta's Critique Of Newlands' Classification Of The Elements," preceding paper.
2. C. J. Giunta, "J.A.R. Newlands' Classification of the Elements: Periodicity, But No System," *Bull. Hist. Chem.*, **1999**, *24*, 24-31.
3. S. J. Lachman, *The Foundations of Science*, George Wahr, Ann Arbor, MI, 1992, 3rd ed., particularly pp 50-51.
4. T. S. Kuhn, "Objectivity, Value Judgment, and Theory Choice," in *The Essential Tension*, University of Chicago Press, Chicago, IL, 1977, 320-339.
5. Newlands' writings on classifications of the elements are collected in J. A. R. Newlands, *On the Discovery of the Periodic Law and on Relations Among the Atomic Weights*, E. & F. N. Spon, London, 1884. Some of the relevant documents are available on the Internet at http://webserver.lemoyne.edu/faculty/giunta/newlands.html .
6. D. Mendelejeff, "Ueber die Beziehungen der Eigenschaften zu den Atomgewichten der Elemente," *Z. Chem.*, **1869**, *12*, 405-406; abstracted and translated into German from *Zh. Russ. Khim. Ova.*, **1869**, *1*, 60-77.
7. W. H. Taylor, "J. A. R. Newlands: A Pioneer in Atomic Numbers," *J. Chem. Educ.*, **1949**, *26*, 491-496.

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