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The Textbook Case of a Priority Dispute: D. I. Mendeleev, Lothar Meyer, and the Periodic System

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Introduction

I have no idea who discovered the periodic system of chemical elements, and I am going to tell you why. When you open a chemistry textbook today, you can often find, next to its periodic table, a sidebar with a grizzled bearded man who is depicted as "the discoverer" of the periodic law, the formulator of the table whose checkered countenance greets you from the wall of every chemistry laboratory in the world. Almost always, that bearded man is Dmitrii Ivanovich Mendeleev (1834–1907), a chemist from St. Petersburg who published his version of this system in 1869—or maybe in 1871, depending on how you figure it. Sometimes he shares the space with the grizzled beard of Julius Lothar Meyer (1830–1895), who published his version in 1864, or 1868,[1] or 1870.[2] A hundred years ago, German textbooks might simply have presented Meyer, and some esoteric texts would have also depicted John Newlands, or Gustav Hinrichs, or one or two others—grizzled beards all. The textbooks are endowed with a certainty I do not have; they know what the periodic table *is*, and therefore they know who discovered it first.

Their framework rests on a preconceived notion of what "the discovery" is, what the fact or theory consists of *in essence*. The difficulty with this approach, however, can be illustrated by drawing a lesson from philosopher Ludwig Wittgenstein's conception of language as a game. That is, there are no specific ostensive meanings to certain words, or given grammar rules written in stone, but rather simply guidelines that only make sense within the framework of a specific set of circumstances. Disagreement can stem from stressing either too few of the similarities or too many of the differences between two concepts:

> If someone were to draw a sharp boundary [around a term such as "game"] I could not acknowledge it as the one that I too always wanted to draw, or

had drawn in my mind. For I did not want to draw one at all. His concept can then be said to be not the same as mine, but akin to it. The kinship is that of two pictures, one of which consists of colour patches with vague contours, and the other of patches similarly shaped and distributed, but with clear contours. The kinship is just as undeniable as the difference.[3]

Kicking a ball might be a solitary exercise, or a move in a game of soccer, or an illegal action in basketball. I could select one of these as the meaning of "kick" and exclude all the others as "not kicks," depending on how I am *using the concept at that moment*. The surrounding context gives meaning to that word.

This might not seem to be too much of a problem in science if you are talking about something like a solar eclipse. We might all agree that the observation of the eclipse happened and give credit to the person who saw it first—assuming our watches were synchronized and that we agreed on whether credit should go to the first person who *saw* it, or who *wrote* it down in a notebook, or who *published* it, or who *explained* it, or who *predicted* it. So, even here, in a case of an ostensibly simple observation of the natural world, we encounter an almost irreducible problem of how to assign credit if credit is to be apportioned with respect to *being first*.[4]

The worries get much worse when we talk about the periodic system of chemical elements. Just about every individual who has had even the most cursory science education can recognize a periodic table on sight; it may be, in fact, the most widely recognized icon of science in the world. It would be really nice to be able to give credit to the person who "discovered it." Here we encounter conceptual difficulties, both in terms of what it means to "discover" the system and then concerning what "it" is.

What *is* the periodic system of chemical elements? Is it the abstract idea of a system? Is it recognition of a periodic law undergirding the ordering of chemical elements? Is it representation of that law and system in a tabular format? Which tabular format? (There are roughly one hundred topologically distinct representations of the periodic system.)[5] We find in the scholarly literature a number of competing definitions by chemists, philosophers, and historians of science as to the essence of the table and therefore who should get credit for having arrived at it first. Candidates for the crucial feature include:[6]

1. Recognition that properties of elements repeat periodically with increase of atomic weight.
2. Arranging a subset of the elements in a two-dimensional grid to present this relation.
3. Using this system to classify *all* known elements.
4. Leaving gaps in the system for elements that have not yet been discovered but whose existence can be inferred from the properties of known elements.
5. Correcting measured properties of known elements using the system (also known as retrodiction).
6. Predicting detailed properties of new elements to fill the gaps.

Depending on which of these claims you take to be the essence of the periodic system of chemical elements, you will end up with a different discoverer who is assigned priority for being first.

I have two problems with this picture: the first is with the notion that there is one law and therefore only one discoverer, and the second is with how we as present-day observers of history detect who came first.[7] First to the problem of essentializing discovery with respect to the periodic table. The periodic table is one of the classic cases of so-called "simultaneous discovery," with six individuals vying for credit in the 1860s alone (bracketing supposed "precursors"). Depending on your commitments to the six points above, you will give the credit to Alexandre-Émile Beguyer de Chancourtois (#1, #2),[8] John Newlands (#2),[9] William Odling (#3),[10] Gustav Hinrichs (#3),[11] Lothar Meyer (#4 and arguably #5),[12] and Dmitrii Mendeleev (#6).[13] Tell me who you think discovered the periodic system in the 1860s, and I will tell you what you think the periodic system *is*. This may be an amusing philosophical parlor game, but it is rather dubious history, because it forces us to project back our conception of what the correct system is and look for its antecedents among this plethora of discoverers/codiscoverers.

Now to the problem of how historians measure "Firstness." *Why* were there so many different systems emerging in the 1860s? The 1860s proved a tumultuous period in the history of chemistry—when almost every concept and theory was up for redefinition, rearticulation, or rejection.[14] In September 1860, attendees of the International Congress of Chemists at Karlsruhe witnessed a seminal speech by the Italian chemist Stanislao Cannizzaro, who argued for a revitalization of Amedeo Avogadro's (or Charles Gerhardt's—another priority mess!) hypothesis to provide for standardized atomic weights. By applying Avogadro's rules consistently, it was possible to reconcile many seeming anomalies among atomic-weight determinations (from C = 6 to C = 12, for example) and thus be in a position to compare the corrected weights to each other and seek relationships among them. Two attendees at this Congress, Meyer and Mendeleev, later cited Cannizzaro's influence as crucial in their individual paths to the periodic system.[15] By the late 1860s, only 63 elements had been discovered (very few of them rare earths), so classification of the substances in a two-dimensional grid was simpler than it might have appeared later. Six periodic systems within the decade; none earlier.

So how do we know who came first? Because most scholars who have examined this question are in thrall to a pre-Wittgensteinian notion of essences of theories, they have searched among scientific articles published in the specialized chemical press. If you believe in individualized nuggets of discovery, this is the perfect place to stalk your quarry, since scientific articles focus on specific claims and they cite predecessors. In this way, you can make a claim that someone did not (or did) know about someone else's work and look for which of our six features was affirmed by the author.

My approach is different. I contend that the genre of the scientific article has often structured how we look at the history of science, a bias that is particularly harmful to understanding episodes in the middle of the nineteenth century when that genre was just beginning to congeal. Instead, I take Wittgenstein's concept of a game seriously. In many of the claims to discovering periodicity, one finds that the periodic system emerged in the context of the writing of a chemistry textbook. Yet the histories of periodicity are written mostly or entirely from journal articles, with scant attention to the textbooks. Here I consider Mendeleev's *Principles of Chemistry* and Meyer's *Modern Theories of Chemistry* as loci of the creation of each individual's periodic system.[16] By exploring how the periodic system fits in the composition and then revision of each of their textbooks, I hope to reorient the discussion a smidgen away from who-found-what-first to what-did-each-want-to-do-with-it. In the context of the systems' deployment in the textbooks, we see that both Mendeleev's and Meyer's systems encoded a picture of what chemistry as a whole was about, and as a result we grasp a crucial difference between these two major claimants—specifically, why Lothar Meyer did not predict the properties of any new elements to fill the gaps in his system, while Mendeleev did. I defer here the interesting history of how these two systems got ripped out of their textbooks and placed in the agonistic field of journal disputation, or the importance of scientific priority over Germans for Russian nationalist politics in this period, as well as an extension of this analysis to the other four contenders for priority. The priority dispute proper took place among the scientific community writ large; the systems, however, were born with the classroom in mind.

Mendeleev's *Principles of Chemistry*

If you recognize the name Dmitrii Ivanovich Mendeleev, you probably heard of him in school—for it is in current chemistry textbooks that he is introduced as *the* discoverer of the periodic law, full stop. I will not adjudicate claims of priority here; I only wish to demonstrate what it means when someone gives sole credit to Mendeleev—which features of the periodic system are emphasized and which features are elided. This section will summarize the process by which Mendeleev came to his formulation of the periodic law in the course of writing his textbook, *Principles of Chemistry* (*Osnovy khimii*) in 1869–1871, and then point to how the pedagogical origins of "Mendeleev's periodic law" stresses particular features as the essence of the periodic system.

Mendeleev was born in Tobol'sk, Siberia, in 1834, the last child of a school inspector and the daughter of a factory owner who had fallen on hard times.[17] After his strong (but not exceptional) performance in school, his recently-widowed mother decided to enroll her son in university and conveyed him first to Moscow (where he was turned down by Russia's oldest university) and then to St. Petersburg (where he failed to gain admission to St. Petersburg University but eventually matriculated in 1851 at his father's *alma mater*, the Chief Pedagogical Institute).

Mendeleev graduated with an emphasis in the natural sciences, especially physics and chemistry, and then undertook study for a master's degree in chemistry at St. Petersburg University. After a number of travails—including a stint teaching at a high school in the Crimea, which he detested—he was sent abroad to Heidelberg University for additional postgraduate study.[18] He returned to St. Petersburg in early 1861, two weeks before Tsar Alexander II abolished serfdom, and took on several adjunct positions—including one at St. Petersburg University for a few months before it was closed for two years due to student unrest—until settling into an extraordinary professorship at the St. Petersburg Technological Institute. In this period of relative penury, he first tried his hand at textbook composition to earn some extra money, penned *Organic Chemistry* very rapidly, and received the additional boon of the Demidov Prize of the Academy of Sciences for the final product in 1862.[19] This textbook, composed around the central concept of Charles Gerhardt's and Auguste Laurent's type theory, was soon eclipsed by the structural framework of Aleksandr M. Butlerov, chemistry professor at Kazan (and soon St. Petersburg), whose textbook, *Introduction to the Complete Study of Organic Chemistry* (*Vvedenie k polnomu izucheniiu organicheskoi khimii*), soon became a classic of Russian chemical pedagogy.[20]

Mendeleev was promoted to professor of chemistry at St. Petersburg University in October 1867. This new position demanded that he teach the introductory inorganic chemistry lecture course, a requirement for all students in the rapidly expanding natural sciences faculty. To do this, he needed to assign a textbook. Unfortunately, Russian-language chemistry textbooks did not exactly grow on trees, especially in the late 1860s, when all prior textbooks quickly became superannuated by the rapid developments in contemporary chemistry. A Russian professor had two choices: pick an up-to-date textbook in French, German, or English and translate it (amending it in the process); or write one from scratch.[21] Mendeleev, concluding that scientific developments would likely eclipse the first option by the time the translation was completed and that he was more likely to turn a self-composed textbook into a lucrative financial venture, opted for the second. The idea to write *Principles of Chemistry* was born.

This was a fortunate decision for us, since Mendeleev's formulation of the periodic system of elements grew directly out of the process of composition of this text.[22] *Principles of Chemistry* consisted of two volumes. Volume 1 was largely written in 1868 and concluded in the first month of 1869. The idea for a periodic arrangement of elements was introduced as Mendeleev attempted to map out an outline for volume 2. Volume 1 consisted of a largely empirical introduction to the practices of being a chemist—providing multilayered and detailed introductions to hydrogen, carbon, oxygen, and nitrogen, as well as the halogen family. This left just under seven-eighths of the 63 known elements for volume 2. Mendeleev needed to come up with an organizational system that would compress them into the same span with which he had dealt with only eight elements. What began as an outline for grouping elements together to ease their exposition soon developed, by late February 1869, into a suggestion for an underlying pattern that united *all* elements into a natural system (figures 3.1 and 3.2).

			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 3.1 The first published version of Mendeleev's periodic system, dated February 17, 1869, produced while composing *Principles of Chemistry*.

Source: D. I. Mendeleev, *Periodicheskii Zakon. Klassiki Nauki*, ed. B. M. Kedrov (Moscow: Izd. AN SSSR, 1958), 9.

[31]		Группа I	Группа II	Группа III	Группа IV	Группа V	Группа VI	Группа VII	Группа VIII. Переход к группе I
Типические элементы		H = 1							
		Li = 7	Be = 9,4	B = 11	C = 12	N = 14	O = 16	F = 19	
Первый период	Ряд 1-й	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31	S = 32	Cl = 35,5	
	— 2-й	K = 39	Ca = 40	— = 44	Ti = 50?	V = 51	Cr = 52	Mn = 55	Fe = 56, Co = 59, Ni = 59, Cu = 63
Второй период	— 3-й	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75	Se = 78	Br = 80	
	— 4-й	Rb = 85	Sr = 87	(?Yt = 88?)	Zr = 90	Nb = 94	Mo = 96	— = 100	Ru = 104, Rh = 104, Pd = 104, Ag = 108
Третий период	— 5-й	(Ag = 108)	Cd = 112	In = 113	Sn = 118	Sb = 122	Te = 128?	J = 127	
	— 6-й	Cs = 133	Ba = 137	— = 137	Ce = 138?	—	—	—	— —
Четвертый период	— 7-й	—	—	—	—	—	—	—	
	— 8-й	—	—	—	—	Ta = 182	W = 184	—	Os = 199?, Ir = 198? Pt = 197?, Au = 197
Пятый период	— 9-й	(Au = 197)	Hg = 200	Tl = 204	Pb = 207	Bi = 208	—	—	
	— 10-й	—	—	—	Th = 232	—	Ur = 240	—	
Высшая соляная окись		R^2O	R^2O^2 или RO	R^2O^3	R^2O^4 или RO^2	R^2O^5	R^2O^6 или RO^3	R^2O^7	R^2O^8 или RO^4
Высшее водородное соединение				$(RH^3?)$	RH^4	RH^3	RH^2	RH	—

Figure 3.2 Short-form periodic system. This version, taken from a November 1870 article by Mendeleev, is virtually identical to one which appeared in the first edition of the *Principles*.

Source: D. I. Mendeleev, *Periodicheskii Zakon. Klassiki Nauki*, ed. B. M. Kedrov (Moscow: Izd. AN SSSR, 1958), 76.

Understandably, Mendeleev did not fully grasp in February 1869 the implications of the periodic system, but certain features of the incomplete first system (such as the question marks embedded in figure 3.1) indicate that he was well on the way to thinking them through. He continued to develop the system for the next two years, during which time he revised the second volume of his textbook, and he completed both the research cycle and the textbook in late 1871. Although Mendeleev would of course tinker with the system throughout his life—even adding a whole group of noble gases for the seventh and eighth editions—he insisted that the essence of the law could be found in the first edition. For example, consider this statement from Mendeleev's fifth (1889) edition: "I would like to show in an elementary exposition of chemistry the tangible utility of the application of the periodic law, which appeared before me in its entirety precisely in 1869, when I wrote this composition . . . In this, 5th, edition I did not change a single essential feature of the original work, but only supplemented it."[23] (This notwithstanding the fact that Mendeleev continued to work on the periodic system, and in each of the eight editions of the *Principles of Chemistry* he elevated its significance and its status to a periodic *law*.)[24] Shortly after the publication of the first edition, Mendeleev claimed in a letter to Emil Erlenmeyer—at that moment editor of *Liebigs Annalen*—that even the 95-page research article he had submitted was inferior in detail to the textbook itself: "Despite its size, the present article does not go over the course of my ideas in all the details, which are developed more completely and fully in my Russian articles and in my 'Principles of Chemistry,' and which I would happily acquaint the German public with."[25]

Mendeleev always stressed not only the periodic system's pedagogic origins but also its continued pedagogic utility (a feature of the system appreciated by chemistry teachers to the present day). Statements on this were so important that he preserved them in numerous translations of his original Russian articles: "I will add still another remark: it is that the use of the periodic law facilitates the learning of chemical facts by beginners. I have come to this conclusion during the courses of lectures that I have given for two years, and during the preparation of my 'Traité de Chemie Inorganic,' now published (in Russian), which treatise is based on the periodic law."[26] For however flighty and mercurial Mendeleev might have been as a natural scientist and a professional colleague, he was deeply committed to undergraduate pedagogy and left a lasting impression on generations of students (he retired from the university, although not from lecturing at various other institutions, in 1890).[27] For Mendeleev, the periodic system was pedagogically inflected into its core because it represented a hypothesis-free (to his lights) means of conveying chemistry. He emphasized this in the same letter to Erlenmeyer quoted above: "I want only that you will pay attention to the fact that I do not set up any hypotheses, because in my view these often seduce students as false keys and thus tend to slow down the free development [of] science."[28]

The pedagogic core of the periodic law reflected Mendeleev's deep commitments as to what were admissible and inadmissible hypotheses in chemistry, such as his skepticism about both atomism and valency. It may appear somewhat counterintuitive that Mendeleev remained for most of his life (he recanted somewhat in his final decade) hostile to the very two concepts—the existence of atoms and the integral units of chemical bonding—that seem to many today to be the central features of the periodic system. In 1877, British chemist William Crookes, in an evaluation of the periodic system, observed that "M. Mendeleeff himself declares that the Periodic Law cannot be harmonised with the Atomic theory without inverting known facts."[29] Mendeleev insisted that the periodic system did not provide any evidence either way on the existence or nonexistence of atoms, and he professed himself happier as an agnostic about their ultimate reality. He was deeply suspicious of Prout's hypothesis, which in its earliest form proposed that all atoms were glommed-together compounds of hydrogen atoms; since this original formulation was clearly ruled out by fractional atomic weights, such as chlorine's 35.5, it was later modified as an umbrella term for any belief that atoms were composite in nature. For Mendeleev, Prout's hypothesis was an instance of unwarranted hypothesizing along the same lines as traditional atomism.[30] His suspicion of valency deepened his general hostility toward overly microscopic interpretations of atomic behavior with a competitive defense of his older type-theoretic organic chemistry in juxtaposition to the Kekulé-Butlerov structure theory.[31]

To today's chemists, Mendeleev's views seem rather bewildering—and they seemed so to his contemporaries as well. While he was not the only chemist who resisted atomism and valency, he was one of a dwindling number, and most of his coskeptics were theoretical reactionaries who resisted even the periodic system. With one exception, on every major theoretical speculation in late nineteenth-century chemistry—atomism, substructure to atoms, the existence of the electron, the existence of noble gases, valency, radioactivity—Mendeleev was on the conservative, *incorrect* side.[32] The exception, of course, was the use of the periodic system to predict the properties of unknown elements. Mendeleev was almost alone in advocating this as a feasible use of the system in the early 1870s, and he was spectacularly right three times—correctly foreseeing the properties of elements eventually discovered as gallium (1875), scandium (1879), and germanium (1886). And these successful predictions are the sole reason we now see Mendeleev as a chemical visionary instead of a chemical reactionary. In the textbook context, we very clearly observe Mendeleev's essential conservatism on the chemical-theoretic issues of the day and notice how the periodic system fits this frame beautifully—an organization of the elements that does not require presumptions about Proutian "primary matter" ("protyles"), or adherence to a specific theory of valency. It was supposed to teach students how to reason chemically with a knowledge of the substances and a resistance to fancy speculation.

In the context of scientific journal articles, however, prediction was quickly elevated not only as the major differentiating point between his claim to priority and Lothar Meyer's (which is true enough), but also as the essential feature of the periodic system. The fact that most historians have assiduously analyzed only these journal articles has resulted in an overweening emphasis on prediction in accounts of Mendeleev's formulation of the system.

Mendeleev's system was announced in foreign chemical journals in basically two ways. First, it was reported in the proceedings of the Russian Chemical Society's meetings, a standard informational bulletin.[33] Second, it emerged in Mendeleev's own translated articles. The first of these pieces, in the *Zeitschrift für Chemie* in 1869, contained a translation error that in itself was the source of much dispute between Mendeleev and Meyer.[34] It is fairly clear from archival sources that Mendeleev had previously been unaware of alternative periodic systems that had appeared either in textbooks or in journals. Now that others were laying claim to having provided the foundation for Mendeleev's obviously more comprehensive and refined system, he became both more defensive and aggressive in his priority claims. He soon declared himself "an enemy of all questions of priority," which is a good indication that the speaker is anything but.[35] But how could he defend himself when he was manifestly the last person to publish a periodic system in the 1860s?

He opted for two main points of attack: independence of his system, and its greater completeness. Both came together under a theory of credit-distribution in the sciences. First, independence:

> I consider it necessary to impart, that during the formulation of the periodic system of elements I used the earlier works of Dumas, Gladstone, Pettenkofer, Kremers, and Lenssen on the atomic weight of similar elements, but that I was unaware of the apparently preceding works of de Chancourtois in France (Vis tellurique or the spiral of elements based on their properties and equivalences) and of J. Newlands in England (Law of octaves, according to which e.g. H, F, Cl, Cr, Br, Pd, J, Pt form the first and O, S, Fe, Se, Ru, Fe, Au, Th form the second octave), in which some embryos of the periodic law are to be seen.[36]

Leaving Lothar Meyer, of course, unmentioned, the man he accused of having stolen periodicity. He only ceded Meyer some credit after the Royal Society awarded the Davy Medal for the periodic system jointly to both men in 1882.[37] (After Meyer's death, Mendeleev started to be positively cordial to the man—but only as a precursor, not as the initiator of a full-fledged competing system.)[38]

Once he had established his independence, Mendeleev made a virtue of coming last, arguing that even though others had found germs of the idea, historical exemplars indicated that true credit should only go to the one who fully realized all the system's implications (in analogy to oxygen being attributed to Antoine Lavoisier as opposed to Joseph Priestley): "It is right to consider as the creator (*Schöpfer*) of a scientific idea he who not only recognized the philosophical concern but also the real side of a matter, who knows how to so illuminate the issue that anyone could be convinced of its truth and it becomes general. Only then would the idea, like matter, become indestructible."[39] This naturally implied that the correct parameter to judge credit was who drew out the furthest correct implications. Once one frames the field in this way, the answer becomes obvious: he who correctly predicted the properties of unknown elements. And we all know who that was—not Lothar Meyer.

Meyer's *Modern Theories of Chemistry*

Based on his background, it is somewhat odd that Lothar Meyer became a chemist at all.[40] He was born in Varel, Oldenburg, on August 19, 1830, the fourth of seven children of a local physician and the daughter of a physician. With this pedigree, his father wanted his sons to become doctors, and Meyer was happy to acquiesce, even more definitively so after his father's death in 1850. Although Meyer was five years older than Mendeleev, the two were exact contemporaries in terms of their careers, since Meyer's father was forced to withdraw his son from school at the age of 14 because of the boy's intense headaches. Meyer was apprenticed for a few years to a gardener (which apparently helped with the migraines), and he reenrolled in school and graduated from the gymnasium in Oldenburg in 1851 (a year after Mendeleev). He matriculated from Zürich University in medicine in May 1851, studied under Carl Jakob Löwig and Carl Friedrich Wilhelm Ludwig, moved to Würzburg (and Rudolf Virchow) after two years, and completed his training on February 25, 1854.

That year he moved to Heidelberg—yet another parallel with Mendeleev—to study with Robert Wilhelm Bunsen, whom he adored. Here the divergences with Mendeleev become clearer, for Meyer loved his time in Heidelberg and continually referred back to it. As one of his obituaries put it: "The years spent at Heidelberg were times of great moment, and their influence is to be distinctly traced in the subsequent work of his life."[41] The work performed there went into his dissertation concerning gases in the blood, published in 1857 in Königsberg, which included the first correct analysis of the mechanism of carbon monoxide poisoning: the displacement of oxygen molecule for molecule in the blood. To develop his growing interest in physical chemistry as he moved further away from medicine, in 1856 Meyer moved to Königsberg to study physics with Franz Ernst Neumann, joining his elder brother Oskar Emil Meyer. He left to take a *Privatdozent* position in physics and chemistry at Breslau in February 1859. There he displayed a sharp talent for chemical theory in his critical work: "On the Chemical Doctrines of Berthollet and Berzelius." He also attended the Karlsruhe Congress.

He was called to his first independent position at the School of Forestry at Neustadt-Eberswalde in 1866. In 1868 he succeeded Carl Weltzien as a professor of chemistry and the director of the chemical laboratory at the Karlsruhe Polytechnic Institute, and settled in 1876 in Tübingen, where he taught until his death on April 11, 1895. His biographers always point to his commitment to pedagogy—he trained over 60 doctoral candidates in chemistry at Tübingen (another contrast to Mendeleev, who trained very few). He taught inorganic chemistry during the winter semester and organic chemistry during the summer, and supplemented the latter with a special lecture course on an advanced topic, often having to do with chemical theory. He served twice as dean and was rector the year before his death. (His last documented official action as rector was awarding Otto von Bismarck an honorary doctorate from the Natural Sciences Faculty in honor of his eightieth birthday.)[42] Running like a scarlet thread through this biography, from Virchow to Bunsen to Neumann to Tübingen, is the importance of pedagogy.

As committed as Meyer was to teaching, he was even more passionate about the proper construction of textbooks so that they included a prominent role for chemical theory, which he felt was underemphasized in most classrooms of the day.[43] Like Mendeleev's, Meyer's periodic system emerged during the composition (and revision) of his textbook, *Modern Theories of Chemistry and Their Significance for Chemical Statics* (*Die modernen Theorien der Chemie und ihre Bedeutung für die chemische Statik*), and throughout his life he continued to develop methods by which the system could be used in the classroom.[44] Unlike Mendeleev, however, Meyer drew a direct line from Cannizarro's development of the theory of atomic-weight determination to his own system, thus placing himself within a continuous development: "After Cannizzaro had established the correct principles for the determination of atomic weights, the regularities which had been observed up to that time took shape in the first edition of my 'Modern Theories,' in 1864."[45]

This book was published while Meyer was still in Breslau and comprised a slim 147 pages. It occupied a liminal space between theoretical treatise and textbook, and was intended as a survey of relevant theories in chemistry, especially atomism and valency. Both of these, he emphasized early in the text, were *chemical* theories, and the purpose of this book was to differentiate the domains of theory in chemistry from those theories that were proper to physics:

> It is undeniable that through the adoption and development of the atomic theory chemistry becomes more and more alienated from its near relation physics. The areas became more sharply differentiated; each discipline went on its own path; the common border districts remained in many cases undeveloped when chemistry has not alone seized them, as more often seems to be the case. Yet almost daily new relations were being discovered between chemical and physical phenomena; but even the greatest discoveries produced by the application of physical methods to the area of chemistry could not establish stronger ties across the loose rift between both disciplines, because the goals of both had become different.
>
> Chemists were concerned, first and foremost, with the countless compounds whose possibility atomic theory allowed one to predict, to produce the largest possible number of them, to study them and to order them systematically. Thus chemistry became more and more a descriptive natural science, in which general theoretical speculations, such as those Berthollet had set in the foreground, only occupied a background significance. This change was necessary . . . [A] theoretical chemistry was demanded for an exact knowledge of an extraordinarily large number of chemical

compounds, without which there was a very near danger that it would run aground . . . [P]erhaps only in the coming century can one build a theory of chemistry that, as now the theory of light or electricity [in physics], can teach us to calculate the phenomena from given conditions in advance.

From this goal that Berthollet had in mind, chemistry is even today still endlessly far away . . . Today's chemistry resembles a plant which has its roots spread out in the soil and gathers nutrients for the later sudden flourishing of stalks, flowers, and fruits. The rich material that the rapid development of atomic theory has enabled guarantees for chemistry its lasting autonomy; it will never again be a dependence, a subdivision of physics.[46]

This lengthy extract highlights several crucial points: that chemistry and physics occupied very different domains, and that this difference stemmed from the different role of theory in each; that chemistry was not yet endowed with overarching predictive theories like those in physics; and that the purpose of theoretical developments was to order empirical data into broad schemes. Yet Meyer noted that chemists tended to be skeptical of overhasty generalizations based on theory: "There thus emerged a feeling of uncertainty or doubt about the value of theoretical efforts in general, that speculations about causes and the essence of phenomena were usually hurried and suggestive, often even not directly stated, leaving the reader to abstract them himself."[47] If Meyer wanted to defend the utility of chemical theory in this textbook—and particularly the importance of atomism—he would have to calm this concern of his peers and show how theory could be useful without necessitating leaps to unfounded conclusions.

	4 werthig	3 werthig	2 werthig	1 werthig	1 werthig	2 werthig
	—	—	—	—	Li = 7,03	(Be = 9,3?)
Differenz =	—	—	—	—	16,02	(14,7)
	C = 12,0	N = 14,04	O = 16,00	Fl = 19,0	Na = 23,05	Mg = 24,0
Differenz =	16,5	16,96	16,07	16,46	16,08	16,0
	Si = 28,5	P = 31,0	S = 32,07	Cl = 35,46	K = 39,13	Ca = 40,0
Differenz =	$\frac{89,1}{2}$ = 44,55	44,0	46,7	44,51	46,3	47,6
	—	As = 75,0	Se = 78,8	Br = 79,97	Rb = 85,4	Sr = 87,6
Differenz =	$\frac{89,1}{2}$ = 44,55	45,6	49,5	46,8	47,6	49,5
	Sn = 117,6	Sb = 120,6	Te = 128,3	J = 126,8	Cs = 133,0	Ba = 137,1
Differenz =	89,4 = 2.44,7	87,4 = 2.43,7	—	—	(71 = 2.35,5)	—
	Pb = 207,0	Bi = 208,0	—	—	(Tl = 204?)	—

Figure 3.3 Lothar Meyer's table of elements from the first edition of *Modern Theories of Chemistry* (1864).

Source: Lothar Meyer, *Die modernen Theorien der Chemie und ihre Bedeutung für die chemische Statik* (Breslau: Maruschke & Berendt, 1864), 137.

An excellent illustration of this point was his system for organizing the elements on the twin axes of atomism and valency, often called (anachronistically) his first periodic table (figure 3.3). The image appears late in the book and is meant to show the regularities of the amount of increase of atomic weight within groups of similar valency (*Werthigkeit*), the differences being indicated by the calculations between the rows. The point here was to solidify and emphasize the conceptual utility of both atomic weights and valency theories by showing that they, heretofore treated independently in the book, seemed connected by deeper regularities. This link was mostly implicit in Meyer's account. He introduced the table thus: "The following table gives such relations [between the atomic weights] for six related well characterized groups of elements."[48]

This partial table is pretty impressive; one might think that one could use it as a springboard for evaluating empirical results. But Meyer was very careful to exclude precisely this use of the system:

It is surely not to be doubted, that a definite regularity (*Gesetzmässigkeit*) prevails in the numerical values of atomic weights. It is rather improbable that it is as simple as it appears, if one leaves aside the relatively small deviations in the values of the evident differences. In part indeed these deviations can justifiably be seen as brought about through incorrectly determined values of atomic weights. But this can hardly be the case for all of them; and entirely certainly one is not justified, as is seen all too often, to want to arbitrarily correct and change the empirically determined atomic weights due to a suspected regularity, before experiment has set a more exact determined value in its place.[49]

Thus, immediately after introducing a system of elements, Meyer turned its suggestiveness into an object lesson in theoretical humility. The purpose of this system, and the whole book, was to provide a middle ground in defending the restrained utility of theory as opposed to unrestrained empiricism. As he commented in his conclusion: "The more science progresses, the more it will be possible to keep in abeyance the damaging influence of hypotheses and theories. Also in chemistry one will more and more be in the position, as is now the case in physics, to always keep in view the dependence between each hypothesis and the results of observation compared with theoretical consequences."[50]

The second edition of *Modern Theories*, published in 1872, and at 364 pages now ballooned to over double its original size, further developed his table of elements into a "true" periodic system and insisted even more forcefully on restraint in using it for prediction. Meyer expanded the work to make it more useful as a textbook: "Through this expansion of observational material the book has come to approach more closely the form of a textbook or handbook."[51] Emphasizing his pedagogical intent, he dedicated it to his mentor, Bunsen. There were many interesting features in this new edition—including a mention of Prout's hypothesis, absent from the first edition—but perhaps none more striking than how he now treated the periodic system.[52]

The two most salient aspects of Meyer's development of the periodic system in this second edition were his handling of priority claims and his attitude to prediction. As a rule, both in the textbook and in his journal articles about periodicity, Lothar Meyer was scrupulous about acknowledging both "precursors" and giving lavish attention to Mendeleev (although the latter felt the attention was not lavish enough).[53] Given Meyer's goal of enhancing the status of theoretical developments in chemistry, this distribution of credit made a great deal of sense; by showing a continuous development of atomism through Cannizzaro and to the periodic system, he could demonstrate the utility of continuous attention to theory.

With respect to the possibility—and the *desirability*—of prediction, he was much more circumspect. After displaying a modified periodic table (figure 3.4) and his famous curve of increasing atomic volumes (figure 3.5), Meyer noted:

> As one runs through the row of elements by magnitude of atomic weight, one sees the periodicity of properties in their dependence upon the magnitude of atomic weight very clearly. While the differences of the atomic weights that immediately follow each other seem to pertain to no simple law, one sees between the atomic weights of members of one and the same family entirely regular relations.[54]

This is hardly a ringing endorsement for reformulating chemistry around the periodic system. (Recall, however, that Mendeleev also proposed nothing of the sort; he did not enhance the *structural* centrality of the system as a

I.								H 1		
II.	B 11,0	C 11,97	N 14,01	O 15,96	F 19,1	—	—		Li 7,01	Be 9,3
III.	Al 27,3	Si 28	P 30,96	S 31,98	Cl 35,37	—	—	—	Na 22,99	Mg 23,94
IV.	? 47?	Ti 48	V 51,2	Cr 52,4	Mn 54,8	Fe 55,9	Co 58,6	Ni 58,6	K 39,04	Ca 39,90
V.	? 70?	? 72?	As 74,9	Se 78	Br 79,75				Cu 63,3	Zn 64,9
VI.	? 88?	Zr 90	Nb 94	Mo 95,6	? 98?	Ru 103,5	Rh 104,1	Pd 106,2	Rb 85,2	Sr 87,2
VII.	In 113,4	Sn 117,8	Sb 122	Te 128	J 126,53				Ag 107,66	Cd 111,6
VIII.	? 173?	? 178?	Ta 182	W 184,0	? 186?	Os 198,6	Ir 196,7	Pt 196,7	Cs 132,7	Ba 136,8
IX.	Tl 202,7	Pb 206,4	Bi 207,5						Au 196,2	Hg 199,8

Figure 3.4 One of Lothar Meyer's 1872 periodic tables.

Source: Lothar Meyer, *Die modernen Theorien der Chemie und ihre Bedeutung für die chemische Statik*, 2d. ed. (Breslau: Maruschke & Berendt, 1872), 301.

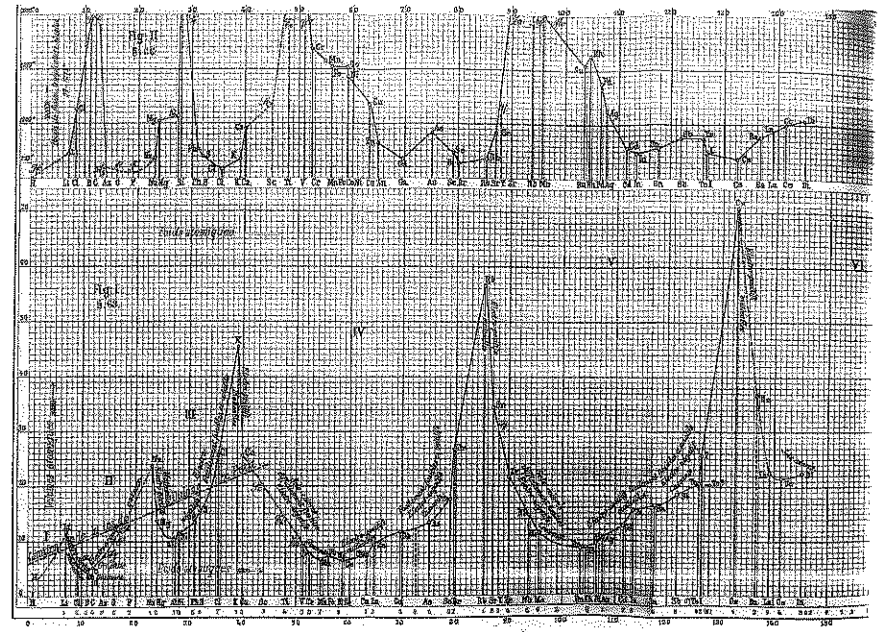

Figure 3.5 Lothar Meyer's atomic-volume curve, also published in the 1872 textbook on the inside cover. This more complete version comes from his 1870 journal article.

Source: Lothar Meyer, "Die Natur der chemischen Elemente als Function ihrer Atomgewichte," *Annalen der Chemie und Pharmacie*, Supp. VII (1870): 354–364, insert.

pedagogical tool in later editions of his *Principles*, although he did accentuate the powers of the periodic law for conceptual understanding.) The importance of Meyer's claim that the periodic system reflected "no simple law" was to exclude the possibility of making predictions based on the system—in a textbook published the year after Mendeleev had done just that. "We are however conscious of the weakness of our weapons," he continued, "so it is as always allowed for us to test our powers through this, that we can predict the properties of still undiscovered elements with the greatest possible probability, in order to later perhaps compare them with the actually observed ones and then be able to judge the value or lack of value of our theoretical speculations."[55] To clinch the point:

> If chemistry is to be spared new deeply distressing catastrophes, one must before all else strive for a correct valuation of hypotheses and theories, that, as we hope, will soon become a general resource for all researchers. As we have happily overcome the false disdain for hypotheses and theories and the overweening fear of their dangerousness, so we must also take care to avoid the opposite extreme in chemistry, the simplistic erection, overvaluation and dogmatization of hypothetical assumptions.[56]

Thus, from the textbook context, we can clearly see that Meyer refrained from making detailed predictions of undiscovered elements—although he left gaps in his table and engaged in some interpolations—not because of timidity or fear of hypotheses but to prove a point about the conjunction of observation and theory.[57] This was a pedagogical point, a point to be drilled into students. (Meyer's extensive experimental work on the accurate determination of atomic weights offered a complementary research agenda to exemplify his pedagogical stance.)[58]

Each further edition of *Modern Theories*, even after he expanded it from chemical "statics" to chemical "mechanics" (the change took place in the fourth edition), continued to downplay prediction, denying that it formed any part of chemistry's domain (at least at present) and assigning it to physics. This was true even in his third lightly-revised edition of 1876, published after the discovery of gallium and the first successful confirmation of Mendeleev's predictions.[59] The fourth edition expanded to 607 pages and included a great deal about atomic dynamics (derived from innovations from organic chemistry), and began ever more to resemble a textbook organized around the twin principles of atomism and valency.[60] His revisions continued to be minor and in the direction of comprehensiveness rather than transformation until the sixth edition, which was published posthumously by his brother. While preparing this version (it had, after all, been thirty years since the first), Meyer decided to split the book into three separate volumes—as it had indeed been split internally since the fourth edition. He had finished the first third and sent it off to the publisher on the morning of April 11, 1895; that afternoon he suffered the stroke that killed him by evening.[61]

Modern Theories was not Meyer's only textbook venture. He also published a more traditional textbook, *Essentials of Theoretical Chemistry* (*Grundzüge der theoretischen Chemie*) in 1890, dedicated to his other pedagogical idol, Franz Neumann.[62] Even though this textbook came after all three of Mendeleev's successful predictions, and was even more emphatically in favor of Prout's hypothesis and other controversial theories, Meyer still urged caution to students in thinking about the reliability of theory: "Never however are we allowed to take even the best established theory for absolute truth; high probability is the highest that we attain."[63] This avowal of a healthy skepticism continued into the multiple revised editions produced posthumously by his students.[64]

Thus we find a striking divergence between Mendeleev and Meyer in terms of their stances on the controversial issues of the day. On every contemporary theoretical issue of consequence—the existence of atoms, their substructure, the validity of Prout's hypothesis, the centrality of valency—Meyer not only stood clearly in their favor but also gave answers that are now considered by chemists to be right on each point, while Mendeleev's are not.[65] We are thus faced with an intriguing contrast: Mendeleev was hostile to most forms of speculative chemistry, was fundamentally conservative on theory, and still made astonishingly accurate predictions of the properties of yet-undiscovered elements; on the other hand, Lothar Meyer felt a strong affinity to theories in chemistry and asserted their validity but refused to consider the periodic system a stable enough platform from which to speculate. What are we to make of this difference?

Conclusion: A question of "boldness"?

Russian (and especially Soviet) authors typically surmounted the impasse by endorsing Mendeleev's scheme for giving credit to the most "developed" system and systematically ignoring his puzzling theoretical myopia. Western scholars have mostly shied away from this approach, but several efforts have harnessed prediction to resolve the strange antisymmetry between Meyer and Mendeleev. In this framework, what is to be explained is not why Mendeleev was inconsistent on the issue of theoretical speculation but assumed that prediction was the natural end result of the periodic system, and frame the question instead as: Why didn't Meyer make any predictions? The answers boil down to an issue of personality—declaring Mendeleev a more "bold" (*kühn* in the German) chemist in hazarding predictions and faulting Meyer for an implied timidity:

> But it is especially in the deductive application of the system, that we find the Russian scientist much in advance of the German; the scope of the phenomena encompassed, the definiteness and lucidity of the reasons adduced for the conclusions arrived at, the number and importance of the predictions made together the marvelous way in which these have been verified, have combined to make this part of Mendeleeff's work one of the greatest scientific achievements of the century, one of the most striking confirmations of the modern method.[66]

Even Meyer ascribed "boldness" to Mendeleev in the third edition of *Modern Theories*.[67]

There is some justification in the historical record for this emphasis on prediction as the relevant axis for differentiating the two chemists. The idea of prediction excited quite a few chemists from the beginning, however skeptical they were toward the correctness of Mendeleev's claims. In one of the first characterizations of Mendeleev's predictions to the German Chemical Society on December 18, 1870, for example, V. von Richter atypically waxed emphatic about the possibility of predicting the properties of yet-undiscovered elements: "Interesting predictions, if some of these elements are eventually really discovered!"[68] In the fifth edition of *Principles of Chemistry*, Mendeleev himself mocked Meyer for not "rushing" to make predictions.[69]

Yet this explanation is unsatisfying, for several reasons. First, it fails to explain why Mendeleev refused to be bold about other "speculations" in chemistry that were rather less radical than his predictions—such as, say, the existence of atoms. Mendeleev's clearly conservative stance on many political and social matters seems to indicate that his caution was more typical than his "boldness," which should suggest that his willingness to predict needs to be *explained*, not presumed.[70] Further, this interpretation ignores the clear evidence of Meyer's enthusiasm for theoretical elaborations in many instances (which, to be Whiggish again for a moment, one might reiterate happened to be correct). Finally, this metric of audacity naturalizes and fixes certain features of chemistry—that it is supposed to be a predictive natural science—that were openly disputed at the time.[71] Reduction to a matter of personal courage obscures much more than it reveals in what should be, at least in part, a story about chemistry's disciplinary boundaries.

Much more appropriate is a consideration of the pedagogical motivations for each chemist and the context of textbook-writing in the development of each of their systems. Both systems emerged as solutions to problems of textbook composition (Mendeleev) and pedagogical presentation of theories (Meyer). In the textbook context, *both* scientists refused to draw extensive implications from their systems: Meyer quite explicitly and Mendeleev by leaving extensive discussion of predictions out of his *Principles*. The difference stems from what happened once the periodic system moved into the journal literature: *there*, Mendeleev began to expand on speculative predictions, while Meyer held his system much closer to its original pedagogic context. Recall that there are two questions that need to be explained: why did Meyer refuse to predict, and why did Mendeleev feel comfortable predicting? The textbook origins of the periodic system provide an answer to the first question. The second question still remains to be answered—indeed, remains to be asked—by philosophers and historians of chemistry.

The purpose of this essay was to clarify and reframe some assumptions of present-day observers as they think about the periodic-table priority dispute. My goal is not to allocate credit differently—or to attribute credit at all, for that matter. Late-nineteenth-century contemporaries already solved that problem to their satisfaction by assigning both men the 1882 Davy Medal for their work on the periodic system, solomonically splitting credit down the middle. Yet even this compromise did not last very long. At a meeting of the British Association for the Advancement of Science in Manchester in 1887, both Mendeleev and Meyer were in attendance at an awards banquet, and already then one could observe Meyer being eclipsed by Mendeleev's shadow. According to an eyewitness:

> [W]hen, at the conclusion of Dr. Schunk's address, there was a call for a speech from Mendeléef[f], he declined to make an attempt to address the section in English, and simply rose in his place to bow his acknowledgments, an action followed by the rising of Meyer from his seat next to Mendeléeff, and who, as if to prevent any misconception, prefaced his speech with the declaration, "I am not Mendeléeff," a statement which may, perhaps, have disappointed some of his hearers, but the round of applause which greeted his further remark, "I am Lothar Meyer," proved that the feeling, if it existed at all, was more than counterbalanced by the anticipation of the pleasure of listening to the words of one whose name will ever in the annals of our science be justly associated with that of the great Russian chemist.[72]

The audience that day knew something that the textbooks relating the discovery of the periodic system have forgotten—that Meyer was not a usurper, a false claimant to the title of discoverer. He was not simply "not Mendeléeff"; he was a chemist with his own approach to the periodic system, a different but related system that was enmeshed in a complex of other pedagogical goals. Yet simultaneously, that audience signaled something else—that after the dust settled, Mendeleev structured the storyline of the periodic law, and Meyer's importance, such as it was, came from being "justly associated" with his Russian counterpart.

Mendeleev's shadow in the story of chemistry has swallowed up any number of others. In 1974, at the beginning of his first published book, *H. G. J. Moseley: The Life and Letters of an English Physicist, 1887–1915*, historian of science John Heilbron found the same effect. Moseley was a striking character for a number of reasons—not least his death at Gallipoli, a sizable blow to British science—but his scientific reputation rests primarily on his use of x-rays to establish that the elements in the periodic system were arranged not by increasing atomic weight (for there were exceptions, such as heavier tellurium preceding lighter iodine) but by the rising quantity of nuclear charge, what came to be known as atomic number. If we were playing the "who discovered the periodic table" parlor game, we could add a seventh point to our earlier list: "Explained the ordering of the elements and the repetition of their properties." Credit under that definition would probably fall to Harry Moseley. Heilbron, as one might expect, knew better than to embark down that path. The closest he came was in his second epigraph, quoted in French from the noted experimental physicist Maurice de Broglie: "Moseley's law justifies Mendeleev's classification; it justifies even the little tweaks that one has been obliged to give to this classification."[73] He, too, was not Mendeleev, and his law mostly survives as an adjunct to a discovery that had been credited to the Russian before Moseley was born. Ask not who discovered the periodic system; ask why you want to know the answer.

Notes

1. This is the odd date out. It comes from examining drafts of Meyer's textbook dated *before* Mendeleev's 1869 publications. See Karl Seubert, "Zur Geschichte des periodischen Systems," *Zeitschrift für anorganische Chemie* 9 (1895): 334–338.
2. Sharing credit between Meyer and Mendeleev used to be more common than it is today. See, for example, Curt Schmidt, *Das periodische System der chemischen Elemente* (Leipzig: Johann Ambrosius Barth, 1917), 22 and Karl Seubert, ed., *Das natürliche System der chemischen Elemente: Abhandlungen von Lothar Meyer und D. Mendelejeff* (Leipzig: Wilhelm Engelmann, 1895), 122.
3. Ludwig Wittgenstein, *Philosophical Investigations*, tr. G. E. M. Anscombe (Oxford: Basil Blackwell, 1953), § 76, 36e.

4. For the classic and still relevant discussion of this problem, see Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review* 22 (1957): 635–659; and Merton, "Singletons and Multiples in Scientific Discovery: A Chapter in the Sociology of Science," *Proceedings of the American Philosophical Society* 105, no. 5 (1961): 470–486.
5. Edward G. Mazurs, *Graphic Representations of the Periodic System during One Hundred Years* (University: University of Alabama Press, 1974 [1957]).
6. This list is drawn from a synthesis of: Eric R. Scerri, *The Periodic Table: Its Story and Its Significance* (New York: Oxford University Press, 2007); J. W. Van Spronsen, *The Periodic System of Chemical Elements: A History of the First Hundred Years* (Amsterdam: Elsevier, 1969); Hinne Hettema and Theo A. F. Kuipers, "The Periodic Table—Its Formalization, Status, and Relation to Atomic Theory," *Erkenntnis* 28 (1988): 387–408; and Heinz Cassebaum and George B. Kauffman, "The Periodic System of the Chemical Elements: The Search for Its Discoverer," *Isis* 62 (1971): 314–327.
7. For thoughtful critiques of the notion of "discovery" in the sciences, see Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (Chicago: University of Chicago Press, 2006), 19–26 and Augustine Brannigan, *The Social Basis of Scientific Discoveries* (Cambridge: Cambridge University Press, 1981).
8. P. Lecoq de Boisbaudran and A. de Lapparent, "Sur une réclamation de priorité en faveur de M. de Chancourtois, relativement aux relations numériques des poids atomiques," *Comptes rendus* 112, no. 2 (1891): 77–81 and P. J. Hartog, "A First Foreshadowing of the Periodic Law," *Nature* 41 (1889): 186–188.
9. The most strenuous advocate of Newlands's priority was Newlands himself; see especially J. A. R. Newlands, *On the Discovery of the Periodic Law, and on Relations Among the Atomic Weights* (London: E. & F. N. Spon, 1884).
10. See Gerstl's correspondence from London, January 29, 1871, printed in *Berichte der Deutschen Chemischen Gesellschaft* 4 (1871): 132 and Cassebaum and Kauffman, "The Periodic System of Chemical Elements," 320.
11. Carl A. Zapffe, "Gustavus Hinrichs, Precursor of Mendeleev," *Isis* 60 (1969): 461–476 and J. W. Van Spronsen, "Gustavus Detlef Hinrichs Discovered, One Century Ago, the Periodic System of the Chemical Elements," *Janus* 56 (1969): 46–62.
12. Tentatively argued in Scerri, *The Periodic Table*, 93 and more vigorously in Friedemann Rex, "Zur Erinnerung an Felix Hoppe-Seyler, Lothar Meyer und Walter Hückel: Berufungsgeschichten und Periodensystem," *Bausteine zur Tübinger Universitätsgeschichte* 8 (1997): 103–130, on p. 130.
13. François Dagognet, *Tableaux et Langages de la Chimie* (Paris: Éditions du Seuil, 1969), 97; Don C. Rawson, "The Process of Discovery: Mendeleev and the Periodic Law," *Annals of Science* 31 (1974): 181–204; Masanori Kaji, "Mendeleev's Discovery of the Periodic Law of the Chemical Elements: The Scientific and Social Context of His Discovery (english [sic] summary)," in *Mendeleev's Discovery of the Periodic Law of the Chemical Elements—The Scientific and Social Context of His Discovery* [in Japanese] (Sapporo: Hokkaido University Press, 1997), 365–380; George Gorin, "Mendeleev and Moseley: The Principal Discoverers of the Periodic Law," *Journal of Chemical Education* 73 (1996): 490–493; and F. P. Venable, *The Development of the Periodic Law* (Easton, PA: Chemical Publishing Co., 1896), 94.
14. See the excellent discussion in Alan J. Rocke, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry* (Berkeley: University of California Press, 1993).
15. For Meyer's acknowledgement of Cannizzaro's influence, see Lothar Meyer, ed., *Abriss eines Lehrganges der Theoretischen Chemie vorgetragen an der Universität Genua von Prof. S. Cannizzaro*, tr. Arthur Moliati (Leipzig: Wilhelm Engelmann, 1891) and Gerhard Fritz, "Lothar Meyer in Karlsruhe," *Bausteine zur Tübinger Universitätsgeschichte* 8 (1997): 75–78. For Mendeleev's, see his contemporary report on the Congress, D. I. Mendeleev to A. A. Voskresenskii, September 7, 1860 (O.S.), published as "Khimicheskii kongress v Karslrue," *S.-Peterburgskie Vedomosti*, November 2, 1860, #238 and his later Faraday Lecture—after having established his claim on the periodic law—in D. I. Mendeleev, "The Periodic Law of Chemical Elements," *Journal of the Chemical Society* 55 (1889): 634–656.

16. I draw liberally on a series of recent studies on using textbooks to analyze the development of nineteenth-century chemistry, especially Anders Lundgren and Bernadette Bensaude-Vincent, eds., *Communicating Chemistry: Textbooks and Their Audiences, 1789–1939* (Canton, Mass.: Science History Publications/USA, 2000).
17. The biographical details are drawn from Michael D. Gordin, *A Well-Ordered Thing: Dmitrii Mendeleev and the Shadow of the Periodic Table* (New York: Basic Books, 2004).
18. For details, see Michael D. Gordin, "The Heidelberg Circle: German Inflections on the Professionalization of Russian Chemistry in the 1860s," *Osiris* 23 (2008): 23–49.
19. D. I. Mendeleev, *Organicheskaia khimiia* (St. Petersburg: Obshchestvennaia pol'za, 1861). On the importance of this text for Mendeleev's later thought in inorganic chemistry, see Michael D. Gordin, "The Organic Roots of Mendeleev's Periodic Law," *Historical Studies in the Physical and Biological Sciences* 32 (2002): 263–290.
20. The textbook is reproduced as volume 2 of A. M. Butlerov, *Sochineniia*, 3 vols. (Moscow: Izd. AN SSSR, 1953–1958). On the three editions (two Russian, one German) of this textbook, see G. V. Bykov, "Materialy k istorii trekh pervykh izdanii 'Vvedeniia k polnomu izucheniiu organicheskoi khimii' A. M. Butlerova," *Trudy Instituta istorii estestvoznaniia i tekhniki* 6 (1955): 243–291.
21. K. Ia. Parmenov, *Khimiia kak uchebnyi predmet v dorevoliutsionnoi i sovetskoi shkole* (Moscow: Akademiia pedagogicheskikh nauk RSFSR, 1963). Chapter 3 of this work discusses the lasting impact of Mendeleev's *Principles of Chemistry.*
22. Gordin, *A Well-Ordered Thing*, Chapter 2, and references therein.
23. D. I. Mendeleev, from the introduction to the fifth edition of *Osnovy khimii* (1889), as reproduced in D. I. Mendeleev, *Periodicheskii zakon: Dopolnitel'nye materialy. Klassiki nauki*, ed. B. M. Kedrov (Moscow: Izd. AN SSSR, 1960), 381. Ellipses added.
24. Gordin, *A Well-Ordered Thing*, 183–187.
25. Mendeleev to Erlenmeyer, [August 1871?], repr. in Otto Krätz, "Zwei Briefe Dmitri Iwanowitsch Mendelejeffs an Emil Erlenmeyer," *Physis* 12 (1970): 347–352, on p. 351. The article in question is Mendeleev's famous "Die periodische Gesetzmässigkeit der chemischen Elemente," *Liebigs Annalen der Chemie und Pharmacie*, Supp. VIII (1872): 133–229.
26. D. Mendeleeff, "The Periodic Law of Chemical Elements," *Chemical News* 41 (1881): 2–3, on p. 3.
27. V. P. Veinberg, *Iz vospominanii o Dmitrii Ivanoviche Mendeleeve kak lektor* (Tomsk: Tip. gubernskago upravleniia, 1910); V. A. Krotikov and I. N. Filimonova, "Ocherk pedagogicheskoi deiatel'nosti D. I. Mendeleeva v Peterburgskom universitete (1856–1867 gg.)," *Vestnik Leningradskogo universiteta* 10 (1958): 126–132; Krotikov and Filimonova, "Ocherk pedagogicheskoi deiatel'nosti D. I. Mendeleeva v Peterburgskom universitete (1867–1881 gg.)," *Vestnik Leningradskogo universiteta* 16 (1958): 140–148; and Krotikov and Filimonova, "Ocherk pedagogicheskoi deiatel'nosti D. I. Mendeleeva v Peterburgskom universitete (1881–1890 gg.)," *Vestnik Leningradskogo universiteta*, no. 4 (1959): 112–119.
28. Mendeleev to Erlenmeyer, [August 1871?], repr. in Krätz, "Zwei Briefe Dmitri Iwanowitsch Mendelejeffs an Emil Erlenmeyer," 351.
29. [William Crookes], "The Chemistry of the Future," *Quarterly Journal of Science* 7 (1877): 289–306, on p. 306.

30. Don Rawson has suggested that Mendeleev's hostility to Prout liberated him from the numerological tendency one often observes in earlier claimants to discovery of the periodic law, particularly Newlands: Rawson, "The Process of Discovery." On Prout and his hypothesis, see W. H. Brock, *From Protyle to Proton: William Prout and the Nature of Matter, 1785–1985* (Bristol: Adam Hilger Ltd., 1985). On skepticism toward atomism and the possibilities for subatomic structure in this period, see:: Alan J. Rocke, *Chemical Atomism in the Nineteenth Century: From Dalton to Cannizzaro* (Columbus: Ohio State University Press, 1984).
31. A. A. Makarenya, "Development of the Valency Concept in the Aspect of the Theory of Periodicity," in V. I. Kuznetsov, ed., *Theory of Valency in Progress*, tr. Alexander Rosinkin (Moscow: Mir Publishers, 1980): 75–84.
32. Gordin, *A Well-Ordered Thing*, Chapter 8. To be clear: these positions are considered incorrect *today*. I flirt with Whiggish language here because Mendeleev's views were also regarded as incorrect to most practicing chemists in his own day.
33. V. von Richter, "[Correspondence from St. Petersburg]," *Berichte der Deutschen Chemischen Gesellschaft zu Berlin* 2 (1869): 552–554 and von Richter, "[Correspondence from St. Petersburg]," *Berichte der Deutschen Chemischen Gesellschaft zu Berlin* 3 (1870): 988–992.
34. D. Mendelejeff, "Ueber die Beziehungen der Eigenschaften zu den Atomgewichten der Elemente." *Zeitschrift für Chemie*, n.s. 5 (1869): 405–406. Mendeleev was aware of the translation error (*stufenweise* vs. *periodisch*; "gradual" vs. "periodic") and took Meyer to task for not checking the Russian original. (Mendelejeff, "Zur Frage über das System der Elemente," *Berichte der Detuschen Chemischen Gesellschaft* 4 (1871): 348–352, on p. 351.) Meyer's response reflected his exasperation: "It seems to me too strong a demand that we German chemists should read, not merely the memoirs appearing in the Germanic and Romantic languages, but those also which are produced in the Slavic tongues and should test the German abstracts for their accuracy." Lothar Meyer, "The History of Atomic Periodicity," *Chemical News* 43 (1881): 15. The dynamics of these linguistic disputes lie beyond the scope of this essay.
35. Mendelejeff, "Zur Frage über das System der Elemente," 352.
36. D. Mendelejeff, *Grundlagen der Chemie*, tr. L. Jawein and A. Thillot (St. Petersburg: Carl Ricker, 1890), 683–684n8.
37. Mendelejeff, *Grundlagen der Chemie*, 684n8. He did give (690n12) Meyer some credit for the 1864 table although he pointed out its incompleteness. For a more detailed, point-by-point, and almost *ad hominem* attack on Meyer, see Mendelejeff, "Zur Geschichte des periodischen Gesetzes," *Berichte der Deutschen Chemischen Gesellschaft* 13 (1880): 1796–1804, on p. 1801.
38. D. Mendeléeff, "Comment j'ai trouvé le système périodique des éléments," *Revue générale de chemie pure et appliquée* 4 (1901): 533–546, on p. 538.
39. Mendelejeff, "Zur Geschichte des periodischen Gesetzes," 1802.
40. Biographical details are drawn from Otto Theodor Benfey, "Meyer, Lothar," in Charles Coulston Gillespie, ed., *Dictionary of Scientific Biography* (New York: Scribner, 1970), IX and X, 347–353; P. Phillips Bedson, "Lothar Meyer Memorial Lecture," *Journal of the Chemical Society* 69 (1896): 1403–1439; Klaus Danzer, *Dmitri I. Mendelejew und Lothar Meyer: Die Schöpfer des Periodensystems der chemischen Elemente*, 2nd ed. (Leipzig: B. G. Teubner Verlagsgesellschaft, 1974) and Friedemann Rex, "Lothar Meyer im Spiegel seiner Veröffentlichungen," *Bausteine zur Tübinger Universitätsgeschichte* 8 (1997): 89–102.
41. Bedson, "Lothar Meyer Memorial Lecture," 1405.
42. Bernd Stutte, "Lothar Meyer in Tübingen," *Bausteine zur Tübinger Universitätsgeschichte* 8 (1997): 79–88 and Danzer, *Dmitri I. Mendelejew und Lothar Meyer*, 62.

43. See the dispute he had with Erlenmeyer concerning the latter's pedagogical practices, which were heavily empirical and, Meyer thought, counterproductively excluded theory. Lothar Meyer to Emil Erlenmeyer, November 28, 1882, HS-242/17, Archive of the Deutsches Museum, Munich, Germany.
44. Lothar Meyer, "Ueber den Vortrag der anorganischen Chemie nach dem natürlichen Systeme der Elemente," *Berichte der Deutschen Chemischen Gesellschaft* 26 (1893): 1230–1250.
45. Meyer, "On the History of Atomistic Periodicity," *Chemical News* 41 (April 30, 1880): 203.
46. Lothar Meyer, *Die modernen Theorien der Chemie und ihre Bedeutung für die chemische Statik* (Breslau: Maruschke & Berendt, 1864), 7–8. Ellipses added.
47. Meyer, *Die modernen Theorien der Chemie* (1864), 1st ed., 12
48. Meyer, *Die modernen Theorien der Chemie* (1864), 1st ed., 137.
49. Meyer, *Die modernen Theorien der Chemie* (1864), 1st ed., 139.
50. Meyer, *Die modernen Theorien der Chemie* (1864), 1st ed., 144.
51. Lothar Meyer, *Die modernen Theorien der Chemie und ihre Bedeutung für die chemische Statik*, 2nd ed. (Breslau: Maruschke & Berendt, 1872), viii–ix.
52. For Prout, see Meyer, *Die modernen Theorien der Chemie* (1872), 2nd ed., 292.
53. Meyer, *Die modernen Theorien der Chemie* (1872), 2nd ed., 294–300. On his distress at Mendeleev's "violent reply" to his articles on periodicity, see Meyer, "The History of Atomic Periodicity," 15. It should be said that while Meyer would specifically credit Mendeleev for his predictions, he also pointed out the places where the Russian chemist was inexact: Lothar Meyer, *Grundzüge der theoretischen Chemie* (Leipzig: Breitkopf & Härtel, 1890), 60–61.
54. Meyer, *Die modernen Theorien der Chemie* (1872), 2nd ed., 302–303. For Meyer's analysis of his curve, see ibid., 307.
55. Meyer, *Die modernen Theorien der Chemie* (1872), 2nd ed., 344.
56. Meyer, *Die modernen Theorien der Chemie* (1872), 2nd ed., 362.
57. One can observe some of these interpolations and gaps through minute inspection of the atomic-volumes curve. Lothar Meyer, "Die Natur der chemischen Elemente als Function ihrer Atomgewichte," *Annalen der Chemie und Pharmacie*, Supp. VII (1870): 354–364, on p. 360.
58. Lothar Meyer and Karl Seubert, *Die Atomgewichte der Elemente aus den Originalzahlen neu berechnet* (Leipzig: Breitkopf & Härtel, 1883).
59. Meyer continued to give Mendeleev a great deal of credit even here: Lothar Meyer, *Die modernen Theorien der Chemie und ihre Bedeutung für die chemische Statik*, 3rd ed. (Breslau: Maruschke & Berendt, 1876), xvii. On the other hand, his patience wore thin with Mendeleev's tone about priority: "His [1869] scheme then still contained much arbitrariness and irregularities that were later eradicated." Ibid., 291n.
60. Lothar Meyer, *Die modernen Theorien der Chemie und ihre Bedeutung für die chemische Mechanik*, 4th ed. (Breslau: Maruschke & Berendt, 1883).
61. Lothar Meyer, *Die modernen Theorein der Chemie und ihre Bedeutung für die chemische Mechanik*, 6th ed., vol. 1: *Die Atome und ihre Eigenschaften* (Breslau: Maruschke & Berendt, 1896), viii.
62. Meyer, *Grundzüge der theoretischen Chemie*, v.
63. Meyer, *Grundzüge der theoretischen Chemie*, 4. On Prout, see ibid., 49.
64. E. Rimbach, *Lothar Meyers Grundzüge der theoretischen Chemie*, 4th ed. (Leipzig: Breitkopf & Härtel, 1907).
65. See Lothar Meyer, Review of Benjamin Brodie's *The Calculus of Chemical Operations*, *Zeitschrift für Chemie*, n.s. 3 (1867): 478–480; Meyer, "Die Natur der chemischen Elemente als Function ihrer Atomgewichte," 354–355; Van Spronsen, *The Periodic*

System of Chemical Elements, 131; and Britta Görs, *Chemischer Atomismus: Anwendung, Veränderung, Alternativen im deutschsprachigen Raum in der zweiten Hälfte des 19. Jahrhunderts* (Berlin: ERS, 1999), 109.

66. Ida Freund, *The Study of Chemical Composition: An Account of its Method and Historical Development* (Cambridge: Cambridge University Press, 1904), 474. For two further examples (among many), see Schmidt, *Das periodische System der chemischen Elemente*, 23 and Stephen G. Brush, "The Reception of Mendeleev's Periodic Law in America and Britain," *Isis* 87 (1996): 595–628, on p. 618.
67. Meyer, *Die modernen Theorien der Chemie* (1876), 3rd ed., 291n.
68. Von Richter, "[Correspondence from St. Petersburg (1870)]," 991.
69. Mendelejeff, *Grundlagen der Chemie*, 692–693n13.
70. Mendeleev's conservatism is discussed at length in Gordin, *A Well-Ordered Thing*, especially Chapters 1, 6, and 8.
71. See the helpful analysis in Mary Jo Nye, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800–1950* (Berkeley: University of California Press, 1993).
72. Bedson, "Lothar Meyer Memorial Lecture," 1409.
73. "*La loi de Moseley justifie la classification de Mendéleeff; elle justifie mêmes de pouce que l'on avait été obligé de donner à cette classification.*" Quoted in J. L. Heilbron, *H. G. J. Moseley: The Life and Letters of an English Physicist, 1887–1915* (Berkeley: University of California Press, 1974), vii.