Scientific Realism and the Periodic Table of Chemical Elements

Jonathan David Sides

Thesis submitted to the faculty of the Virginia Polytechnic Institute and State University
in partial fulfillment of the requirements for the degree of

Master of Arts
In
Philosophy

Richard Burian, Chair
Laura Perini
Joseph Pitt

July 13, 2006
Blacksburg, Virginia

Keywords: Philosophy of science, periodic table, scientific realism, scientific theories,
scientific representations.

Scientific Realism and the Periodic Table of Chemical Elements

Jonathan David Sides

ABSTRACT

The periodic table poses a difficulty for both scientific realists and anti-realists. The anti-realist has difficulty accounting for the success of the table during a period in chemistry when many theories and concepts changed; the spatial relations of current tables in use do not show fundamental changes from the original tables proposed by Mendeleev. Yet, most versions of scientific realism are based upon the understanding that theories are some collection of written propositions or equations. The table as an image successfully functions very much like a theory: it is an organization of known facts, has been used to make predictions, and is plastic enough to accommodate unforeseen novel facts. Assuming the truth of the representational relations between the table and the world poses interesting issues for the realist. Ian Hacking's entity realism and the structural realism of several philosophers are both possible versions of scientific realism that fail to account for the table. Hacking's version fails in this case because the role of representation is central to understanding the history of the table; structural realism fails because it diminishes to much the role that first order properties have as they relate to the formulation of the second order relationships that comprise the table. Philip Kitcher of *Science, Truth, and Democracy* leaves himself open to two interpretations about the metaphysics of pluralism. One of these is indefensible; the other is quite well supported by the plurality of successful periodic tables.

Table of Contents

Preface

This thesis is the product of a very fruitful and engaging time spent as a student in the philosophy department at Virginia Tech. As such, I owe tremendous thanks to the many people who helped with this project. Special thanks goes to my advisor and committee chair, Dr. Richard Burian, for his advice, insights, and most importantly, his patience in helping me work through this project. I also thank my other committee member, Dr. Joseph Pitt and Dr. Laura Perini for their willingness to assist with this project and their very valuable comments and criticisms. Additional thanks go to the rest of the philosophy faculty for their helpfulness in working through the thesis process and to the graduate students in philosophy as well as science and technology studies for their suggestions, friendships, and the many discussions that we had when we should have been working logic problems.

I thank the students of the University of Chicago and the attendees of Mephistos 2006 for their comments and critiques of early chapter drafts.

I thank my parents, Larry and Norma Sides for their endless support as I pursue my education in philosophy.

Lastly, I thank my fiancée, Meghan, for her willingness to proofread sections of this thesis and her loving encouragement towards the completion of it.

List of Figures

Introduction

This thesis tackles questions related to the implications of the periodic table of chemical elements for scientific realism. The questions that I examine relate to understanding what the scientific status of the table actually is and how we ought to view it. Of central importance is whether the table itself is a theory, or simply a representation of periodicity that is/can/or should be stated propositionally. Part of the early debate surrounding atoms centered on whether or not they were the fundamental parts of the universe, and indeed Mendeleev himself argued until his death that his atoms were indivisible. Due to the fact that we now conclusively know that atoms are not immutable, indeed, not the fundamental parts analogous to earth, air, fire, and water, how are we to treat the table? Is it just a useful construction for scientists who work at the level of atoms, or does the table represent a genuine picture of nature? I argue that the table offers a difficult case for both sides of the realist/instrumentalist debate.

My approach relies heavily an analysis of the historical changes both that led up to the acceptance of periodic tables and in the development of chemistry after such tables were sufficiently entrenched.

The philosophical literature and secondary scientific literature about the periodic table generally falls into three categories. The largest portion of the literature, even philosophical, is highly historical. Examples of how Mendeleev developed the periodic law include Leicester (1948), de Milt (1951), Goldwhite (1979), Emsley (1987), Brooks (2002), and Kaji (2003), though numerous other accounts exist. Several accounts of precursors to periodicity exist, including Berguryer De Chancourtois (1889), Kauffman (1969), Benfey (1992), and Cohen (2004). In analyzing why the periodic table was so successful initially, the literature credits the predictive power of the table (e.g. Lipton (1990)). However, Scerri and Worrall (2001) argue persuasively that prediction played a much smaller role than the textbook history suggests. My paper acknowledges this historical fact, but then makes the case that predictive power is epistemologically highly important for the debate about scientific realism.

The second category of literature consists largely of a debate over how much the table can be reduced to some physical theory such as quantum mechanics. Eric Scerri's work seems to exhaust this discussion, but of special note is the debate that he and Friedrich (2004) had in *Foundations of Chemistry*. Also of note is Kragh's (2001) account of early subatomic explanations of why the periodic table succeeded and the historical account of Hettema and Kuipers (1988). This is a very fruitful debate, but we should to step away from it for a moment and note the significance of the fact that the periodic table has managed to survive many changes in chemical theory.

The final category literature consists of a largely pedagogical debate about which exact styles or layouts of the table would best depict periodicity. A fine resource for this topic is Mazurs (1974). Since I look at the periodic system as an image the various depictions of the table are relevant. To some degree, the fact that virtually all tables show the same relationships between atoms is of significance. The differences between types of tables are accounted for in my third chapter.

My paper fits in this debate somewhat tangentially. For the most part, I use the table as a case study for jumping into the debate over scientific realism. In other words, the philosophical debates that occur over the periodic table should be momentarily silenced. Instead of addressing question about reduction to atomic theory, we should look at the very obvious things that we know about the table, namely that it is an image and that it has survived for (in the world of modern science) a relatively long time. The consequences of this investigation yield interesting consequences for more general questions about realism and theories.

Chapter 1: The periodic table, realism, durability, and theories.

Since Mendeleyev first formulated the periodic table in 1869 we have had numerous opportunities to dispose of it in favor of alternative taxonomies. My paper outlines these historical points, and then suggests that the fact that the periodic table endures through rather major changes in chemical theory and major chemical discoveries provides support for a scientific realist interpretation about what the table refers to—not only the existence of atoms, but especially regarding *relationships between atoms*. Determining exactly what form of scientific realism we are warranted in adopting is more difficult. I examine Hacking's entity realism as one likely candidate, but then reject it. The periodic table has survived changes in chemistry not in the same way that theories do—namely they maintain the same discursive content—but as an image. Hacking's representation/intervention and theory/entity distinctions, while useful in many cases, fail to capture what has happened in this particular case, especially as regards the realism of the relationships suggested by the table. Ultimately, in light of the fact that we have good reason to believe that the table refers to real relationships, I argue for a form of scientific realism that tries to recapture representation without relying on discursive theories. In other words, the success of the table indicates that we can use it as an image semi-independently of discursive theories in chemistry that may or may not explain the table or reduce it to some physical theory. Finally, this leads me to speculate on the theory-likeness of the table.

I address the following instances in which the periodic table has weathered significant changes, paying special attention to the first case.

- Discovery of Argon and the noble gases
- Atomic weight versus atomic number
- Mendeleev's insistence upon the immutability of the elements
- The initial problem of the rare earth elements (as genuine anomalies)

Mendeleyev first formulated the table at a very volatile (no pun intended) time in the history of chemistry. Some initial problems presented themselves, especially the problem of miscalculated atomic weights. The table offered a reasonable way to confirm atomic weight—and in a few instances was spectacularly successful. However, later developments in chemistry suggested that a proper way to classify the elements should not be based on atomic weight, but on atomic number. Furthermore, the discovery of the noble gases initially presented a problem for the table since they filled no apparent holes. Nonetheless, the table was able to accommodate their incorporation without major change regarding spatial relations. Mendeleyev presupposed the immutability of the elements and preached the doctrine to his death, yet the fact that he is entirely wrong on that point has not changed the usefulness, nor the formulation of the table. The discovery of the rare earth elements did not cause the disposal or major reformulation of the table. Likewise, change in physical theories about the structure and behavior of the atom have not changed the way the table works.

I investigate what implications this history has for the epistemic status of the periodic table. Nancy Cartwright's understanding of models being robust over theory change seems quite attractive here. However, we should not speak of the table as a model. Instead, I argue for the table as being a theory-like map that has survived several shifts in attempts to explain it.

Chapter 2: Structural Realism and the Table.

In this chapter I examine another alternative to standard theory-based accounts of realism. Structural realism comes in many varieties, but I pay special attention to the formulations proposed by Grover Maxwell, John Worrall, and James Ladyman. Broadly taken, structural realism says that the continuity across theory change indicates that we can be realists about the structure of theories; strong versions say that realist interpretations of science apply *only* to the structure of theories. The authors differ on why the structure is preserved and why we are warranted in being realists about it. In the end, I reject structural realism as a legitimate competitor with other forms of realism, but accept a weaker form of the position that admits that structural realists have point when evaluating the epistemic priority of features of theories.

Chapter 3: Pluralism Alternative formulations of the table.

I examine some of the didactic differences between alternative depictions of chemical periodicity. Since I argue in previous chapters that the table as an image is particularly relevant, the particular forms that the table takes should be as well.

Why have we settled on the standard form seen in most classrooms and textbooks? It was standardized in the 1960s and part of the explanation is that it was well suited for textbooks. Surely this is right, but I argue that that cannot be the only virtue that the modern long-form table has over its competitors. Some significant debate has been had over short-form versus long-form tables, but I examine some of the more exotic varieties to see what usefulness they might have. We should ask whether or not the two dimensional table (regardless of representation) captures all the relationships of periodicity, and see if any three dimensional table would be at all useful tools as opposed to scientific novelties.

In examining these questions I use the periodic table as a case study that presents tremendous difficulty for Phillip Kitcher's thesis in *Science, Truth and Democracy* that science looks at the world as we make it. This means that since scientific inquiry is the direct product of human interest, our classifications are ultimately grounded in a human perspective of the world. In adopting the form of structural realism that I do, I make the metaphysical assumption that the world has a specific structure. Kitcher leaves himself open to at least two interpretations, and one of these might confront the metaphysics that I assume up to this point. The bulk of this chapter is spent evaluating Kitcher by examining different depictions of the table and the whether or not the numerous representations give warrant to pluralism. I argue that while one interpretation if Kitcher is indefensible, that plurality of useful periodic tables provides evidence for a stronger (and more likely) interpretation of Kitcher. I will also evaluate other tables that are "wrong" in that they provide inaccurate taxonomies of the underlying structure of the world. I propose that even though the modern elements are not fundamental in the sense that the Greeks thought of elements or immutable in the sense that Mendeleev thought of them, the fact that the periodic table depicts their relationships so consistently and that chemists can profit so much by the table indicates at least in this case one interpretation of Kitcher must be wrong—it appears that nature really can be cut at certain *non-arbitrary* joints. This result, of course, does not undermine the claim that nature allows for a plurality of representations and will, in fact, support such arguments.

Chapter 1

The Periodic Table, Realism, Durability, and Theories

Introduction

Since Dmitrii Mendeleev and Lothar Meyer first formulated the periodic table in the 1860s and 1870s we have had numerous opportunities to fundamentally alter or eliminate of it. However, early in its history, the table became entrenched in scientific practice (though some physicists had their doubts until about the turn of the century) and never really suffered any serious revisions. I support the thesis that the only way to account for the success of the periodic table means adopting some form of scientific realism over and above any variety of constructivism. If scientific realism is not true, if instead some version of constructivism is true, then at several specific historical points we should have been able to find a flaw with the periodic table significant enough to dispose of it. I will outline the story of the discovery of argon and suggest that the event highlighted several misconceptions within chemistry. I suggest that a consequence of the durability of the periodic table in the face of rather major changes in chemical theory and major chemical discoveries indicates that we should adopt some form of scientific realism, but that many traditional accounts of realism that are theory-centric will prove inadequate for the specific features of the table. One of the earliest versions of scientific realism that does not rely on theories is Ian Hacking's (1983) entity realism (ER). After examining Hacking's position, I conclude that, although his approach is instructive, we should reject it in favor of an approach that does not diminish the priority of representations while maintaining that the realism need not rely on the truth (or approximate truth) of discursive theories.

The term ‘durability’ has at least two senses. We might talk of a thing being around a long time as being durable, or we might talk of a thing’s durability if it survives repeated battering. Usually the first case entails the second; the great pyramids are durable in both senses. However, a thing need not have been around a long time to be considered durable. Think of dent-resistant car doors as one example. Furthermore, being around a long time does not of necessity imply durability. Thus, the argument that follows will not fall prey to counterexamples from medieval science. Certainly the geocentric theory lasted a much longer time than has the periodic table, but we should consider the periodic table to be more durable in that it has survived in a much harsher environment. If the stringent standards, scientific technologies, and number of investigative researchers of the modern era were put in the context of the ancient world it is an open question whether or not geocentric astronomy would have lasted for several millennia.

The periodic table has survived these changes in chemistry in a manner similar to the way that discursive theories survive changes in science. Explanatory theories survive according to the degree that they maintain the same linguistic content (or trade it for appropriate new terms according to the most current science[1]); the periodic table has survived as an image (with some modifications), despite the fact that several theories associated with the table have been superceded. In other words, the success of the table indicates that we can use it as an image semi-independently of discursive theories in chemistry that may or may not explain it. Finally, I will advance the claim that the periodic table has enough theory-like characteristics in itself that it should hold a place in

[1] E.g., consider how a theory about biological development could persist in the mid-nineteenth century despite the fact that one of the key terms of the discipline—evolution—had taken on new and different meanings and thus needed to be replaced.

hierarchies of theories or, alternatively, that the concept of ‘theory’ should be expanded to include non-discursive representation. For example, I think it would be consistent with Lakatos’ (1977) account of science to suggest that the durability of the periodic table indicates that it belongs in the center of a research program as part of the “hard core” of chemistry, while attempts at reduction are part of the “protective belt” that buttress periodicity.

Unfortunately, to this point much scientific literature and philosophical discussion about the table has centered on whether or not some specific theory from physics adequately explains its features. See for example Eric Scerri’s numerous works on the table and quantum theory or Helge Kragh’s (2001) account of early subatomic explanations. Indeed, this reduction of chemistry to physics is a stranglehold from which philosophy of chemistry has been attempting to escape. Furthermore, nearly every physics textbook at the introductory university, and many chemistry ones as well, inform students that the periodic table has been adequately explained by quantum theory. Now this reduction may be correct, though I will point out later the account should not be trusted as being fully explanatory (see also Scerri, 2004).

<u>A brief history of the discovery of argon.</u>

Larry Laudan (1981) advances one of the strongest attacks against realism. He suggests that by examining the history of scientific theories and noting the tremendous and near universal failure of theories (except in cases of those currently in use), we are compelled to adopt some form of anti-realist attitude about empirical inquiry. This is the so-called “pessimistic meta-induction” that infers, on the basis of past failures of scientific theories, that current theories will likely also fail. Such a position invites many varieties of criticism, but the one that I will adopt here is rather limited in scope. The periodic table allows us (momentarily) to dodge arguments like Laudan's by stepping back from theories and dealing with the tools of science. This particular tool has a peculiar durability, even though theories rise and fall all around it. I suggest that for a moment, instead of looking behind the table at attempts to reduce it to physical theories, philosophers should actually look at it and see what the durability implies for realism, for progress, and even for the status of theories themselves. Eventually I argue that the table is like theories and might even itself be one, so the logical status of Laudan's challenge still stands. However, given the entrenchment of the table into scientific practice, scientific practice would not rationally operate on any other assumption than that the representations mapped by the table are at least approximately true.

Mendeleev's table (*figure 1, figure 2*) has suffered few major structural alterations in the past one hundred thirty years[2], but the discovery of argon presented a problem not foreseen by Mendeleev. Examining this case will provide evidence for the durability and theory-likeness of the table. First I will provide some background to show the difficulty that the discovery of argon presented. Notice the difference between *figure 1* and *2* in that all members of the right-most column of *figure 2* are missing from *figure 1* (they had not yet been discovered).

[2] Even though some changes in depiction have been made, the general epistemic structure has been preserved. See Chapter 3.

Table 1: Mendeleev 1869.

			Ti = 50	Zr = 90	? = 180
			V = 51	Nb = 94	Ta = 182
			Cr = 52	Mo = 96	W = 186
			Mn = 55	Rh = 104,4	Pt = 197,4
			Fe = 56	Ru = 104,4	Ir = 198
			Ni = Co = 59	Pl = 106,6	Os = 199
H = 1			Cu = 63,4	Ag = 108	Hg = 200
	Be = 9,4	Mg = 24	Zn = 65,2	Cd = 112	
	B = 11	Al = 27,4	? = 68	Ur = 116	Au = 197 ?
	C = 12	Si = 28	? = 70	Sn = 118	
	N = 14	P = 31	As = 75	Sb = 122	Bi = 210?
	O = 16	S = 32	Se = 79,4	Te = 128?	
	F = 19	Cl = 35,5	Br = 80	J = 127	
Li = 7	Na = 23	K = 39	Rb = 85,4	Cs = 133	Tl = 204
		Ca = 40	Sr = 87,6	Ba = 137	Pb = 207
		? = 45	Ce = 92		
		? Er = 56	La = 94		
		? Yt = 60	Di = 95		
		? In = 75,6	Th = 118 ?		

Figure 1, Mendeleev 1869; Reprinted in Bensaude-Vincent 2001, 137.

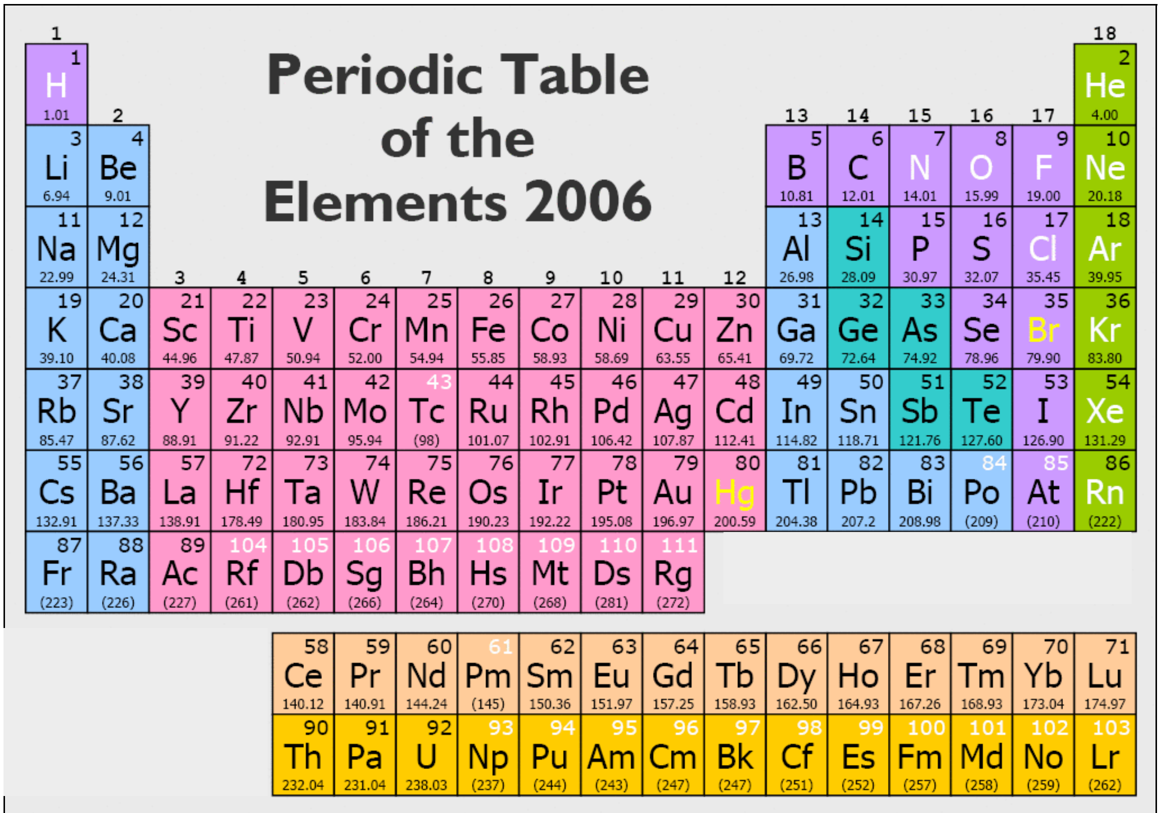


Figure 2, Molecular Research Institute, <*http://www.molres.org/cgi-bin/pt-request*> [3]

Mendeleev's first table captured a great number of chemical analogies—similar properties of elements that repeated in periodic fashion. He was able to relate (and expand) the "law of octaves"[4] and "chemical triads"[5] in a two-dimensional way such that the similarities among elements became immediately apparent, even to the point where he could predict the existence of as yet undiscovered elements and alter miscalculated atomic weights (the question marks in *figure 1* indicate places where Mendeleev

[3] In 1969 Mazurs compiled different versions of the periodic table. Mazurs takes the literature on over 100 types of periodic tables and organizes them, attributing each type to its originator and updating the tables to include all current (for 1969) scientific data. Hence, he attributes this type table to Mendeleev, though it is obvious that in 1869 many of these elements had not yet been discovered. Also of curious interest, while this medium-long table has become "the" version that freshmen learn, Mendeleev first suggested it only in a footnote and predicted that it would always be too unwieldy to ever become popular.
[4] The observation that noted that properties of elements tended to repeat periodically after every eighth element as the elements increased in size. These groups became the rows of periodic tables, though they are expanded beyond eight members after element 18.
[5] The observation that several groups of three elements (e.g., Li, Na, K; Cl, Br, I; etc.) shared very similar properties. These groups became the columns of periodic tables and were expanded well beyond three in many cases.

suggested changes be made). The table was a wonderful system in that it classified elements according to a broad range of properties and phenomena.

By the 1890s Mendeleev had published mature versions of his table in his famous inorganic chemistry textbooks and about seventy elements were known. Periodicity was still understood as a function of atomic weight, which presented a problem. “The piece that would not fit at this stage was the atomic weight of argon, which suggested a place for it on the periodic table where there was clearly no room for it” (Guinta, 2001)[6]. Early empirical data suggested that argon has an atomic mass of approximately 40 which would have placed it right between potassium (39) and calcium (40), thus splitting the alkaline and alkaline earth groups (represented as columns in the modern medium-long depictions) of the table. In late 1894 Lord Rayleigh and William Ramsay (published 1895) had made the original discovery of this gas by using two methods to isolate it from atmospheric air, one using high-voltage sparks and another using hot magnesium, both employed in order to remove nitrogen and other components of air. In January of 1895 they presented a full report to the Royal Society (London) about the discovery. The specific heat of the material was determined to be 1.66, implying that the gas was monatomic (thus, if accurate, indicating discovery of a new element) and the density was roughly twenty times that of hydrogen. Of particular importance was the complete inability to make the gas react and form compounds. If this new gas was an element, then it had an atomic weight of about 40.

But where is one to fit this new element? Not only does the proposed atomic weight squeeze argon into a spot where there is no hole, but its inert nature made it unique among known elements. Central to the periodic law was that as elemental size

[6] The majority of the following account derives from this source.

(atomic weight) increased, properties of elements repeated in periodic fashion. Long before Mendeleev and Meyer made their independent formulations of the periodic table, chemists had been aware of a similarity between the properties of elements and their atomic mass. For example, in 1816, Döbereiner noted the existence of numerous "chemical triads" already mentioned, groups of three elements that shared similar chemical properties and which followed a pattern such that the atomic mass of the 2nd element in the series was the mean of the 1st and 3rd.

Li (7)   Na (23)   K (39)   (39+7)/2 = 23

The success of the periodic table was in part due to the systematization of these relationships in periodic fashion so that mass and properties for all known elements are shown accurately and completely (here I gloss over much of the history and the science of the matter). So the problem with argon was not only that it did not fit the current versions of the table with respect to simple succession of atomic weights, but also that it shared properties with no known elements meaning that it had no chemical analogs; if the empirical data was awry and needed to be recalculated, chemists did not even have a hint of what values to look for. Yet, the experimental data for boiling points and melting points were sharp enough to suggest that they were correct, and suggestions that argon was really a compound (such as a trimer of nitrogen) were largely fantastic. Argon presented a genuine and problematic anomaly for periodicity and the periodic table.

In early 1895, helium was isolated, though it would not be until a few years later that suspicion subsided that helium gas was a mixture of multiple elements. In June, Ramsay, Collie, and Travers presented a paper in which they showed that the specific heat ratio was 1.66, with a density of about 2, and also inert. In 1898 xenon, neon and krypton were also isolated.

Even prior to the discovery of argon, however, Ramsay had speculated that the periodic table did have a space for another column. Indeed, he always had a place in the periodic table for argon. Problematic with putting it in a new group was that Mendeleev's table relied quite heavily on succession of atomic weights. In fact, in 1869 he predicted based on periodicity that several atomic weights needed to be changed. In some cases he was correct (gold) and in others he was famously incorrect (tellurium)[7], though many of these debates were still happening at the time of argon.

What happened, in effect, was that the periodic table as an image faced a serious challenge. The anomalous nature of argon was strong enough to suggest that the idea of periodicity as Mendeleev presented it was entirely mistaken. The further discoveries of other noble gases in fact vindicated Mendeleev's periodic law and showed that the problem was merely that argon belonged to a "family" of elements that 1) followed the periodic law very well, but 2) had not been discovered or suspected (except by Ramsay).

There is a difficulty in appreciating the nature of this problem from just looking at flat tables. In fact, the periodic law envisions the elements as a continuous series that contains properties that periodically loop back on each other. Really, the periodic table is like a Mercator projection of a globe that has been cut along one side and squashed flat. You can get from Tokyo to Anchorage by crossing the "cut"; likewise the series is continuous from Ne to Na, Ar to K, etc. (see *figure 2*). One can get a sense of this continuity by wrapping the periodic table into a spiral (*figure 3*). The argon problem was solved by suggesting that the table needed to be cut all the way down and inserting an

[7] Idhe, 246-7.

entire column. This would be like saying that a Mercator map was incomplete if a topographer had neglected to map an entire meridian.

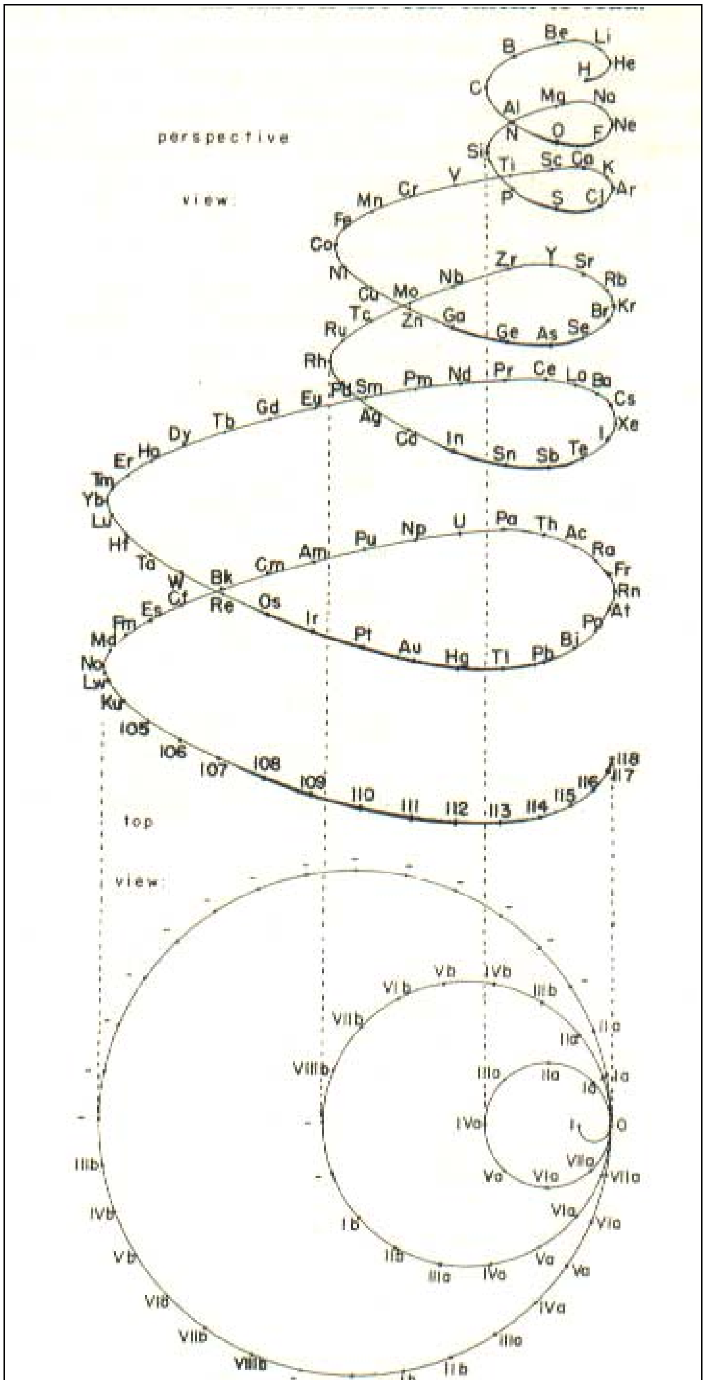


FIG. 82. Stintzing 1916. Type IIIA3–1

Figure 3, Stintzing 1916; as printed in Mazurs 1974, 75

So what is the philosophical significance of this argon story? The discovery of argon illuminated several problems with the periodic table, the sum of which, I conjecture, would be significant to an anti-realist about the table if the table was only of instrumental value and did not actually represent some real features of the world, even if those features were as yet not entirely articulated. The problems include:

1) Building periodicity upon atomic weight and not allowing for atomic weight inversions. This tension has since been alleviated by the work of Moseley[8] who discovered that emission lines correlated exactly with what he called atomic number, which was subsequently to become known as the number of protons in the nucleus of an atom. The table was subsequently modified so that succession was determined by the increase in atomic number without the apparent arbitrariness of the occasional atomic weight inversions, especially in the rare earths.

2) Lack of a place for inert atoms. This problem was significant only for the few years prior to the discovery of other noble gases and determination of their properties. However, the periodic table actually provided yet another prediction: if you place helium and argon in a column between the alkali metals and the halogens then you can make yet more predictions about holes left in other periods. In a case of remarkable anticipation, Crook correctly placed krypton and neon into his three-dimensional periodic system in 1898 *prior* to the establishment of density or atomic weight.

3) Elevation of the conflict between periodicity and kinetic theory. This issue is only a problem insofar as we attempt to reduce chemical laws to physical

[8] Brock, 340-342.

ones. This paper is not the place to elaborate related issues, but though we would like to resolve disputes between chemical theory and physical theory, there is no epistemic reason to avoid realism if we have different sets of laws across different domains. Perhaps the laws of nature really are Hacking's Borgesian library (219). Furthermore, I suggest that in conflicts between physics and chemistry there is no *a priori* reason that we must defer to the physical accounts.

There were also some additional problems with the early periodic tables not related to the noble gases, but which were eventually solved via interrelated inquiry (such as the discovery of more elements). For example:

4) The rare earths. Mendeleev's early tables exhibit absolute confusion about what to do with these elements. However, this is in part due to the fact that few of these elements were known at the time. Discovery and manufacture of these elements, coupled with the shift to atomic number noted earlier gave clear direction as to how to place them. The biggest problem that they pose now is how to represent them in the table. In the "medium-long" version that most of us are familiar with, these are the two rows that are "footnoted" out.

One possible alternative to the periodic table would be to abandon it in favor of graphical representations. While Meyer did publish several tables in 1870, he also did publish this graph (*figure 4*) that tracks atomic volume against atomic weight. While the shape of the plot in this graph does indicate the periodic recurrence of one property of the elements, it is not a proper representation of the periodic law (which states that *several* properties follow recurring patterns). A collection of several of these graphs might have been the first place for chemists to turn when or if they did contemplate discarding tabular representations. Even though a periodic table usually does not explicitly state these properties, it is a taxonomy that is constructed with several different properties in mind, thus those relationships can be extracted from it. For example, while *figure 2* nowhere speaks that electron affinity increases as you approach fluorine, this fact is used to construct the table. This particular feature (along with several others) is now encapsulated within the table. Furthermore, a collection of graphs like Meyer's is not visually reducible to a table. In other words, you cannot superimpose several of these graphs (e.g. atomic volume vs. atomic weight, ion size vs. atomic weight, electron affinity vs. atomic weight) over each other and expect the shapes of each graph to be similar enough in pattern to elucidate general trends of all the properties.

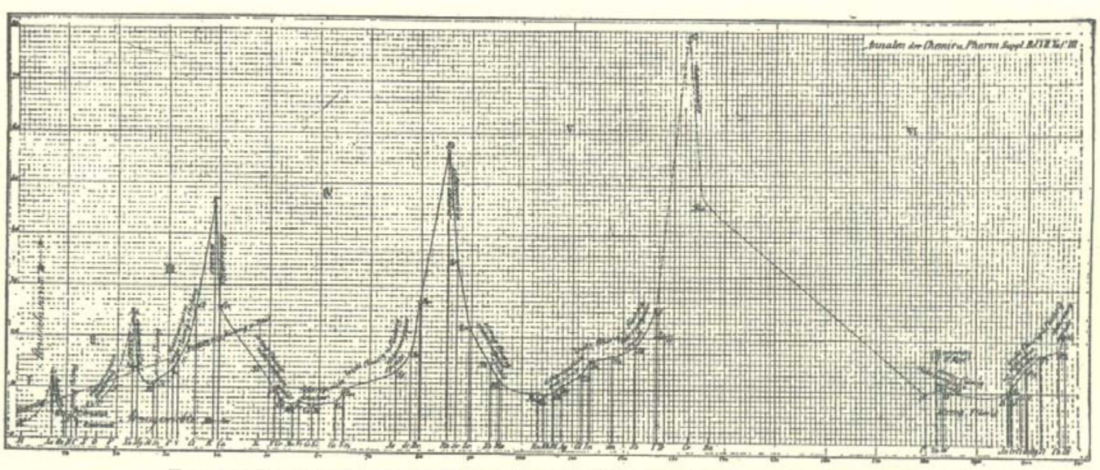


TABLE 3. Graph to illustrate periodic properties of atomic volumes, Lothar Meyer, 1870.

Figure 4, Meyer 437.

I should step back here and note that I have not made much use of Mendeleev's predictions based on his first formulation of the periodic law. True, they were quite spectacular, but my project is more related to the durability of the table as it faced significant problems. Indeed, I follow Scerri and Worrall (2001) and suggest that the reason that the table was accepted and has become so entrenched in chemistry is not prediction alone, and probably not even primarily predictive power, as much as accommodation of empirical facts, the weathering of the storms posed by unanticipated data. However, notice this hugely important difference between Mendeleev's table and Meyer's graph—even though Meyer's graph does allow for easier accommodation of unforeseen elements such as argon, you cannot make predictions about the existence of new elements from the graph. It has no holes to be filled. The plasticity of the table is remarkable in that it was able to expand enough to include novel facts without undergoing significant structural change other than the addition of a column. Furthermore, none of the problems posed above were able to batter the table enough for it to be abandoned.

Such a claim is by no means strong and daring—after all, what philosopher or scientist even bothers to question the legitimacy of the table anymore? More important is that if we really probe the history (and I admit that the argon example hardly captures all the contemporary issues at debate) we find that the periodic table met with numerous crises, the solutions of which linked together to provide the incredibly stable framework that constitutes the modern periodic table and warrants our acceptance of it by the accolades of realism. What *kind* of realism it warrants will be discussed below.

*****

In rows or columns next to Mendeleev's early tables you will notice that he often lists the possible compounds that corresponding elements can form with oxygen and hydrogen (e.g., *figure 5*)

Table 4: Mendeleev 1881.

Расположение элементов по периодам.

R^2O	I		Li = 7	K 39	Rb 85	Cs 133	— —	— —
RO	II		Be = 9	Ca 40	Sr 87	Ba 137	— —	— —
R^2O^3	III		B = 11	Sc 44	Y 89	La 138	Yb 173	— —
RO^2	IV	(H^4C)	C = 12	Ti 48	Zr 90	Ce 142	— —	Th 231
R^2O^5	V	(H^3N)	N = 14	V 51	Nb 94	Di 146	Ta 182	— —
RO^3	VI	(H^2O)	O = 16	Cr 52	Mo 96	— —	W 184	U 240
R^2O^7	VII	(HF)	F = 19	Mn 55	— —	— —	— —	— —
RO^4				Fe 56	Ru 103	— —	Os 192?	— —
	VIII			Co 58	Rh 104	— —	Ir 193	— —
				Ni 59	Pd 106	— —	Pt 195	— —
R^2O	I	H = 1	Na = 23	Cu 63	Ag 108	— —	Au 196	— —
RO	II		Mg 24	Zn 65	Cd 112	— —	Hg 200	— —
R^2O^3	III		Al 27	Ga 69	In 113	— —	Tl 204	— —
RO^2	IV	(H^4R)	Si 28	?? 72	Sn 118	— —	Pb 206	— —
R^2O^5	V	(H^3R)	P 31	As 75	Sb 120	— —	Bi 209	— —
RO^3	VI	(H^2R)	S 32	Se 79	Te 125?	— —	— —	— —
R^2O^7	VII	(HR)	Cl 35,5	Br 80	J 127	— —	— —	— —
Период:			1-й	2-й	3-й	4-й	5-й	6-й

Двойною чертою отделены: типические элементы.

Вверху таблицы элементы четных, внизу — нечетных рядов.

Figure 5, Mendeleev 1881; Reprinted in Bensaude-Vincent 1974, 141.

If you open any introductory chemistry textbook you will find numerous diagrams and graphs and representations of the relationships that the periodic table captures. In modern long-form tables for example, with a few exceptions to each of the following, as you move to the left and downward atomic radii generally increase in size, as you up and to the right first ionization energy increases, so does electron affinity, etc. Furthermore, nothing could be clearer than the fact that elements in many columns share numerous like properties, especially among the naturally occurring elements. Alkaline ions form compounds with halide ions, oxygen and sulfur have similar covalent bonding behavior, etc. What explains those regularities?

We may be justified in placing a great deal of confidence in some of the theories that are put forth to explain periodic regularity. Bonding behavior is likely accounted for by the behavior of valence shell electrons and some theory of orbitals and orbital hybridization likely underlies this. We could be realists about these theories and such a debate about them is definitely quite fruitful. But for the sake of argument, let us assume that, for example, any current theory of orbital hybridization is almost completely wrong in light of Laudan's claims about science. Let us look at our current theories in light of the failure[9] of all the old ones with a cautious eye and deny that we should be realist about theories. For the moment I grant this point entirely, though I will snatch it back in a moment.

Hacking (1983) avoids discussion of realism about theories by moving towards a realism about entities. It is possible, for example, that no one theory about the electron is true. However, even if we deny any or all particular theories we may conclude that even though our theories do not describe electrons correctly, electrons are themselves real—if we can do things with them, "spray them" as Hacking says. Therefore, philosophers of science should stop obsessing with representations of nature and look to interventions for

[9] This word is used for the sake of hyperbole. No good theory—one that was able to direct scientific research over any significant period of time and set a framework for successive theories—deserves to be called a failure, even if scientists have discarded it in favor of newer ones, ones that more accurately and even more truly explain natural phenomena. It is not a contradiction deny that theories are or should be merely instrumental attempts to explain nature and also maintain that the old theories are instrumental towards framing the context from which succeeding theories arise.

realism. We need not be realists about laws and theories to maintain a robust scientific realism.

Concerning the periodic table, entity realism work is a tempting solution, but that just does not go far enough to account for the case of the periodic table. I make stronger claim than Hacking—one that re-elevates the role of representation in realism. The difficulty in applying Hacking's entity realism to this the periodic table centers on the objects referred to by the terms used. 'Electron' refers to a concrete entity that may or may not really exist in the world. Our warrant for realism about the term involves "spraying" the entity. The periodic table refers to relationships among entities, and as such adds one layer to the picture. The mere existence of the entities in question (atoms) is either assumed or grounded in some series of experiments, and on top of that sits the periodic table that maps similarities, analogies, and differences of those entities. To further push the issue, it is not clear how exactly the distinction between representing and intervening holds here. In at least some sense, when scientists use the periodic table to predict the existence or properties of elements, they are using a representation to intervene in the world. Perhaps the meaning of intervention that I use here is not what Hacking would prefer—he likely means laboratory work that pokes and prods the physical world. If that is the meaning that he uses, then it must be expanded. How does one poke and prod a relationship like the fact that all the elements in the left-most column of *figure 2* form 1+ cations (generically: that all these elements share a similar set of properties)? Mendeleev used a novel cognitive map if these relations to guide interventions that enabled scientists to look into the world to fill in the gaps. Somewhat paradoxically, the history of table suggested that we could refer to things that were not at the time known to exist—and that those references were vindicated by the eventual discovery of suggested elements or elemental properties.

Even if we have difficulty settling on some particular linguistic explanations of the table, I argue that we are still warranted in believing that the relationships between atoms as represented in it are real, in part because of the extreme stability of the representation in the face of serious external pressures (like the discovery of argon), and in part because of internal pressure (the prediction of unknown elements). This is not because of we can "spray" the relationships but because we can represent them in a fashion that then allows tremendous predictive success, successful interventions, accommodation of both known and novel facts, and so on.

A great deal of philosophical literature only speaks of realism about theories. At this point, I suggest a threefold division of areas where we might profitably apply scientific realism:

- Theories and laws of nature
- Models, charts, tables, graphs, pictorial representations, etc.
- Entities, both observed and unobserved, and the relationships between them

Plenty of philosophers have already discussed the first group, and as we saw Hacking's concern was with the third group. My interests are primarily with the second area. In a given situation, it is possible to appropriately adopt the attitude of realism about any one of these three areas, even in some cases where realism is not warranted in one or two other areas (e.g., Hacking's suggestion that we can be realists about electrons and are not compelled thereby to be realists about any particular theory of electrons). By no means am I implying that every feature of science that falls into one of the top two areas is intended as a realist representation of the unobservable entities. Especially in the middle tier, many representations are employed that are clearly just intended as useful approximations. This is why it is separated off from the area of theories, because it is mixed bag of representations some of which should not even count as candidates for realism because they only survive in scientific practice as useful approximations. For example, we still occasionally depict electron orbitals like miniature solar systems, yet the point is not to be realistic about the depiction so far as to say that "this is how orbitals really are," but to try to conceptualize one small feature of electron orbitals. At the same time, some of these models clearly must be treated in a purely realist fashion. To call the Watson and Crick DNA model just an approximation misses many facts about what the model does. It is intended as a true or approximately true model of how the DNA molecule actually is structured, and it is not intended as a metaphor, as is speaking of solar system-like electron orbitals. Certainly even realistic models obey certain conventions, but as Perini (2005) has argued, so do written words. Thus conventionality alone is not a necessary reason to preclude images and models from functioning like linguistic statements as appropriate parts of scientific arguments.

As my discussion of the periodic table shows, interpreting tables and graphs as designating entities, kinds of entities, and relationships among those kinds enables us to understand how we can represent real relationships in nature even when we struggle to articulate theories to account for those specific relationships. Notice that in Mendeleev's and Meyer's day no account of the structure of the atom was available to explain (or attempt to explain) why, for example, fluorine and chlorine both form similar sodium salts. All that the table did was map the observable phenomena and display the analogy.

It did not show how or why the particular relationships did exist. Yet, the success the table to accommodate so many different and varied phenomena and for which underlying explanations were only later defended shows that this mapping is not mere coincidence and indicates that some real relationship holds. We my not have know exactly how to articulate the underlying structure of atoms, but we have certainly moved one step beyond mere phenomena that must be explained.

*****

The point of the previous section has been to recover realism about the entities and relationships representation by the periodic table without appealing to the status of particular explanatory theories. Earlier I allowed the supposition that all the underlying theories pertaining to unobservable features of the table might be wrong. I have tried to appeal to the durability and stability of the table as a reason to be a realist about it and if my argument succeeds then I can also suggest a way to recover realism about theories. I suggest that the periodic table is one feature of modern science that is so set in scientific practice that we can and must look to it when forming our theories about chemical atoms. The stability of the table both sets the parameters for acceptable theories about atomic structure and when, coupled with experimental data, indicates what constitutes a good theory. It adds more empirically grounded, yet emergent, constraints on the allowable theories Intuitively, the more constraints a theory satisfies while still adequately explaining the phenomena the stronger the indication that science is honing in on truer explanations of atoms. For example, any theory of chemical structure must now be consistent with the trend of increasing electron affinity as you approach fluorine on the table. If we were to pretend that the table did not exist and were unaware of this fact then clearly the range of logically possible atomic theories would be larger. By having representations like this table that limit logically permissible theories we have a smaller pool of theories to pick from. Obvious problems for this account would arise if several such stable facts as I have noted here were themselves inconsistent and allow for no unifying theory. Perhaps that is the state of the tension between physics and chemistry.

Indeed, there must be some sort of a feedback loop at work between representations like the periodic table and general theories in chemistry. The ontological status of the table is a legitimate question. I maintain that the best available account treats the table as a kind of map of the elements, providing relations of the elements to one another.[10] The table surveys the chemical landscape, and then maps numerous relevant relationships, though admittedly not all (in the same way that a map does not capture all relationships of a physical landscape). Maps have theory-like properties. Notice the theory-like features of the periodic table: predictions can be made from it, it can accommodate novel facts, auxiliary hypothesis around it can be altered to preserve it, etc. Perhaps it would not be too bold to suggest that the periodic table should count as a theory. Although I wish to avoid stepping into the debate about the reduction of the periodic table to quantum chemistry, let me use that issue to show how the feedback loop might work. Discrepancies exist between the theoretically expected energy states and experimentally determined energy states for numerous elements (Mazurs 105-107). These discrepancies can be best depicted with an alternative version of the periodic table (*figure 6*). Take note of the irregular way that certain elements (e.g. La, Mn, Tc, etc.) drop out of sequence. This is because the experimental values for the energy states of

[10] Several philosophers have suggested the theory likeness of maps. See Toulmin (1953), Polanyi (1962), and Giere (1999).

some elements do not match the theoretical values. Those theoretical values are on the one hand suggested by some versions of the table (*figure 2*), while alternatives diagram the experimental values. Whatever explanatory theory develops by quantum mechanics must be able to accommodate both types of tables. Furthermore, if the theory begins with the tables as significant features to accommodate, then it runs less of a risk of *ad hoc* adjustments.

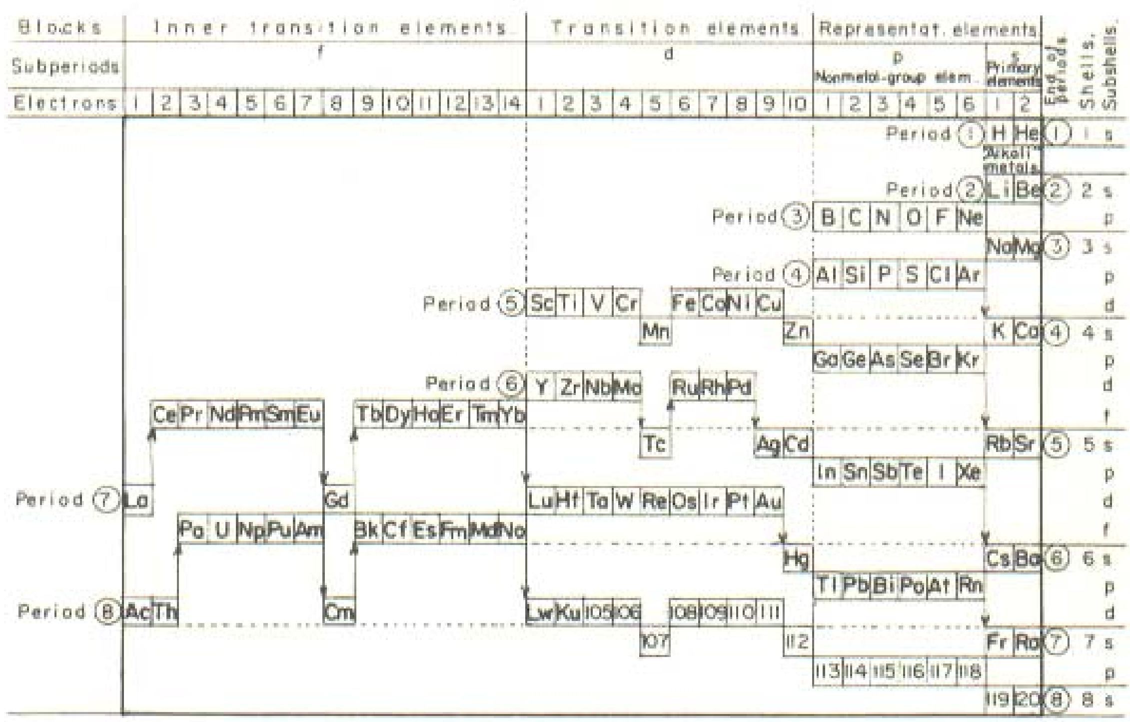


FIG. 138.   Pohl 1958.   Subtype IIIC5–3b.

Figure 6, Pohl 1958; Reprinted in Mazurs 1974, 135.

There seems to be no reason that theories must be put in discursive form, especially if a pictorial or graphical representation functions in the same way as a set of propositions that make up the core of a theory. If this position holds then we can revisit discussions regarding realist interpretations of theories, albeit with an entirely new area to explore. In conclusion we are left then with two options: either realists must modify their overall position towards the inclusion of the role that graphical and tabular images, or we must recognize that images often function like theories and therefore the definition of ‘theory’ should be expanded beyond discursive propositions.

Chapter 2
Structural Realism and the Structure of the Periodic Table

1. Introduction.

I will here continue exploring the thesis that a strong version of scientific realism provides the best available interpretation of the chemical periodic table of elements. Because of the historical events surrounding the table, especially the unique relationship of the table to various "supporting" theories, we cannot turn to a standard discursive theory-based realism.

As I argued in the previous chapter, we have good reasons to be realists about the table, but now let me clarify that position to push the issue at hand. Let us take as a starting point that support for a realist account of a particular feature of the world picked out by science requires at a minimum the demonstration of the satisfaction of two conditions: the Independence Condition (IC), that there really does exist a mind-independent world, and the Knowability Condition (KC),[11] that we can indeed know the relevant features of the external world under question. Now the periodic table satisfies both these conditions quite nicely, though in startling fashion given that philosophers generally apply these conditions to *theories*. I grant that my argument is largely of the "success of science" variety, but by adding the stability of the periodic table to its successes we meet both conditions in reasonable fashion. Not only did the table emerge out of various accounts of how to systematize the elements, but it also weathered several potentially devastating challenges (witness the discussion of the discovery of argon in Chapter 1). Granted, because of the various forms of the table, some discussion must be given to convention and instrumental uses of the table but this will be the subject of my

[11] From class notes: Philosophy of Science, Virginia Tech, Fall 2005.

final chapter. For now it will suffice to say that not just any type systematization could have all the advantages of the periodic tables that we inherited (with some modifications) from Mendeleev, but that the one we have is both highly stable and highly successful.

The predictive success and accommodation of novel facts by the table provides further support for satisfaction of both conditions. The fact that the table was formulated prior to the discovery of underlying atomic structure indicates very strongly that we have mind-independent features to the world. Furthermore, we can trust that these features are knowable and not just constructed (otherwise the discovery of underlying atomic structure would be the "miracle" that science disallows). So while the table itself might satisfy these minimal conditions for realism, they are not necessarily met by the many theories that, historically, have purported either to support or to explain the table. That is not to deny that at some point in time (even now, perhaps) we might have both proper explanatory theories about the table and sufficient justification for being realists about them, but as of now such confidence is not necessarily warranted. I need only appeal to the contemporary debate and lack of consensus as to whether or not the periodic table in any configuration is adequately reduced to and explained by quantum physics (Scerri, 2000). Despite these circumstances, we really do know things about the world from the table itself, such as trends correlating electronegativity and placement of the elements on the table. In the last chapter I turned to Hacking's entity realism (ER) as one possible candidate to capture the theory-independent realism that (I argue) is shown by graphic representations of the periodic table. On the whole, ER to be rather inadequate because the table does not just capture features of the atoms as kinds (regardless of what theory we hold to about atomic structure), but as in the electronegativity example, the primary purpose of the table is to map relationships among and between elements; the periodic table maps the *periodic* repetition of chemical properties. On the one hand it thus "represents" the world, so Hacking's demotion of representations is perhaps too hasty for the realist. However, he was operating largely with representations as theories, so perhaps we owe him charity on that point. However, the periodicity of the table practically begs for explanations by theorizing; thus, the current debate about reduction to quantum physics is both necessary and expected.

The table survives semi-independently of any of these explanatory theories. The argument is a historical one: changes in theories in chemistry have not fundamentally changed the table, but the entrenchment of the table adds a constraint as to what theories in chemistry about elements are adequate and acceptable. It is possible that the periodic table may serve as a paradigmatic case supporting a form of realism called *structural realism* (hereafter SR). This position suggests that philosophers are warranted in being realists about the structure of scientific theories, but not the content, either because there just is no content (such as first order properties) to fill the structure or the content is for some reason or other inherently unknowable (the KC either is not or cannot be satisfied except with regard to structure). In other words, SR purports that we can be realists about the correspondence between the structure of theories and the structural features of the world, but we do not have warrant beyond that. In some sense SR could not be more incompatible with ER. ER absolutely depends on the existence of theoretical entities even though it tries to gloss over the actual theories by moving right to what can be done with the entities; no version of SR has any commitment to the existence of any particular theoretical entities. On the other hand, both versions do share a similarity in that they try to divide out the actual content of the theories. ER moves downward towards a correspondence about entities while SR moves upward toward structures and their relationships to other strucutres. ER is clearly an epistemic position with metaphysical consequences; some formulations of SR just take epistemic stances, or purportedly just take epistemic stances, and some actually make stronger metaphysical claims.

This chapter first examines some of the literature about SR and sort through various different articulations of SR. Just as there are many varieties of realism, there are many varieties of SR, and I will examine three of them in particular. I will try to create the most charitable formulation of each version of SR as it pertains to the history of the periodic table and various issues already discussed. Ultimately, I reject these formulations of SR as being inadequate to account for the current status of the periodic table. I argue that we can make legitimate knowledge claims about the chemistry of the periodic table, but that no formulation of a structure/content dichotomy will be able to say that each and every legitimate knowledge claim is one about structure and that none of these claims can be considered to fall on the content side. In conclusion, however, I will examine matters from the other direction by taking SR as my starting point and then attempting to expand the approach of SR beyond the domain that structural realists have deemed appropriate. Out of this dialectic I hope to construct a form of realism very similar to SR by using the periodic table to expand on some comments made by Psillos (2001) and provide an articulation towards a new version of realism.

2. What is Structural Realism? Three Versions.

Structural realism has its roots in the writings of (at least!) Russell, Poincaré, and perhaps Carnap. What follows will not deal with these sources directly, but only with how these philosophers make their appearances in the more contemporary literature. Although SR has several types of proponents, I believe that three rather distinct positions emerge, and these are best exemplified by Grover Maxwell, John Worrall, and James Ladyman. This section of the paper will be organized around a review of their contributions and certain critiques of their work germane to my case study.

Maxwell

We can begin our survey of SR with Grover Maxwell (1962, 1970). For Maxwell, SR is an *epistemic* constraint. It appears that Maxwell coined the term 'structural realism', but we will quickly find that his account differs markedly from several other accounts that bear the same label. He attempts to defend a strong form of scientific realism in the face of ontological discontinuity, the "pessimistic meta-induction" that claims that more or less all scientific theories of the past have somehow failed or been superseded, therefore we lack justification for believing that current theories will not fail is like fashion.

Maxwell's strategy involves taking a given statement from a theory and then replacing it with its corresponding Ramsey sentence. A Ramsey sentence, in Maxwell's formulation, replaces all "first order" terms with existential variables. For example, almost any theory about electrons might include the sentence 'Electrons have a charge of negative-one' (dub the sentence E). However, according to Maxwell we are ignorant of the intrinsic nature of entities, so we should replace E with a sentence that substituted an existential variable for the purported designating term 'electron'. Properties of unobservables like electrons and atoms just cannot be the referents of theoretical terms because we have knowledge not of direct properties, but of phenomena. Maxwell is a realist (following Russell here) in that he believes in an isomorphism between the structure of theories and the structure of the world. However, as Psillos (2001) notes, on this account there is simply nothing left to a theory "except formal properties and observable properties" (S17) and the formal properties are just those that are unobservable or perhaps merely as yet unobserved. Maxwell does not wish to say that the structure of phenomena is isomorphic with the structure of the world, but his view nevertheless appears to reduce to phenomenalism. Furthermore, on Maxwell's account, if the Ramsey sentence captures the phenomena then of logical necessity it is true. Again following Psillos, further non-structural constraints must be placed on a Ramsey sentence system, but as soon as we move this direction we begin to lose the uniqueness of structural realism that makes it a competitively viable alternative to other formulations of scientific realism. Some philosophers (e.g. Votsis, 2003) still follow formulations of SR that approximate Maxwell's approach, but most appear to adhere to some other conception of the theory.

As far as applying Maxwell's SR to the periodic table goes, this theory will not really capture all the features that I outlined in the previous chapter, especially how we get to the relationships among atoms. Relationships are second-order properties (Sellars 346-7). They are not properties of particular atoms as such, but higher order attributes of the collection of all atoms, whereas the size of an atom is a first order property. Where Maxwell's system fails is that there has been a constant historical interaction between the first and second order properties. For example, a serious drawback to his argument is that insofar as the elements not-yet-found were predicted, a whole slew of first order properties (such as density, specific weight, melting and boiling points, etc.) were also predicted (Van Spronsen 1969, 220-223) along with the mere existence of an element that had such and such atomic weight. Furthermore, an obvious advantage of the table is that first-order properties of elements and the compounds that they can form can frequently be easily deduced by simply picking out the location within the structure, i.e., the location such as in the halogen column.

The relationship between first order properties and second order properties is a very tight one. In constructing the table, Mendeleev "organized" the elements based on these first order properties, thus (potentially) discovering the second order structure. However, Mendeleev used this second order structure to motivate the discovery of new elements by suggesting that they had such and such first order properties based upon holes in the table (see *figure* 1) and suggested that a few known elements had had some of their properties miscalculated. The first order properties of elements informed the constructing of the table in the first place; the second-order structure of the table fed back into predictions about the first order properties of as-yet-undiscoverd elements. To replace first order descriptions of the elements with existential variables is a pedantic exercise that too thoroughly denies their historical importance both for laboratory work and theorizing about second order analogies between elements. Furthermore, the discovery of new elements and their properties and the first order level serves as a vindication and confirmation of the truth of the second order relations.

<u>Worrall</u>

While much of the literature does discuss Maxwell's formulation of SR (especially with regard to his co-opting of Russell), most philosophers acknowledge that the contemporary debate about SR really begins with John Worrall (1989) (e.g. Ladyman, 1998). In Worrall's formulation, SR is a weak realist position in that he accepts IC but builds a realism that is much more tentative regarding the KC than direct or naïve realism. Rather, Worrall takes the historical stance that it just is the case that across theory change structure is preserved. If I may thus characterize him, his theory amounts to a "limited optimistic meta-induction" wherein we may infer that in cases of progressive theory change the higher order structure of theories is continuous, thus dodging the problem of ontological continuity. Worrall argues that we should not accept a scientific realism about the ontology of theoretical entities described in a theory (with a nod to the pessimistic meta-induction) but that we can be strongly realistic about the mathematical, logical, and structural form of a given theory.

A curious characteristic of much of the SR literature is that it is devoid of solid examples. Many papers rely on highly abstracted analogies without using concrete instances from science, but Worrall is careful to note the case of shifting from Fresnel's theories to Maxwell's (James Clerk, not Grover) theories about optics as paradigmatic of his project. (Another notable exception is the debate that transpired between Cao (2003a, b, c, d) and French and Ladyman (2003a, b) plus Saunders (2003) in a 2003 issue of *Synthese*.)

> There was an important element of continuity in the shift from Fresnel to Maxwell—and this was much more than a simple question of carrying over the successful empirical content into the new theory. At the same time it was rather

> less than carrying over of the full theoretical content or full theoretical mechanisms…There was continuity or accumulation in the shift, but the continuity is one of form or structure, not of content (Worrall 117).

In discussing this quotation, Ladyman (1998) says Worrall not only avoids the force of pessimistic meta-induction, but he also does not make the success of science seem miraculous. So here we have Worrall in a nutshell: he avoids the problem of ontological discontinuity that Laudan attacks, but he also keeps a structural continuity thus keeping his realist badge firmly sewn in place. At present we will simply note that Worrall's paper is ambiguous as to whether he is making primarily epistemic claims or primarily metaphysical ones. Do we not know anything except structure because there is nothing else to know (a la Russell) or only because of some other facts that prevent our knowing the unobservable theoretical entities? If it is the first disjunct then Worrall makes a metaphysical claim. The second disjunct can be further disjoined depending on what "fact" we could instantiate it with some facts would lend an epistemological interpretation and other a metaphysical interpretation. For example, most concerns about instrumentation and resolving power, sensitivity, etc. would be epistemic, whereas talk of the inaccessibility of the noumenal realm is at least bathed in metaphysics even if not metaphysical all the way to its core.

Initially Worrall's conception seems very promising as a way of accounting for what happens with the periodic table. Perhaps 'structure' is somewhat of a vague term on many counts (what *specifically* is the structure that has been preserved from Fresnel to Maxwell?) but a) at least we do not have to deal with the hazards of Ramsey sentences and b) the structure of the periodic table that has been preserved across theory change is incredibly clear. The chemical analogies—that neon is like krypton in many ways, sodium like potassium, carbon like silicon and slightly less like nitrogen and very different from uranium, etc.—are preserved by the spatial layout of the table, regardless of what assumptions we have about *why* the table works. Vagueness in this case is not problematic because the structure is just right there in front of the observer, all drawn out in boxes on a funny-shaped grid. The periodic table did not develop in a vacuum and was the result of previous attempts and visually represented classifications. However, the change from those representations (imagine all the elements simply placed on a mono-radial helix like beads on a string) to the particular tabular representations was one of immense structural change. Certainly many other versions of the periodic table have been proposed, but we could say that these disfavored depictions were analogous to failed theories of, say, optics—nice tries, perhaps, but not satisfactory for one reason or another (often because of empirical inadequacy; see my treatment in the final chapter).

Furthermore, consider this quotation from Worrall in light of claims that I have been making about the table.

> On the structural realist view, what Newton really discovered are the relationships between phenomena expressed in the mathematical equations of his theory (122).

Let us just change a few key terms to fit the case at hand:

> *M1*) On the structural realist view, what *Mendeleev* really discovered are the relationships between phenomena expressed in the *graphical representation* of his theory *about periodicity*.

M1 is completely compatible with the arguments I have made in order to show that the real power of the table lies in the systematic arrangement of the elements in order to accentuate their relationships to each other. Additionally, the fact is that from Mendeleev's table up to now we clearly have a general structural continuity. Sure, Mendeleev systematized the "phenomena," but is that what he *really* discovered? I suggest not. Mendeleev never thought of himself working with mere phenomena. No contemporary thought of the elements as mere phenomena. Modern chemists do not even conceive of the elements as being mere collections of phenomena—they are all committed to the actual existence of the elements. The reason for this broad consensus is the way that first and second order properties fed back on each other. Yes, Mendeleev was a very self-consciously philosophical chemist and had a very strong instrumentalist streak to his thought (see Kultgen 1958) but his instrumentalism seems restricted to models of atoms and compounds, not to atoms and compounds themselves. Furthermore, returning to Hacking's "they are real because we can spray them" argument, the enormous evidence we have in favor of the existence of the elements lends the following reformulation of *M1*.

> *M2*) On the structural realist view, what Mendeleev really discovered are the relationships between ***actual elements*** expressed in the graphical representation of his theory about periodicity.

Even though I fully grant that the instability of the theories over, under, and around the periodic table, *M2* just is not equivalent to Worrall's account of SR. The periodic table makes too many legitimate claims about the nature of the things it purports to describe for us to just sweep them aside in favor of the primacy of structure. As in my discussion of Maxwell, the feedback between first and second order properties is too tight to dismiss the first order properties and say that all Mendeleev was doing was discovering relationships. That was the primary triumph of the periodic table, but of only slightly less importance is the how the table informed the discovery of first-order properties.

Ladyman

In response to Worrall's epistemic/metaphysical ambiguity Ladyman (2001) is abundantly clear where he stands. He insists that SR should be formulated as a metaphysical position. He argues that the Russell/Maxwell position does not avoid the problem of ontological discontinuity for reasons similar to those that I have already outlined under the Maxwell heading above. Rather, he claims that

> To be an alternative to both traditional realism and instrumentalism, structural realism must incorporate epistemic commitment to more than the empirical content of a scientific theory, namely to the 'structure' of the theory, while stopping short of realists' commitment to the full ontology postulated by the theory (1998, 415).

Theories are underdetermined by the evidence for them (418) so Ladyman leaves us with the suggestion that rival theories are alternative representations of the same mathematical structures (421).

> Representations are extraneous to physical states but they allow for our empirical knowledge of them. Objects are picked out by individuating invariants with respect to the transformations relevant to the context. Thus on this view, elementary particles are just sets of quantities that are invariant under the symmetry groups of particle physics (421).

The point that Ladyman is trying to make here is an attempt to avoid the problem that plagues Maxwell (1970) and that Psillos (2001) points out, namely, that preference for one theory over another when both are empirically adequate is indicated by how well both fit within an overall unified framework. The symmetry groups of particle physics provide the framework that determines the invariants and thus delimits just what theories of elementary particles are acceptable.

Does this formulation of SR work to solve the problem of realism and the periodic table? Again, it is lacking. We might say that the periodic table provides the unified framework within which theories about elements count as acceptable, even if multiple theories of elements themselves are merely empirically adequate. This does not seem to work for the simple reason that it does not solve the problem of underdetermination. Even *within* one of these frameworks there are still likely multiple theories that save the phenomena and fit the relative context, yet which are not logically equivalent. For example, consider the fact that we understand that the elements of the table have their particular relationships because of periodic repetitions of outer shell configurations of electrons. Yet, the table did not demand the truth of this particular explanation.

A further objection is the intuitive "no miracles" argument. If we just start listing the successes of the periodic table then Ladyman's metaphysics collapses.

1) The table represents multiple facts about the phenomena for every known element.
2) Over its history the table accommodated novel and unanticipated facts with very few fundamental alterations.
3) The table was used to predict the existence and properties of numerous elements.

The very strong intuition is that the information about relationships among atoms and the first order properties of the atoms are just too intertwined. The table has successfully cataloged a large number of phenomena, including especially the nature of elements—for us to reasonably say that any other possible system comes close to challenging the current one. The pull here is that most chemists think that the table has mapped the actual structure of the world in some realist sense (including accommodating many first-order properties without reducing them down). Alternative representations of the periodic law are all fine and good but it does not seem possible that a representation not based upon the periodicity is even reasonable. If the periodic table is not "true" then a miracle has occurred.

3. A modification of Structural Realism: content rich.

Psillos' conclusion in (2001) is very brief, but it captures the direction that I would like to take SR in the context of the periodic table. I will quote it in its entirety.

> Let me close with a positive note. *One way to read SR is to take a modest epistemic thesis that emerges from looking into the history of scientific growth*. There is no heavy metaphysical machinery behind it, nor any absolute claims about what can or cannot be known. It is just a sober report of the fact that there has been a lot of structural continuity in theory-change: we have learned a lot about the theoretical and empirical laws, although our views about what entities in the world are related thus have seen some major discontinuities. In a certain sense, this is the insight behind Worrall's motivation for SR. All this can

> reasonably be accepted without abandoning realism. What isn't acceptable is any form of strong thesis that draws a principled division between the (knowable) structure of the world and some forever elusive (or worse, non-existent) *X* (S23, *emphasis mine*).

The construction that follows is not found in any structural realist texts with which I am acquainted; indeed most or all structural realists would reject it outright. I will follow Psillos' modest reading of SR, even though I doubt that any proponent of SR actually holds to the position that he puts forth (though perhaps Poincaré does). Most structural realists do make those absolute claims about knowledge and do grind the heavy metaphysical machinery. Fine, but what I am after is a realism that will account for the periodic table, and I believe that the *attitude*[12] of structural realism is a useful and appropriate starting point.

Let me take this "modest epistemic thesis" from Psillos and articulate a specific version that might explain the problems associated with fitting the periodic table to some variety of realism. Most realisms are too committed to the idea that theoretical statements play the central role in science to account for the way that the table has survived serious changes in theory. An alternative to these found in ER fails to account for the relationships between elements that are illuminated by organizing the elements in periodic fashion. We could now say that one of the flaws of ER is that it is too focused on first-order properties, while SR is too focused on second-order structure. Although I have rejected three versions of SR for one reason or another, if we were to be structural realists as a first epistemic step we might be able to solve the puzzle that I have raised.

[12] One is very tempted to be cheeky and say that the structure of structural realism is worth preserving, but not all of its content.

Therefore I propose a form of scientific realism that gives structure epistemic *priority* but not the epistemic or metaphysical *exclusivity* preached by SR. In other words, we might just take the position from looking at the history of science that what frequently happens is that one theory is replaced by another of similar structure, but we could stop short of making the metaphysical implication that only structure exists or the epistemic claim that only structure is knowable. A general problem with SR is the "structure and only structure" results that we are left with. Perhaps this is an artifact of the largely physics-centric approach of contemporary philosophy of science. For example, within biology we can be pretty certain that 'blood circulates' is true. Certainly this theory has a structure that is isomorphic with the actual world, but that structure has clearly been "filled in" with the appropriate constants. We have reached the end of inquiry on the debate as to whether or not blood does or does not circulate. However, old notions within ancient and medieval science held to the circulation of various fluids within the body. The structure of those theories is clearly similar with the final, unquestioned, story about blood circulation, but they were just filled in incorrectly.

So we should look at the points that structural realists have made about the history of science. As a result it would be quite reasonable to be realists about structure and to hold onto the structure of theories a little more tightly than the content of them. This places several types of theories firmly in the class of analogies. As Sellars (1967) puts it:

> Is the analogy between molecules and billiard balls a matter of particular molecules and billiard balls sharing *identical* attributes, or is it, at least in part, a matter of their having a *similar* attributes. As I see it, it is the concept of similar

> or analogous attributes which is the key to the understanding of theoretical explanation (346).

From here we can see the role of first and second order properties. Though billiard balls and atoms may have no first-order properties in common (perhaps they do but we must be agnostic), they certainly share the second order property of being able to move about in space (346-7).

However, once you have the right structure I see no restriction that says the structure can never be colored in. The periodic table is in a sense a case of special analogies because the analogies in question are internal to the table; they obtain between different elements themselves, not elements and some helpful mental picture. Therefore, we can say that if the table works at all, it is because the first-order properties of atomic mass, ionic size, melting point, etc. are themselves similar from element to element.

Second order properties also have special role to play in the periodic table. I have already stated that the table constrains what counts as a good scientific theory of chemical elements. In other words the table maps the structure of the world, but we still have work to do to fill in the appropriate theories about that structure. In some cases this has already been accomplished. For example, periodicity was originally seen to be the function of atomic weight of an atom (later discovered to be the number of neutrons and protons), but a few anomalous elements (e.g. Tellurium) were out of sequence. The justification for switching those elements to their “appropriate” places based on chemical properties was left until after discovering that periodicity is really a function of atomic number (number of protons, only).

What the history of science shows us is that frequently science does get the structure of nature approximately right—this is the first phase in the push towards realism. Determining this "correctness" for the philosopher or scientist could be very difficult. However, at this point we ought to turn to the historian as ask what structures to theories and practice have been preserved across changes in science, being very careful that constructions of "similarity" are not careless and invented. It would be very easy to build a Whig history of some scientific change and construct similarities where none actually existed in the history of the practice. For the periodic table it must be the case that the structure is right, that phosphorus and hydrogen and tellurium exist even if we are not exactly sure what they are *essentially*. My contention is simply this: contra the structural realists, we can have certainty that in some of the episodes of science (e.g. 'blood circulates', 'DNA has a helical form', 'Chlorine normally forms one-negative anions', etc.) we have properly instantiated the structures of our theories and theory-like structures. The case of the periodic table pushes that point very strongly because of the way the structure was developed starting from content and then was used to reshape some of that content and suggest the existence of more content.

Perhaps now I have suggested a moderate solution to the problem of what form of realism would support the history of the periodic table. I confess that I have barely given an outline of this alternative form of realism and that much work needs to be done still. An epistemic hurdle still waits: how do we know when we have properly filled in the received structure? At what point can we say that the content is correct and the theory adequately complete? At this time I cannot answer fully these questions, especially with respect to theories that describe unobservables. One suggestion, though, would be to draw examples from sciences other than physics and to draw from structures other that those proposed only by mathematical or discursive theories. The discussion of pluralism in my concluding chapter also might make these questions slightly less pressing. Insofar as we must wait for the improved tools and practices of scientific observation we may be committed to some limited form of pluralism. If that is the case, then (on pragmatist grounds), there are likely several ways of properly filling in the structure, or even several appropriate structures on which to spend our efforts.

Chapter 3
Pluralism and the Periodic Table

Introduction

By making an appeal to my own form of structural realism my argument must satisfy not only the knowability condition and the mind-independent condition, but it must also fit at least one further constraint. My argument assumes a further qualification of the nature of the mind-independent world, namely, that the world has structure that can be discovered. For a realist, this assumption should be relatively trivial, perhaps even an immediate consequence of any realist discussions of the periodic table.

However, Philip Kitcher of *Science, Truth and Democracy* (2001) has advanced a metaphysical position that might directly conflict with the metaphysical assumptions that I have been making. Kitcher provides a brief exposition of his position can be nicely encapsulated in a thought experiment. He asks us to imagine a block of marble:

> How many things are there here? One large block of marble, of course, but the question is notoriously ill posed. For there are many different lumps of marble inside the big block, many potential statues waiting to be released….If Bernini (say) chips away at the block and produces his *David* then he transforms the block into new objects…or, from another perspective, he changes the environment of a particular *David*-shaped lump that was already there (44).

As a metaphor for the world, he continues that

> Independently of our conceptions, those objects, those chunks of marble exist. We draw (or chisel) the lines, but we don't bring the chunks into being (45).

This position is in sharp contrast with Plato's metaphor of the world as an animal and the goal of science properly executed is to carve Nature at its joints. I argue that there are two readings that we might take of Kitcher—one is philosophically indefensible, but the other is well supported by various periodic tables. In the context of scientific realism, these are two possible readings are—either he says that the world has no structure at all[13] (S_0), or he says that it has some structure but that there just is no necessary and "right" way to divide and group the parts (S_1). Seeing how either of these positions would evaluate the claim of carving Nature at its joints would help illuminate the differences. S_0 denies Plato's metaphor explicitly—Nature is not like an animal with joints to carve. S_1 does not necessarily deny that Nature is like an animal. Rather, S_1 would profess either an ontological skepticism ("Are there really joints?") an epistemological skepticism ("Can we be sure that we know the joints?") or a skepticism about practice ("Isn't the choice to carve at joints *arbitrary*?") or some combination of these skepticisms—some or all of which may be reasonable when dealing with the world of unobservable entities and relationships and similarities among them.

Therefore, let me suggest two metaphors instead of Kitcher's one. S_0 conceives of the world as a bar of soap[14]; nature is largely homogenous and can be cut however we so choose. S_1 conceives of the world as a block of granite. It is not homogenous (on any account that divides the world into classes more fine grained than, say, "rock" versus "non-rock"). We might decide to pick out the feldspar if we wish to use it, but there is also no reason that we must, nor is there anything that prevents us from classifying at another scale. Instead of classifying the granite as 'quartz', 'mica', and 'feldspar' we may just decide to classify the rocks as 'your chunks' and 'my chunks' and 'her chunks',

[13] It is not likely that this is a proper read on Kitcher's actual stance, even in *Science, Truth, and Democracy*. However, the way he uses his metaphors leaves a reader open to considering this as a reasonable consequence of Kitcher's ideas on this reading.
[14] Contrary to how Kitcher uses the block of marble, marble is not in fact so homogenous. It does contain easily definable chunks that are bounded by veins and cracks, etc.

or perhaps we could crush the granite for driveway gravel and simply divide it by size or sort the pieces by color.

Granted, the granite metaphors are a bit silly, but I belabor them because Kitcher makes too much of his block of marble, and would be better served if he indicate that only some statues could be carved out of a given block. As regards realism, S_0 is both incompatible and fairly easily refutable. Certainly the world is full of things that are differentiated enough that we may classify them is many ways, even if those ways are often inconsistent with each other. Some of our distinctions may be arbitrary to varying degrees, even in science (take for example the species concept). But surely not all of the distinctions are made with carefree abandon. S_1 is the more likely reading of Kitcher, and it allows that the world is not uniform, though we may argue over how to lump and split the parts. The reason that this distinction is so critical is that the metaphysics behind S_0 pose a legitimate threat to the assumptions that color my previous discussion of the periodic table. S_1 will seem compatible with the multitude of types of tables. In this chapter, I show how different versions of the periodic table provide warrant for S_1 but not for S_0.

The reason that we must entertain S_1 at this point is that in my account of periodicity I have cheated somewhat in speaking of "the" periodic table. Really, there exist a massive number of different forms the table could take. There are long tables, short tables, medium tables, spiral and helical tables, linear tables, three-dimensional tables, and so on. In fact, Mazurs excellent compilation (1969) suggests that the 700 periodic tables published in the first hundred years since Mendeleev fall into 146 groups of types and sub types. This would not pose difficulties, except that no one table is the canonical type. No one of these tables can universally satisfy the goals of every experiment in chemistry or capture every relationship between the atoms. Indeed, most chemistry teachers believe that the construction of such a table is impossible (Bensaude-Vincent 2001, 153). As we will see below, a given table may best represent some relationships of the atoms, but as regards other relationships perhaps we would best appeal to other tables.

Furthermore, one can make the argument that the most common form of the periodic table was selected in large part due to the demands of textbook manufacturers. In fact, some tables just do not lend themselves to reproduction in textbooks (e.g. 3-D tables, tables with all the elements in one linear sequence, etc.). If taken too strongly, this provides *prima facie* evidence for the S_0 position by implying that the successful tables are successful purely because they are convenient, yet I build a stronger argument in this chapter by examining several different tables, both successful and unsuccessful.

If the explanation of a given table's success is not to be based purely in convenience, then we must ask whether or not the periodic law is consistently represented by a variegated collection of successful tables. More specifically, is the general structure preserved from table to table? The answer to this question will depend on how broadly we are to construe 'structure.' First, the term must not be conceived of as referring primarily to the visual structure of the table (what does it look like?) but rather as referring to the ontic features of the table. These ontic features will often have visual consequences (e.g., see the problem of where to put argon as discussed in chapter 1) but visually distinct tables can have nearly identical references in the world. For example, nearly any tabular layout of the periodic law can be approximated very closely by a helix with radii of varying lengths (*figure 3*, for example, represents almost the same features of periodicity as does *figure 2*, with the added benefit of emphasizing the continuity of the series of elements).

The context in which we pose these questions matters, however. For a realist such as myself, behind the questions is the assumption that some tables really are better than others. There are two measures of what makes a good table. One measure is the simple, straightforward standard of truth: do the relations represented by a table correspond to the world? Some of the tables that follow simply do not, and thus are worse tables. Alternatively, a researcher can have a set of experiments to work on, and some tables will not suit his projects even though they are perfectly adequate for other projects. Consider again the comparison of a periodic table with a map. In this latter measure of better, a map of the Metro line in Washington, D.C. would be better for me than a topographic map of the city; nevertheless I do not claim that they topographic map is universally bad. Kitcher allows that some representations of nature will be better than others, but on an S_0 reading, this is only within the context of the second standard of better, based on the fact that creatures-like-us are the ones performing the investigations. We come to the projects with certain goals and aims already in mind and this constraint—I use this word because just being human or even a particular type of human should not necessarily be called a "bias"—at the very least colors how we execute our scientific inquiry. The breadth and nature of this idea will be examined below.

The objectives of this chapter will be to use the plurality of graphic representations of the periodic law to show and to eliminate the ambiguity in Kitcher's position. In order to give Kitcher a more charitable reading the next section dispenses with S_0 as being philosophically unrespectable. Throughout the critique I will look again at some of the historical peculiarities of chemical periodicity and I will address the conceptual difficulties posed by the existence numerous tables, none of which is one hundred-percent adequate for every goal of chemistry.

Do Natural Divisions Exist?

Considered from an artist's perspective, the world is neither as amorphous nor as fine-grained as a block of marble (as Kitcher describes it). As the scientist carves the world up he is bound to discover many a nasty lump that will not easily yield to his hammer and chisel. Leaving open the question of *how* to classify the things of the world, it is intuitively obvious that the world has things that we sort into classes. Furthermore, once we start sorting things, we may run into classes that are nearly indivisible, groups that can only be divided with great care. Consider the extension of the term 'having atomic number 8.' The term picks out a large number of items, all and only all atoms of oxygen. A quick survey of the various periodic tables reveals the not so startling fact that every table has carved this particular group out. Now it is possible to carve up the world in such a way that the members of the class of all items 'having atomic number 8' are parceled into various different groups. The question is whether or not any of these groups are relevantly meaningful. By expanding the term we can come up with an infinite number of classes, such as 'items having the atomic number 8 and located in Virginia,' 'items having the atomic number 8 and located in Utah,' 'items having the atomic number 8 and part of a glucose molecule,' etc. Knowing about oxygen in Virginia and Utah and in glucose molecules may be useful to some researcher or for some project, but it is not the case that the glucose molecules have a *special* kind of oxygen or that oxygen atoms in Virginia have any different properties than oxygen atoms in Utah, except for the properties of location. While it might be the case that a given oxygen atom acquires new properties when in a given molecule of sugar (say, it might have a unique oxidation number), if you take that atom out of the compound it becomes just like all oxygen atoms. Whatever properties that oxygen atom had are either 1) actually properties of the "system" of the sugar molecule, i.e., emergent properties of several interacting parts, or 2) they are latent properties that all oxygen atoms have that will manifest when bonded in a similar sugar molecule. Extending the idea leads us to the conclusion that even if the properties of items having atomic number 8 are context-dependent, the sum total of those properties within in each context are the real features of oxygen. If I move the entire group of oxygen atoms in Virginia to Utah and find that when in the new location those atoms behave the same way in Utah as in they did in Virginia then I may reasonably infer that the location does not carve off a real group.

Therefore, in a minimal sense we might conclude that some properties have a basis in the real world independent of human construction, whereas others are only properties of a thing by fiat. In the marble metaphor, however, the "lumps" in the block are all properties by fiat. This is why Kitcher's assertion "We draw (or chisel) the lines, but we don't bring the chunks into being" (45) is ambiguous in all the wrong ways. Chunks as material stuff really do have existence independent of any human consideration, investigation, or construction; chunks as specific lumps (even on a pluralist account of the divisions of the lumps) do not have this independence from human meddling. Until Bernini conceives of the particular chunk that is *David* that chunk is just an undifferentiated part of the material block. Unfortunately for the S_0

reading of Kitcher, we are justified in holding that the world has “chunks” independent of human construction. The different functional and causal roles played by various things (e.g., DNA, water, organisms, ecosystems, ions, etc.) indicate that divisions among things exist and not *merely* because we humans declared the proper divisions.

<u>The World Has Structure, but How Much?</u>

However, in denying that the world is homogeneous (and that all divisions are human-dependent) we do not address all of Kitcher’s arguments. Even if the world is composed of some semi-indivisible groups we must still face the fact that classification happens at several levels—science can hardly be satisfied with a taxonomy that selects one group of things but gives it no relation to any other group. ‘Things with atomic number 8’ does us little good unless we can compare it to ‘things of atomic number 7’ and the like. This higher relation is also a taxonomy, but it is here that we meet with the full force of Kitcher’s arguments. The ‘reality’ of these relations is much more tenuous, especially in the many cases where we appear to have equal justification (or lack of justification) for putting one group of things into two incompatible groups. “Are viruses living?” for example.

Lacking critical analysis, a strong human intuition is that we frequently have sorted the world into the correct parcels. Some are even adamant about the divisions, and Kitcher claims that

> Those in the grip of Plato’s metaphor will think we ought to go further. There is a *right* way to divide the world into objects and it is encapsulated in the divisions found in natural language (46).

This statement is a strawman. Even within the field of biological taxonomy, philosophers and scientists recognize a fundamental distinction between "ordinary language" and the classifications of the biologists (Dupre 1993, 18ff). Surely natural language is not the shelter to which a scientist turns when he wishes to determine if there is a right way or a wrong way to divide the world. An amateur naturalist may take recourse there, but in large part the job the studied taxonomist accepts involves either confirming or denying the divisions set by everyday speech; the taxonomist has to take 'jade' and determine what the kinds are. The natural language divisions may help or hinder the task, but they are by no means the final word.

However, even though taxonomists do not take natural language divisions as authoritative, it is not clear that the process of rigorous scientific investigation does us any better *if* the goal is to find real human-independent divisions. "Our ways of dividing up the world into things and kinds of things," claims Kitcher, "depend on our capacities and interests" (59). Certainly this statement is true, but upon reflection it is probably vacuously true. Our divisions do depend upon our capacities—Aristotle could not have properly included 'electrons,' 'protons,' and 'neutrons' in his system for the simple reason that he had no way of detecting them. He likely also had committed the same error of glaringly overlooking bacteria because nobody had been kind enough to invent the microscope for him. However, the worth of second part of the claim—that our classifications are a function of our interests—is of unclear value. Of course it is true that our interests affect how we lump and split the world. Things and relationships that we do not care about we do not even bother to name, thus I cannot even speak of them now. Thus the question is really one of *degree*. "How much do human interests affect our divisions?" is the question with which we are left. Kitcher wants to maintain some kind of realism, and in order to do that he cannot concede to the social constructivists that our divisions are *wholly* determined by our interests. This point is yet a further reason to reject the S_0 reading of Kitcher—on that account everything must be socially constructed. Therefore, if Kitcher wishes to remain a realist, he must say that our divisions are based upon our capacities, interests, *and* the role of the external world. Given that the world influences our capabilities and sets the parameters for the possible things to find interest in, Kitcher's observation is far from unimportant; however, an absolutely necessary variable is the role that the world plays on us as we go about dividing it up.

Granted, I understand that Kitcher is making his argument from the perspective of a philosopher of biology. The types of questions that philosophers of biology face include a great deal of metaphysical instability. 'Species,' if they exist, are plastic and fluid things. They may not capture essential differences between two groups of organisms. If something is in flux you do not know what the relevant essential features, if any, might be. Therefore he says

> It would be absurd pedantry to insist that a single way of classifying organisms must take precedence and that one of the taxonomic schemes is "unnatural." For the purposes of the classification are both obvious and well motivated: researchers want to divide up the organisms in ways which help combat human disease. Once again, the partitioning of nature accords with our interests and, in a less obvious way, with our capacities (49).

Certainly it is likely that in some corners of scientific practice we are left with an irreducible pluralism. The term 'gene' has been used to refer to the coding regions of

DNA, the coding regions plus regulatory regions, all of that plus helper proteins and RNA, features of developmental systems, or even just whatever it is that makes the difference between two different forms of the same trait. Likely we must take a pluralistic account of the term ‘gene’ and be careful not to equivocate on the ambiguities. Yet, this pluralism also could be an indication that we have reached a point where the prudent course of action is to abandon a concept if we continue to diverge increasingly further away from unified accounts of the meaning of the concept.

Taking another route of argument, in *Science, Truth and Democracy,* Kitcher gives precious little treatment of the non-biological sciences. Yet, he clearly does argue that his thesis applies to all of science, perhaps even all of human inquiry. If scientists divide the world according to human interests and capabilities, surely non-professionals do so as well. However, there is at least one significant difference between biology and some of the other sciences, one difference important enough to cause us to question whether or not Kitcher’s perspective should be extended to all the sciences, even if he is entirely correct about biology. Highlighting this difference will actually help to show how we can use the periodic tables for the S_1 position.

Regarding the case of chemistry in particular, perhaps at one point in time events of cosmological natures caused the “evolution” of the elements. Regardless of the deep history, those elements have reached a state where they have stopped “evolving,” except for a few new elements that are laboratory-created. We still have many discoveries left to make about the nature of atoms. We even have atoms left to make (say, those above atomic number 111). We do not expect these elements to somehow acquire new characteristics, however. Nuclear reactions may be a relatively recent discovery for humans, but the capacity to undergo nuclear reactions is a property that elements themselves hold that can be expressed in the proper context. The kind of thing that we don't expect would be something like finding a substance in which, one day, the atoms each acquired a plural nucleus.

The physical nature of atoms differs from the physical nature of biological systems as Kitcher conceives of them in a metaphysically fundamental way. The fundamental nature of atoms does not change (perhaps it cannot, but that question is likely beyond our capacity to answer at present). Species, for example, can change across time. I do not mean that scientists use different terms for the groups, even though they do, but rather that once we settle on a definition of species, the fundamental features of certain animal groups may shift such that the species changes into a stable class with different characteristics. Even though any element can undergo changes, the fundamental properties of being that element do not. It is as likely that uranium will be able to undergo nuclear reactions (in the right context) as it is that the sun will rise next morning. It is bizarre to think of uranium losing such ability; it is not bizarre to think of finches gradually or even suddenly preferring a different kind of seed to eat. This fact sheds some light on the differences between the plurality of periodic tables and the plurality of biological taxonomic schemes. Examining some actual periodic tables[15] should help us to identify the exact metaphysical differences.

We can say that to some degree or other some classification systems must be rejected because of inconvenience, thus giving a nod in the direction of the "interests and

[15] These tables do not all have the same prestige; they are not a sequence of tables that represent the development of thought. Some of the tables used here were rejected almost as soon as they were published, whereas others have long pedigrees. For example, *figure 12*, the most common form of the table in use today, was first developed by Mendeleev (see note 3).

capacities" criteria. *Figure 7* is not an accurate classification of the elements according to periodic relationships; *figure 8* is accurate, but both are just too confusing to be of any utility or pedagogic value.

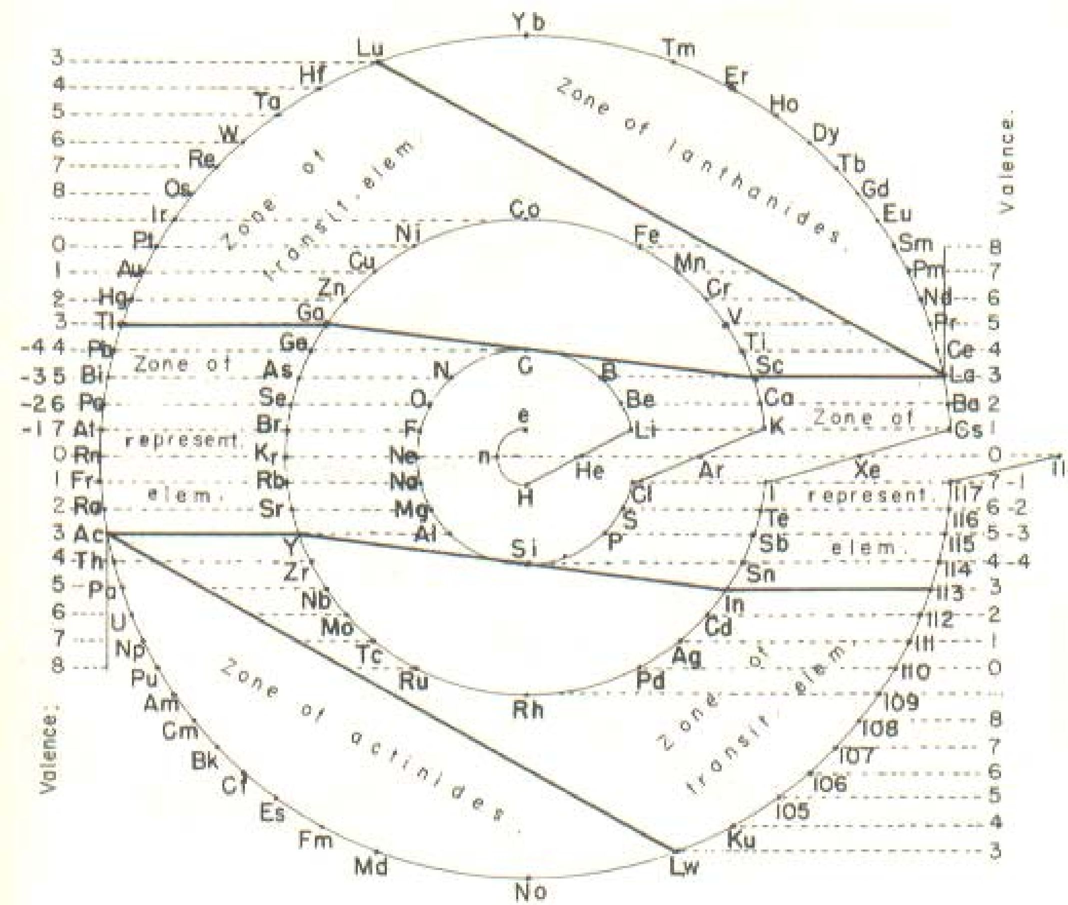


FIG. 79. Rydberg 1913. Type IIIB2-1.

Figure 7, Rydberg 1913; Reprinted in Mazurs 1974, 73.

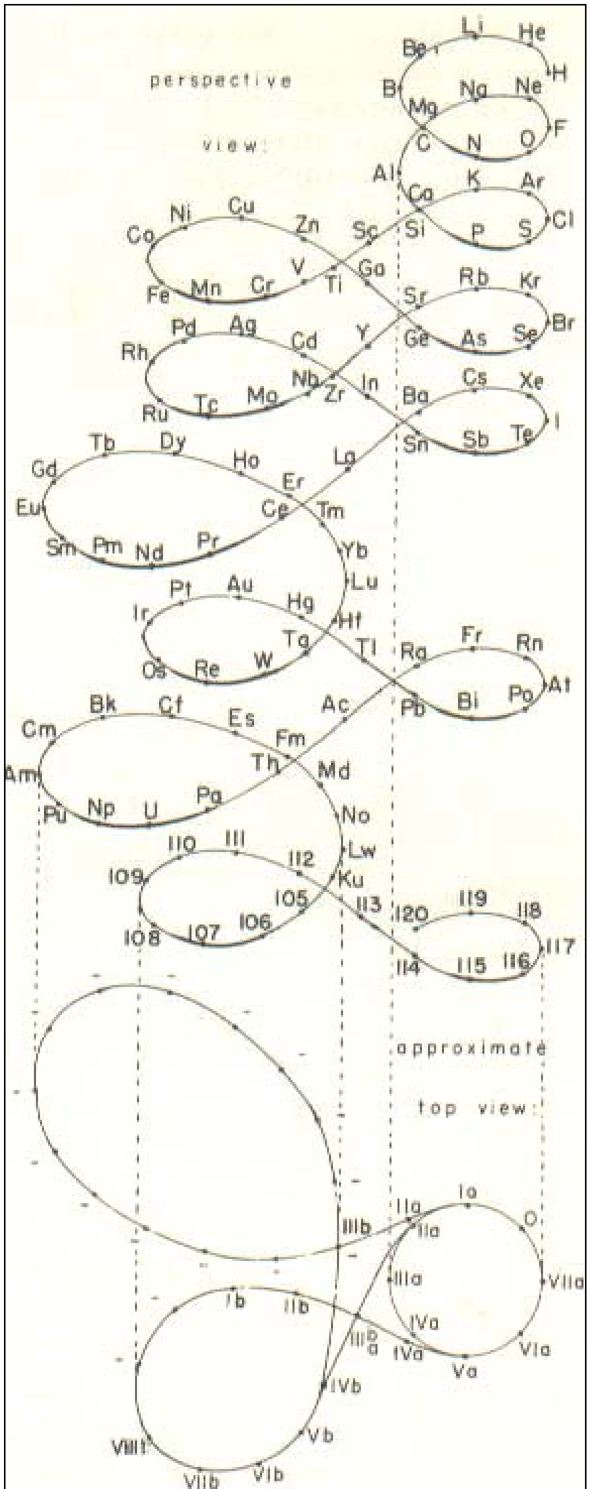


FIG. 86. Schirmeisen 1900. Type IIIA3-2.

Figure 8, Schirmeisen 1900; Reprinted in Mazurs 1974, 79.

These two periodic systems are just too visually complicated. *Figure 7* requires a "tennis-match" like visual approach to see the chemical analogies captured by a simple tabular layout (just try tracing the Li, Na, K, Rb, Cs, Fr series to get the effect). It has the sole advantage of clearing showing the continuous sequence of elements, but lacks much in the way of mapping the chemical analogies—you cannot tell from this table what the analogies are. You have to know that beforehand in order to pick out the pattern. The supreme disadvantage faced by *figure 8* is the constriction of two dimensions. This modified leminscate form would benefit greatly by being represented in three dimensions. Perhaps a scientist could learn to use this the same way that a concert pianist can decipher visually complicated musical scores, but insofar as visual simplicity is to be preferred, other versions of the table contain the same epistemic content (e.g., showing that the halogens share commonalities) that are much easier to read. As it is, the spatial relations on the page hinder easy identification of the relationships that atoms have, even though the classification is empirically consistent with other good tables. In the end both of these representations as they stand have been rejected just as a matter of convenience.

In essence, *figure 8* poses too high a cognitive load on researchers. Perhaps a computer could make use of this table, but certainly not high school chemistry students. Similarly, I have not argued that the relations represented by periodic tables *cannot be* replaced by a set of propositions; I only imply that they *have not been* so reduced. A valuable feature of representing the periodic analogies with spatial organization is that one can quickly and easily perceive the relations with some little training. The spatial relations also had the added advantage for Mendeleev in that he could pick out the holes in his tables. A human would not have been able to make predictions if the same material was represented by just propositions; it is an open question as to whether or not a supercomputer could have been programmed to allow for the making of such predictions.

Cognitive load, however, is not the reason that explains why several other varieties of periodic representation have not been adopted for general use. *Figure 9* is a leminscate that is visually a very straightforward figure eight that repeats, but the problem with this representation is that the divisions that it makes do not correspond with any conceivable set of phenomena—it does not correctly map any significant number of chemical relationships. Using this table to teach about chemistry would be like giving somebody in the Washington Metro a map of the London Underground. Occasionally some things would be right—there are trains that have stops and the curved and straight stretches may sometimes overlap—but quickly the individual would be very lost. Visual simplicity is only a virtue when representational accuracy is not sacrificed.

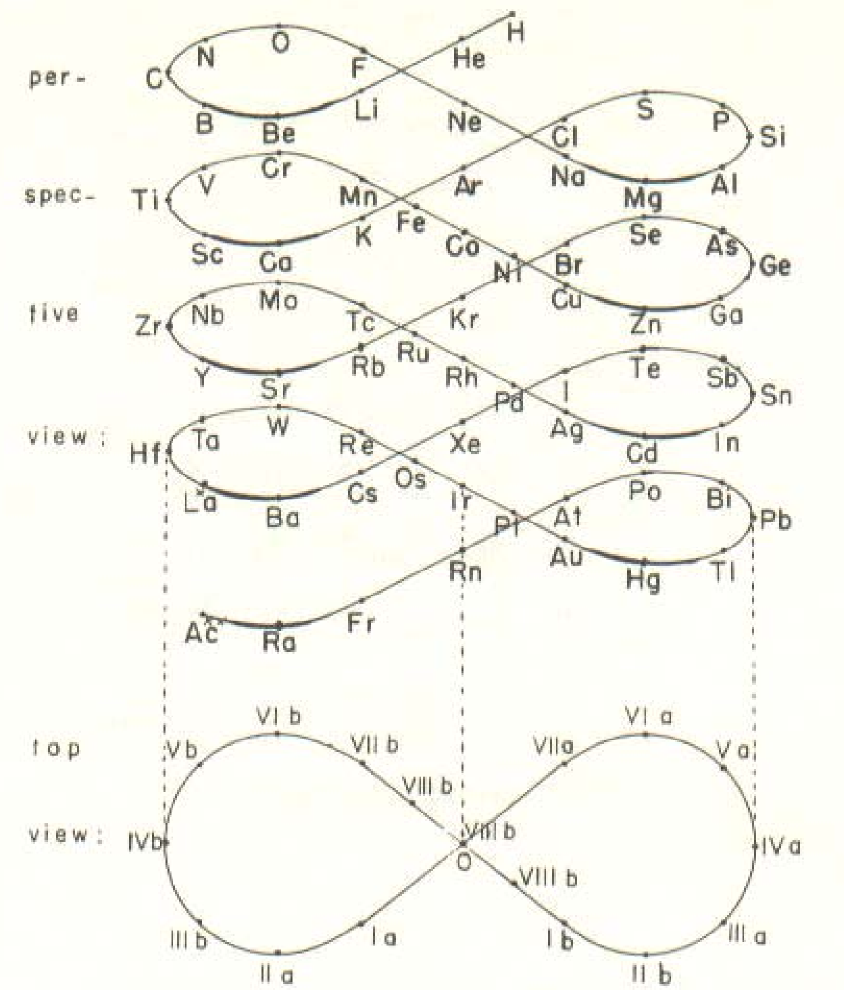


FIG. 52. Crookes 1898. Type IIA1-2.

Figure 9, Crookes 1898; Reprinted in Mazurs 1974, 51.

The first two figure eights do not correspond to the generally well established data about chemical similarities. Fluorine has no similarities with manganese, and yet they are positioned as if they should share some commonalities. The center of the diagram (Begin with "He" and read straight down to "Rn") commits the almost laughable error of intermixing the noble gases with various metals. The reasons to reject such a representation are not matters of capability or interest, but of the way nature itself is. The only reason that we can make such a preference is that nature it in some sense not like a bar of soap. Even granting that we are viewing nature from the eyes of science and thus have a distinct perspective and goals in mind, nature does not permit of being cut just any which way.

Likewise, the short tables (*figure 10*) that were in fairly common use until the discovery of a greater number of transition metals and the lanthanide and actinide series suggest chemical analogies where none really exist. The confusion in Group VIII is evident from this representation, but would not have been at the time it was originally constructed seeing as how the noble gases had not been discovered.[16]

Series \ Groups	I	II	III	IV	V	VI	VII	VIII
1.							H	He
2.	Li	Be	B	C	N	O	F	Ne
3.	Na	Mg	Al	Si	P	S	Cl	Ar
4.	K	Ca	Sc	Ti	V	Cr	Mn	Fe Co Ni
5.	Cu	Zn	Ga	Ge	As	Se	Br	Kr
6.	Rb	Sr	Y	Zr	Nb	Mo	Tc	Ru Rh Pd
7.	Ag	Cd	In	Sn	Sb	Te	I	Xe
8.	Cs	Ba	La^{x}	Hf	Ta	W	Re	Os Ir Pt
9.	Au	Hg	Tl	Pb	Bi	Po	At	Rn
10.	Fr	Ra	Ac^{xx}					

La^{x} = lanthanides; Ac^{xx} = actinides.

FIG. 23. Newlands 1865. Type IC1-1.*

Figure 10 Newlands 1865; Reprinted in Mazurs 1974, 21.

[16] Mazurs (1971) did the fine work of taking the original depictions of the periodic law and updating them with the contemporary knowledge of his day. Thus some of these figures include elements only recently discovered. This gives us an advantage of seeing how mistaken some early depictions were, but the depictions might have been excellent efforts given the data of their day.

The reason that a table is successful at representing periodic relationships depends upon more than just human capacities. Kitcher claims “What counts as an omission or an inaccurate spatial representation depends upon the conventions associated with the kinds of maps, and, in their turn, those conventions are in place because of the needs of potential users” (56). Of course conventions matter—the language convention of the Kitcher’s audience dictate why he wrote in English—and in the sphere of diagrammatic representation they are of supreme importance. However, those needs of potential users are not the primary reason that a particular representation works—some representations map the nature of the world quite well and their success depends primarily on how well the represent the world. Consider *figures 11* and *12*.

Figure 11 is often erroneously attributed to Bohr because he was the one who popularized this formulation of the elements. The advantages that this table has over a standard medium long table (*figure 12*) is that is has no large empty space (look above the transition metals in *figure 12*) which somebody must understand to be unfillable space. This is just one less convention that somebody must know. Continuing with *figure 11*, even though the table is not in continuous columns, the traced lines can indicate what elements have vertical similarity. Thus it has one more convention that must be known. This feature results not only from the fact that users have specific needs, but from an interaction of that need with the way that the world is fundamentally structured. The fact that this table is also aesthetically elegant has little to do with what it represents.

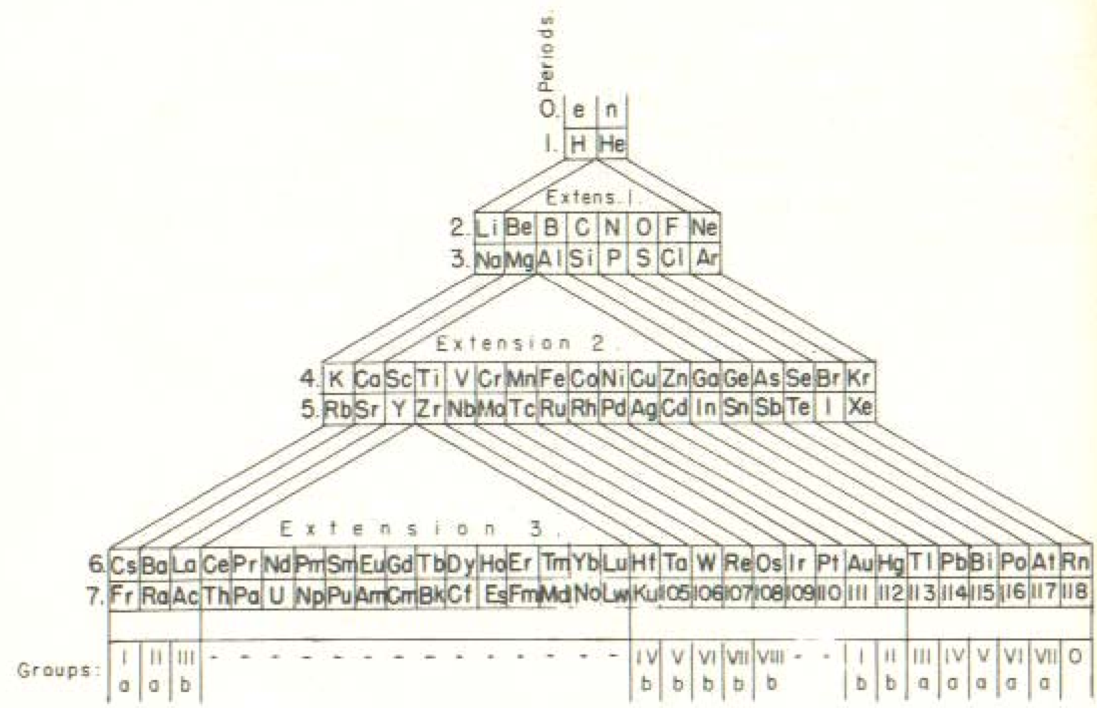
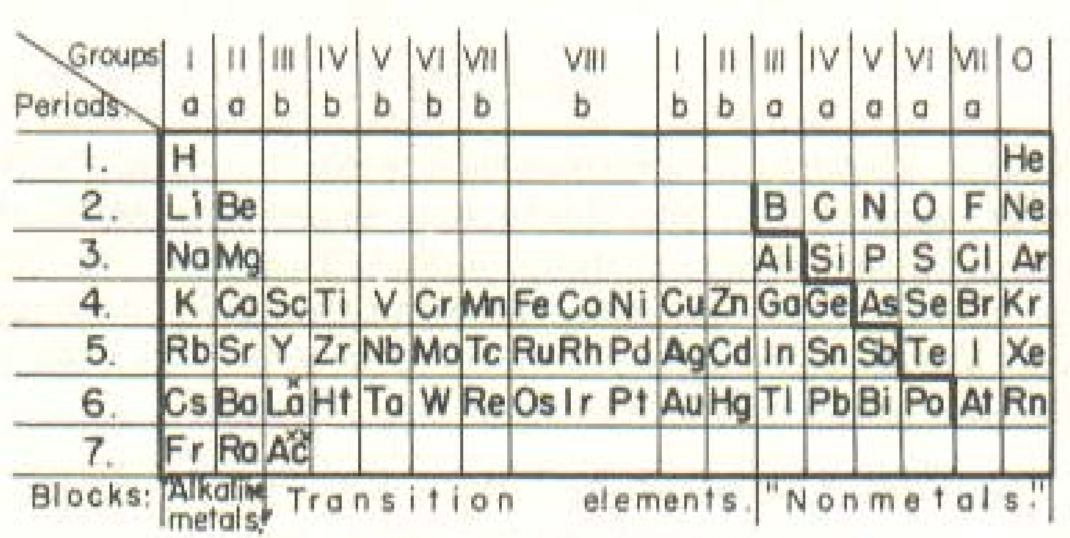


FIG. 92. Bayley 1882. Type IIIC3-3.

Figure 11, Bayley 1882; Reprinted in Mazurs 1974, 84.



Periods \ Groups	I a	II a	III b	IV b	V b	VI b	VII b	VIII b	I b	II b	III a	IV a	V a	VI a	VII a	O
1.	H															He
2.	Li	Be									B	C	N	O	F	Ne
3.	Na	Mg									Al	Si	P	S	Cl	Ar
4.	K	Ca	Sc	Ti	V	Cr	Mn	Fe Co Ni	Cu	Zn	Ga	Ge	As	Se	Br	Kr
5.	Rb	Sr	Y	Zr	Nb	Mo	Tc	Ru Rh Pd	Ag	Cd	In	Sn	Sb	Te	I	Xe
6.	Cs	Ba	La*	Hf	Ta	W	Re	Os Ir Pt	Au	Hg	Tl	Pb	Bi	Po	At	Rn
7.	Fr	Ra	Ac**													
Blocks:	"Alkaline metals"		Transition elements.								"Nonmetals."					

FIG. 67. Mendeleev 1869. Type IIC2-4.

Figure 12, Mendeleev 1869; Reprinted in Mazurs 1974, 63.

Figure 13 is a periodic representation of electron orbital shell states. This is a particularly good table in that it accurately and adequately represents the traits of one feature of the elements without sacrificing many other chemical analogies (notice how the elements still appear in columns even though the rows are disjoined). Basically, this table has a slightly more expanded spatial field which allows for representing very precisely the relationship of one of the properties of the elements. It is like a cut-away anatomical diagram that shows something that is normally “underneath” other representations. Normally, representing the periodic law does follow some kind of uncertainty principle: the more precisely you try to represent one property, the less accurately the other properties will be represented. What is nice about this table is that it defies this principle rather well. Compare it with another table (*figure 14*) that has represented the electron states exactly right, yet offers no visual representation of any analogies among atoms. The only content to this diagram is dashes inside the ring that represent the location of electrons in the ground state for each atom (you can see the shell-shaped spiral of these). Given that, this table probably should not even be considered to represent periodic law.

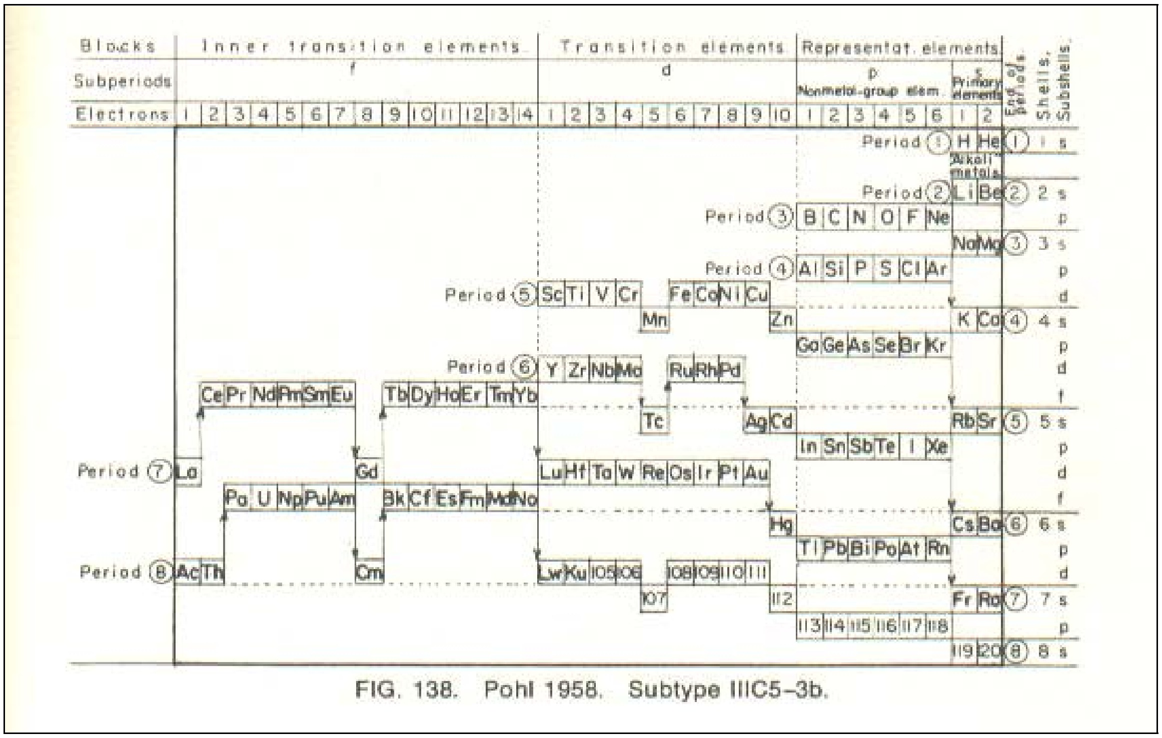


Figure 13, Pohl 1958; Reprinted in Mazurs 1974, 135.

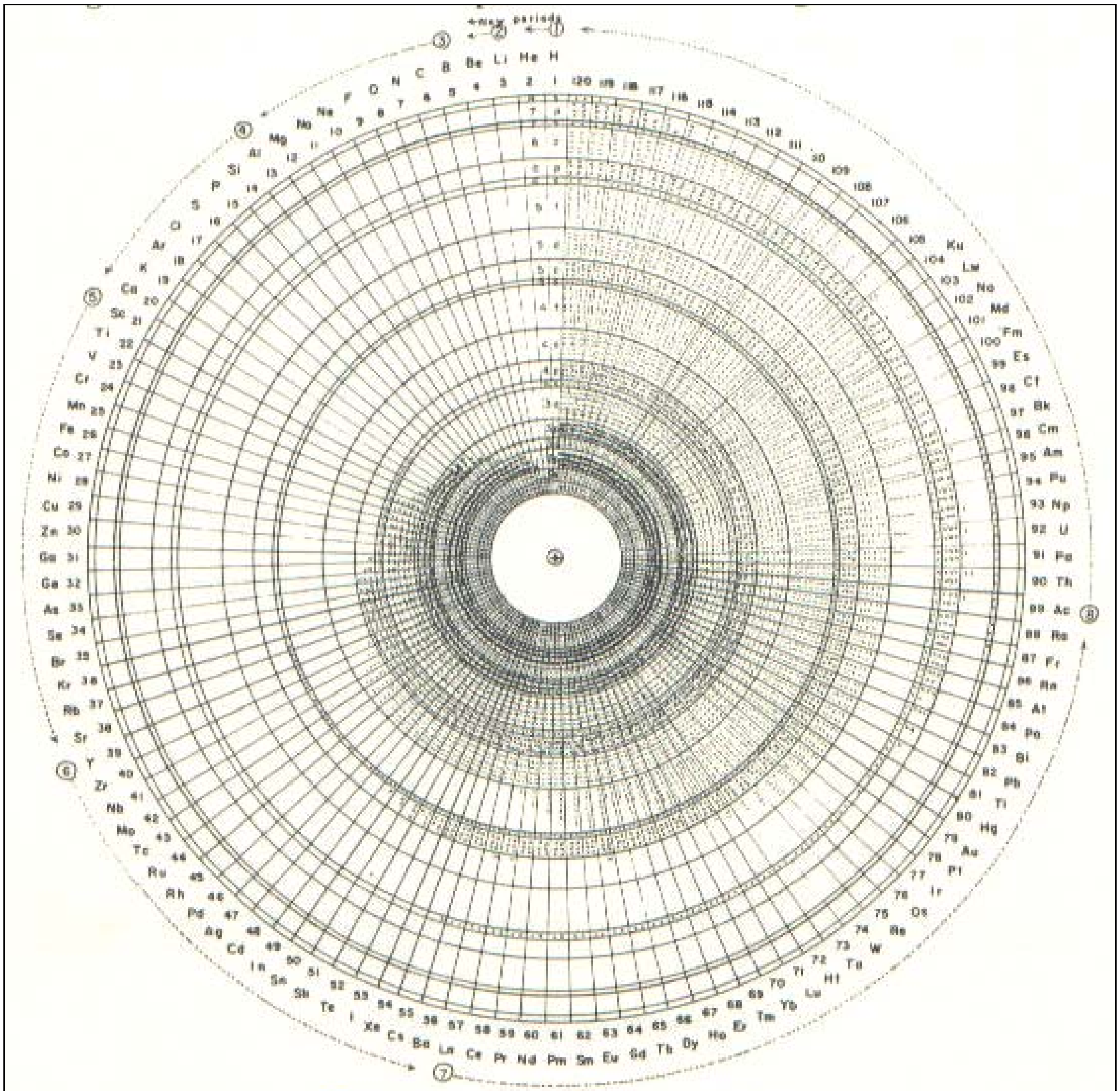


FIG. 76. Stoye 1954. Subtype IIIB1–1b.

Figure 14 (Mazurs 70)

What moral can we draw from these diagrams? Simply the one already stated—the fact that some of them are better (*figures 11*, *12*, and *13*) has to do with facts above and beyond human interests and capacities. While human interests and capacities play an important role (see why *figures 7* and *8* are rejected), it is not ultimately the decisive factor. Case in point, the underlying structure of the world can determine which tables will be successful, even if we are not aware of that structure. However, while the tables in *figures 11*, *12*, and *13* are all good tables and show the periodic law very well, they each show it somewhat differently and they are not wholly compatible. Thus an S_1 reading of Kitcher is very likely appropriate because the exact nature of the world is unclear. The murkiness exists because unobservables can (by definition) only be studied obliquely, and also because we do not have some metaphysical priority measure that tells us which properties must be represented. When faced with the limitations of representational taxonomies like the periodic table we cannot always map every relationship of the atoms, but nothing in nature dictates which ones must be referred to by any system. Those three tables are each accurate insofar as they are intended to be; now we may say that "depending on human needs and interests" we can select the table of interest which to use for our studies.

One final point must be said. Even though these good tables are not entirely compatible, they are very similar, and their similarities are best appreciated when looking at the bad tables I discussed. Explaining this similarity further illuminates my interaction with Kitcher and its consequences for realism. Many of these tables were formulated before the discovery of the fact that atomic number was the reason for periodic repetition of properties. Kitcher makes a relevant comment in passing:

> Notice, however, that the idea that these microstructures play a systematic unifying role depends on the *prior* identification of a class of manifest properties. Perhaps because of our sensory and cognitive capacities, perhaps because of interests that people have developed, either naturally or as a result of the accidents of human history, we focus on certain aspects of the world that intrigue us [emphasis original] (50).

I conclude with this analysis of the quotation from Kitcher in light of the periodic phenomena. Yes, a class of manifest properties was identified prior to appeal to microstructures, but three points are relevant:

1) The manifest properties were identified before the discovery of the microstructures.
2) The systematic unification was not completed by appeal to the microstructures. Unification happened by arranging the elements in tables and it was not until 1913 that a common underlying structure was appreciated (Van Spronsen 293).
3) The arrangement suggested by the microstructure (that the elements in columns would have similar electron cores) closely matched the already-in-use tables.

So what are we to make of the fact that the formulation of the periodic table preceded a proper understanding of the microphysical structure that explains the chemical analogies? If our interests were the sole reason that the table that we developed was so successful, then the match between the table and the later-discovered microstructure would have been miraculous. Therefore, it must be the case that the world played a causal role in constraining what kinds of tables could have been constructed. Different visual conventions and needs allow for the different tables, but the successful tables still carve up the world rather similar fashion, allowing for variances due to emphasis of some differing sets of properties. Perhaps as we develop better ways to observe atoms we will reach a better understanding of their nature such that we develop more stringent criteria for what makes a “better” table.

Conclusion: A Final Word on Maps

The durability of the periodic table suggests strong evidence that the table maps relationships chemical elements correctly. This implies that scientific realism as opposed to some version of instrumentalism provides the proper interpretation of the periodic table. However, insofar as accounts of scientific realism rely solely on purely linguistic propositional content they will be unable to interpret those same features that that I have argued that we have reasons to be realists about. The table does not refer to the things that it represents primarily by explanatory statements. Rather, the spatial layout and dimensions of the table are what indicate the relations in the world.

True, we could conceivably create a list of statements that that refer to the same relationships between the atoms that the table refers to. Furthermore, any periodic table requires some set of auxiliary interpretive propositions. New students of chemistry, especially, need verbal descriptions to become competent users of the table as a tool for research. However, as with maps, the relevant information is contained within the visual diagram, not in descriptions of how to use the diagram. As I suggested in my final chapter, all of the information contained in the table could be translated into a finite set of propositions. We could take the information contained in a map and create a set of propositions; consider how one might give directions to get from one location to another by saying the roads that should be taken and when to turn left or right, etc (this is likely just a subset of all the set of propositions that would be truth-equivalent with the map). However, for the same reason that we would not always replace maps with sets of propositions, we do not replace periodic tables. The set of propositions is just too cumbersome to be useful in scientific research.

I emphasize the likelihood that early research into the periodic law would not have been able to make predictions about the existence of new elements because the holes would not have been apparent. Again, the map analogy is very helpful: take a road map and trace a course to get from one town to another (pick a nice, rural route with several back roads). Describe the same route, but neglect to mention left hand turn entirely. From the description alone, it would not be clear that an omission had been committed—you could only discover this by traveling the route and becoming frustrated, or referring back to a map to notice that the left hand turn was omitted. On the other hand, if a map has holes in it some will be readily apparent (e.g. think of a globe with a giant white spot for Madagascar) but others require an independent investigation. The first class would be like the holes that Mendeleev wanted filled; the second class would be similar to the holes that were not known to exist, i.e., argon and the noble gas column.

From this analogy of the table as a map it is tempting to place the table within a scientific hierarchy well below theories. One might say that because maps provide necessary oversimplification of the region that they are intended to map, thus they are *only* tools for real work of trekking around in the world. One might take too far an implication from my argument about the table as setting parameters for what makes an appropriate theory in chemistry by concluding that the real serviceableness of the is in how it helps us construct good theories. Apart from suffering from a mistaken theory-centric view of science, such a move is mistaken on a more fundamental ground. Many or nearly all discursive theories work in much the same map-like way; they cut out the irrelevant parts of the real world in order to hone in on the particular feature of interest.

This is most often accomplished by creating theories that do not have a direct correspondence to the world, e.g., there is no such thing as an ideal pendulum, nor are there frictionless planes, nor do we normally find cases in which the gravitational force of one body acting upon another is the only force at work. Neither do we find that periodic behavior exactly obeys any one periodic table that has yet been constructed. Yet, in this manner theories are yet more map-like. A map of a subway can be completely and exhaustively accurate for determining rough distance relations via rail between all the stations of the system, and therefore we do not fault the map as being false because it neglects to show topography lines or locations of four-star restaurants within the city. Neither would we say that the map has to be specific for just the purpose of determining the distance relations from station to station. It would not be erroneous of me if I were to take my subway map and mark the location of four-star restaurants that I particularly liked, or to mark that such and such station is close to a really nice bookshop. In this respect, being map-like is not a sufficient reason from saying that periodic table are significantly different from theories.

The talk in terms of maps draws together information from all my chapters. I already gave a discussion in my first chapter of the theory-likeness of maps, applying the analogy to the second chapter further emphasizes the first and second order feedback loop. We make maps of second order relations by accounting for some of the first order relations of the landscape. These second order relations allow the inference of first order similarities. As should be apparent from the discussion above, Kitcher's pluralism is well supported by the theory-map analogy. Many maps can chart the same region, all with different purposes in mind. What is necessary for the success of a given map is that the map corresponds to the world according to the necessary conventions then. We should also note that success should not be determined by the sheer number of users. Certainly only a relatively few people use maps that trace electrical lines and telephone wires, but to the degree that those maps are transparent (i.e., convenient) and correspond to the world they will be successful for the entire class of people interested in electrical lines and telephone wires.

Discussion about maps also highlights an issue that I have not treated, but that I can note for future research. Each map maps the area in question at a given level and the relevant information will be different at each level. What is important for a map of the globe will be very different than what is important for a map of a university. The consequences of this for realism are particularly striking. In my final two chapters I occasionally spoke of "the structure of nature" but that is too hasty. Nature has different structures at different levels, and distinctions that we make at one level may not be wholly consistent with the distinctions that we make at another level. Furthermore, the distinctions need not be just of size and degree of observability. My discussion of the periodic table has stayed at one level, but I support the motivation behind Kitcher's pluralism that claims that we can be working at one level with completely different purposes in mind and thus categorize its features in different ways. Though I have not treated the issue here, my thesis opens the possibility of applying the image-as-theory idea to the understanding of levels within nature and how realism applies at each level.

In fact, Toulmin (1953), Polanyi (1962), and Giere (1999) and many others have all suggested that the map/theory analogy is a fruitful one. Maps (and periodic tables) speak to us in very different way than do discursive theories. While they may have different sets of conventions for interpretation, diagrams do have a language of their own that scientists regularly use and teach to budding researchers. Therefore, we should evaluate the worth of particular periodic tables on similar grounds as we would evaluate theories about ideal pendulums. Most importantly, this includes the ability of a diagram to bear truth, often in a much more organized and succinct way than a set of statements. That is what the periodic table does for us and that is why we should be realists about it.

Works Cited

Bensaude-Vincent, B.: 2001, "Graphic Representations of the Periodic System of the Chemical Elements," in *Tools and Modes of Representation in the Laboratory Sciences*, ed. Ursula Klein. Boston Studies in the Philosophy of Science, **222**. Kluwer Academic Publishers, Dordecht, The Netherlands, 133-162.

Beguryer De Chancourtois, M.A.E.: 1889, "A First Foreshadowing of the Periodic Law." *Nature* 46: 186–188.

Benfey, O.T.: 1992, "Perspective Lecture: Precursors and Cocursors of the Mendeleev Table: The Pythagorean Spirit in Element Classification." *Bulletin of the History of Chemistry* 13: 60–66.

Brock, W.: 1992, *The Norton History of Chemistry*, Norton, New York.

Brooks, N.: 2002, "Developing the Periodic Law: Mendeleev's Work During 1869-1871," *Foundations of Chemistry,* **4**, 127-147.

Burian, R. and Trout, J.: 1995, "Ontological progress in science." *Canadian Journal of Philosophy*, **25**, 177-202.

Cao, T.: 2003a. "Structural realism and the interpretation of quantum field theory." *Synthese*, **136**, 3-24.

---.: 2003b. "Appendix: ontological relativity and fundamentality—is QTF the fundamental theory?" *Synthese*, **136**, 25-30.

---.: 2003c. "Can we dissolve physical entities into mathematical structures?" *Synthese*, **136**, 57-71.

---.: 2003d. "What is ontological synthesis?—A reply to Simon Saunders." *Synthese*, **136**, 107-126.

Cohen, B.: 2004, "The Element of the Table: Visual Discourse and the Preperiodic Representation of Chemical Classification," *Configurations*, **12**, 41-75.

de Milt, C.: 1951, "The Congress at Karlsruhe." *Journal of Chemical Education* 28: 421–424.

Dupré, J.: 1993, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science.* Harvard University Press, Cambridge MA.

Emsley, J.: 1987, "The Development of the Periodic Table of the Chemical Elements." *Interdisciplinary Science Reviews* 12: 23–32.

French, S. and Ladyman, J.: 2003a, “Remodelling structural realism: quantum physics and the metaphysics of structure. *Synthese*, **136**, 31-56.

---.: 2003b, “The dissolution of objects: between Platonism and phenomenalism.” *Synthese*, **136**, 73-77.

Friedrich, B.: 2004, “…Hasn’t It?: A commentary on Eric Scerri’s paper ‘Has Quantum Mechanics Explained the Periodic Table?’ now published under the title ‘Just How Ab Initio is Ab Initio Quantum Chemistry?’,” *Foundations of Chemistry*, **6**, 117-132.

Giere, R.: 1999, *Science without Laws*. University of Chicago Press, Chicago.

Guinta, C.: 2001, “Argon and the Periodic System: The Piece That Would Not Fit,” *Foundations of Chemistry*, **3**, 105-128.

Hacking, I.: 1983, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science,* Cambridge University Press, Cambridge.

Hettema, H. and T.A.F. Kuipers.: 1988 “The Periodic Table – Its Formalization, Status and Relation to Atomic Theory.” *Erkenntnis* 28: 841–860.

Ihde, A.: 1984, *The Development of Modern Chemistry*, Dover Publications, New York.

Kauffman, G.B.: 1969, “American Forerunners of the Periodic Law.” *Journal of Chemical Education* 46: 128–135.

Kaji, M.: 2003, “Mendeleev’s Discovery of the Periodic Law: The Origin and Reception,” *Foundations of Chemistry*, **5**, 189-214.

Kitcher, P.: 2001, *Science, Truth, and Democracy*. Oxford University Press, Oxford.

Kragh, H.: 2001, “The First Subatomic Explanations of the Periodic System,” *Foundations of Chemistry*, **3**, 129-143.

Kultgen, J. 1958, “Philosophic conceptions in Mendeleev’s *Principles of Chemistry*.” *Philosophy of Science*, **25**, 177-183.

Ladyman, J. 1998, “What is structural realism?” *Studies in the History and Philosophy of Science*, **29**, 409-424.

Lakatos, I.: 1977, “Science and Psuedoscience,” *Philosophical Papers*, vol. 1, Cambridge University Press, Cambridge: 1-7.

Laudan, L.: 1981, “A Confutation of Scientific Realism,” *Philosophy of Science* 48: 19-49.

Leicester, H.: 1948, “Factors Which Led Mendeleev to the Periodic Law,” *Chymia*, **1**, 67-74.

Leicester, H., Klickstein, H.: 1965, *A Source Book in Chemistry 1400-1900*, Harvard University Press, Cambridge MA.

Lipton, P.: 1990, “Prediction and Prejudice.” *International Studies in the Philosophy of Science* 4: 51–60.

Maxwell, G.: 1962, “The ontological status of theoretical entities.” *Scientific Explanation, Space, and Time.* Eds. Herbert Feigl and Grover Maxwell. *Minnesota Studies in the Philosophy of Science*, **3**. University of Minnesota Press, Minneapolis, 3-15.

---.: 1970, “Theories, perception, and structural realism.” *The Nature and Function of Scientific Theories: Essays in Contemporary Science and Philosophy*. Ed. Robert Colodny. University of Pittsburgh Series in the Philosophy of Science, **4**. University of Pittsburgh Press, Pittsburgh, 3-34.

Mazurs, E.: 1974, *Graphic Representations of the Periodic System during One Hundred Years*, University of Alabama Press, University, AL.

Mendeleev, D.: 1869, “The relation between the properties and atomic weights of the elements.” in 1965, *A Source Book in Chemistry 1400-1900*, Leicester, H., Klickstein, H., eds. Harvard University Press, Cambridge MA.

Meyer, J. L.: 1870, “The nature of the chemical elements as a function of their atomic weight.” in 1965, *A Source Book in Chemistry 1400-1900*, Leicester, H., Klickstein, H., eds. Harvard University Press, Cambridge MA.

Morganti, M.: 2004, “On the preferability of epistemic structural realism.” *Synthese*, **142**, 81-107.

Perini, L.: 2005, “The Truth in Pictures.” *Philosophy of Science* 72: 262-285.

Pohl, H.: 1958, “Eine neue Darstellung des periodischen Systems in Verbindung mit dem Schalenaufbau der Atomhuelle.” *Universum* (Vienna) 13(6), 176-180.

Polanyi, M.: 1962, *Personal Knowledge: Towards a Post-Critical Philosophy*. Chicago University Press, Chicago.

Psillos, S.: 2001, “Is structural realism possible?” *Philosophy of Science*, **68**, S13-S24.

Rayleigh Lord and Ramsay, W.: 1895, “Argon, A New Constituent of the Atmosphere,” *Philosophical Transactions of the Royal Society of London A* 186, 187-241.

Saunders, S.: 2003. “Structural realism, again.” *Synthese*, **136**, 127-133.

Scerri, E.: 1991. “Electronic Configurations, Quantum Mechanics and Reduction.” *British Journal for the Philosophy of Science* 42: 309–325.

---: 2000a, “The Failure of Reduction and How to Resist the Disunity of Science in Chemical Education.” *Science and Education* 9: 405–425.

---: 2000b, “Naïve Realism, Reduction, and the ‘Intermediate Position’,” *Of Minds and Molecules: New Philosophical Perspectives on Chemistry*, eds. Nalini Bhushan and Stuart Rosenfled, Oxford University Press, Oxford, 51-72.

---: 2001, “The Periodic Table: The Ultimate Paper Tool in Chemistry,” in *Tools and Modes of Representation in the Laboratory Sciences*, ed. Ursula Klein. Boston Studies in the Philosophy of Science, **222**. Kluwer Academic Publishers, Dordecht, The Netherlands, 163-178.

---: 2004, “Just How Ab Initio is Ab Initio Quantum Chemistry?,” *Foundations of Chemistry*, **6**, 93-116.

Scerri, E. and Worrall, J.: 2001, “Prediction and the Periodic Table,” *Studies in History and Philosophy of Science* 32: 407-452.

Sellars, W. “Scientific Realism or Irenic Instrumentalism,” in *Philosophical Perspectives.* Charles C. Thomas, Springfield, IL: 337-369.

Stoye, K: 1954, “Eine neue Darstellung des Periodensystems.” *Chemie in der Schule* 1(1), 32-34.

Toulmin, S.: 1953, *The Philosophy of Science: An Introduction.* Harper Tourchbooks, London.

Van Spronsen, J.: 1969, *The Periodic System of the Chemical Elements: A History of the First Hundred Years*, Elsevier, Amsterdam.

Votsis, I. 2003. “Is structure not enough?” *Philosophy of Science*, **70**, 879-890.

Worrall, J. 1989, “Structural realism: the best of both worlds?” *Dialectica*, **43**, 99-124.

Curriculum Vitae

Jonathan David Sides
1627 John Tucker Road
Aroda, Virginia 22709
jdsides@vt.edu

Education:

2004-2006	Virginia Polytechnic Institute and State University Master of Arts in Philosophy Thesis title: Scientific Realism and the Periodic Table of Chemical Elements
2002-2004	Virginia Polytechnic Institute and State University Bachelor of Science in Biochemistry; *summa cum laude* Minors in Chemistry and Philosophy
2000-2002	Piedmont Virginia Community College Associate in Science, Science; *summa cum laude,* Honors Program
1996-1999	Home-educated under the tutelage of Larry Sides, M.Ed. and Norma Sides, R.N.

Publications and Presentations:

"The Periodic Table, Scientific Realism, and Theories." Presentation delivered at Mephistos 2006, University of Chicago, Chicago.

Work Experience:

Self-employed

May 1999-present	*Llama shearer serving farms in the Blue Ridge Mountains and Shenandoah Valley of Virginia.*

Virginia Polytechnic Institute and State University

August 2004-May 2006	*Graduate Teaching Assistant in Philosophy.*
January 2003-May 2003	*Textbook recorder for students with learning disabilities.*

Summit Ministries Summer Leadership Conference Center

May 2002-August 2002	*Classroom director/ Test administrator.*

The Learning Center, Piedmont Virginia Community College

October 2000-May 2002	*Peer tutor for biology, chemistry, mathematics, and English composition.*

Extracurricular Activities:

Phi Kappa Phi (Honors Fraternity)
December 2003-present

Golden Key Honor Society
October 2003-present

Virginia Tech Philosophy Club
November 2002-Present

Phi Theta Kappa (Honors Society)
September 2001-present

PVCC Honors Program
August 2000-May 2002

Christian Student Fellowship
August 2000-May 2002

Honors, Awards, Prizes:

Jordan Fischer Memorial Scholarship	2004
Dean's List—Virginia Tech (Four semesters)	2002-2003
W. R. Winslow Agricultural Scholarship	2003
Floyd-Francis Scholarship	2003
American Goat Society Scholarship	2002
Farm Bureau Scholarship	2002
President's List—PVCC (Five semesters)	2000-2002
Nominee, Barry M. Goldwater Scholarship	2002

Professional Goals:

I plan to promote the integration of philosophy and the applied sciences by teaching at the college or university level, developing curriculum for K-12 students, and researching in the philosophy of science.