Political, Cultural, and Technological Impacts on Chemistry

An Interview with Michael Gordin, Director of Graduate Studies of the Program in the History of Science, Princeton University

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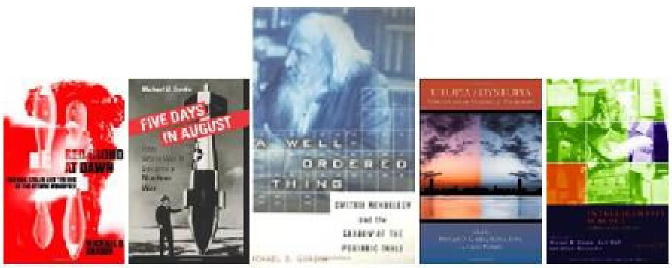
Chemical Information Bulletin, Spring 2011, Vol. 63, No. 1, p. 50-56

Michael Gordin is the Director of Graduate Studies of the Program in the History of Science at Princeton University. He has done extensive research on the history of the modern physical sciences and Russian history. He earned his A.B. (1996) and his Ph.D. (2001) from Harvard University and served a term at the Harvard Society of Fellows. He has published articles on the introduction of science into Russia in the early 18th century, the history of biological warfare in the late Soviet period, the relations between Russian literature and science, as well as a series of studies on Dmitrii I. Mendeleev. His book on the life and chemistry of Mendeleev[1] is considered the most comprehensive and authoritative study published on the formulator of the periodic table of elements. Dr. Gordin has also worked extensively in the early history of nuclear weapons and is the author of *Five Days in August: How World War II Became a Nuclear War*[2] (2007), a history of the atomic bombings of Japan during World War II and an international history of nuclear intelligence, *Red Cloud at Dawn: Truman, Stalin, and the End of the Atomic Monopoly* (2009)[3]. He has also co-edited the four-volume *Routledge History of the Modern Physical Sciences* (2001), *Intelligentsia Science: The Russian Century, 1860-1960* (2008)[4], and *Utopia/Dystopia: Conditions of Historical Possibility* (Princeton, 2010)[5]. He is now working on a history of the modern category of "pseudoscience" in postwar America, from the age of McCarthy to the counterculture, centering on the sensational career of Immanuel Velikovsky (1895-1979), whose 1950 best-seller, *Worlds in Collision*,[6] sparked three decades of controversy over the boundaries of legitimate science. Professor Gordin teaches lecture courses in the history of modern science, technology and society, and translation in the history of science, as well as seminars on nuclear-weapons history, the history of pseudoscience, the Soviet science system, and biography.



Svetla Baykoucheva: *The United Nations has designated 2011 as the Year of Chemistry, and I am very pleased to be able to interview someone who has performed such extensive research in the field of history of chemistry. Your book on Dmitrii Mendeleev1 shows deep understanding not only of chemistry, but also of the socio-political environment in Russia at the time. How does the cultural milieu of an epoch, a country, a region, or an organization influence the developments in science and the public attitude about it?*

Michael Gordin: This is a great question, in many ways it is the central concern of the history of science, and clearly there is no straightforward answer to it. There are many factors that influence the development of science at any particular time and place: the experimental equipment and resources available to the scientist, his or her level of education and preparation, access to communication from other scientists, and the general state of science at the time, to name just a few. Some of these factors are pretty tightly bound with intellectual matters, and some of them are more broadly social or cultural, and I think it would be an error to rule out any particular factor by fiat. In some cases, such as Mendeleev's, the need to reform the pedagogy of chemistry for students in St. Petersburg proved crucial to his creating a framework for organizing the elements which eventually grew into the periodic system we know today. The concerns were both social and political (how do you educate a large number of students who have inadequate preparation) and intellectual (the rapidly expanding knowledge of the properties of elements, especially their atomic weights, in the 1860s). That's not to say we wouldn't have a periodic table without educational reform in Russia — far from it, as we know by the existence of multiple competing systems. Rather, I mean to say that the form we received has a great deal to do with the specifics of that time and place; the content is a more nuanced philosophical matter. The purpose of the history of science is to elucidate all these various factors and point to their relative weights in specific episodes.

SB: *Two of your books (Five Days in August: How World War II Became a Nuclear War[2] and Red Cloud at Dawn: Truman, Stalin, and the End of the Atomic Monopoly[3]) were devoted to nuclear proliferation in the context of the Cold War How do these topics relate to the history of chemistry?*

MG: My colleagues often ask me the same thing. Nuclear weapons in the early Cold War, after all, are indeed a long way from Mendeleev and Imperial St. Petersburg. Certainly as topics they are pretty different, but as ways of investigating the past they are not that far apart. One of the great challenges in writing the history of science is avoiding what we call "Whiggish" interpretations of history; that is, writing a history of the past which leads inevitably to the present, placing the end of the story right there in the beginning. This kind of presentist version of history is very tempting in the history of science, because science's achievements are so obvious, and seem so inalterable. The important point, from the historical point of view, is that they were not obvious to the scientists engaged in making the discoveries. They were beset by uncertainties, alternatives, doubts, and vigorous arguments. It is the historian's task to capture those uncertainties and show the past as it unfolded, not tell a just-so story for the present. Well, after publishing the Mendeleev book, I found myself grabbed by a set of questions concerning the early nuclear arms race, and wanted to see if the same approach would yield results there, even if these weren't, strictly speaking, classic "history of science" questions. For example, in Five Days in August, I focused on how American military officials, politicians, and scientists thought about the atomic bomb in the period before surrender of the Japanese government in August 1945, and especially in the five days between the bombing of Nagasaki and that surrender. At that time, no one could say that the bomb "ended the war," because the war was not yet over, so how did they think about it? Was it a revolutionary weapon or not? And in *Red Cloud at Dawn*, I concentrated on the period between the end of World War II and the detonation of the first Soviet atomic device in August 1949, in order to explore how people on both the American and Soviet sides evaluated the arms race before, strictly speaking, any such race existed. The approach is heavily indebted to the history of chemistry, even if the topics aren't. To be honest, I'm looking forward to returning to more chemical questions now that I have spent all this time with nuclear weapons.

SB: *You are the co-editor of a monograph,* Intelligentsia Science: The Russian Century, 1860-1960, *for which you also wrote an essay on the Heidelberg Circle—a group of Russian chemists who specialized in Germany and who later founded the Russian Chemical Society. Who were these people and what impact did they have on the development of chemistry both in and outside Russia? What motivated them to choose chemistry as a career? What was the role of learned societies at that time?*

MG: Russia entered the decade of the 1860s facing a series of severe challenges. In 1856 it had lost the Crimean War, a defeat which was interpreted by the elite and the intelligentsia as a sign that Russia was "backward" in significant ways with respect to the Western powers. They began to promote a series of military and fiscal reforms in an effort to modernize the state, the most famous of which was the abolition of serfdom in February, 1861. But the problem of *technical* modernization also occupied these decision makers, and they initiated a program to sponsor talented young scientists (and other scholars, like lawyers and physicians) to study abroad, absorb the very latest word in their specialties, and then return to Russia to help rebuild a self-sustaining community at home. And, to a great degree, it worked. Many of the leading lights of Russian chemistry, to pick the example I know best, and those behind the formation of the Russian Chemical Society in 1868, were part of this temporary emigration: Dmitrii Mendeleev, Aleksandr Borodin, Vladimir Markovnikov, and others. Each was drawn to chemistry for different personal reasons, but the choice was in a sense no surprise: chemistry was the most dynamic and exciting science at mid-century, and it was the science most well established in both St. Petersburg and Kazan, which trained these individuals to a level where they could take advantage of their sojourn abroad. As for learned societies, we see a proliferation of chemical societies all across Europe during this time period, and they served a crucial role in creating a national community of scholars who could communicate with each other, establish journals, and lobby their states and national industries for greater support of chemistry. As a step in the professionalization of chemistry, these societies were vital.

***SB:** In a chapter published in the same book, you characterized the Russian national style of scientific discourse as "theoretical, bold, impulsive, and stridently argumentative. It was the style of D. I. Mendeleev and V. V. Markovnikov. It was also the style of Emil Erlenmeyer." Are there national differences in the way scientists perform research and discuss scientific ideas and experimental results?*

MG: Yes and no. At almost any point in the past two centuries (although, interestingly, not so much before then), you can find cases of scientists claiming that their work bears some specific "national style" in a laudatory sense, or that the manner of research of their competitors from another national context bears a deleterious national style. We can easily jot down a number of these crude stereotypes: Russians are impulsive and bold; Germans are nit-picking and meticulous; the French are abstract and conceptual; the Americans are pragmatic and application-oriented. I do not endorse any of these points of view as being *accurate* descriptions of how people really were or are. Instead, in the article you mentioned I point to how certain Russians chose to brand themselves as being bold and speculative; the irony being that the person they were patterning themselves on most was Erlenmeyer, a German. These assertions of "national styles" have been over the years very important aspects of how scientists have understood their own activity, and as such they are significant for the historian to analyze. Some of them — such as the high level of mathematics found in certain chemical communities — can be traced to national educational systems and thus are more likely to bear a relationship to deeper processes, but many of the others are rhetoric. But, at the risk of belaboring a point: just because something is rhetoric doesn't mean it is historically insignificant.

***SB:** Which events and discoveries in the history of chemistry have happened unexpectedly and have become turning points for the development of science?*

MG: This is a great question, and one that opens up a number of very interesting issues about how science has evolved over time. No one would doubt that unexpected events happen in the laboratory all the time — Becquerel leaving his uranium salts on top of some film in a drawer, for example. But it is pretty rare for something *completely* unexpected to happen, since the chemist has a certain collection of equipment and reagents available and is usually trying to accomplish something particular in the laboratory that day. As anyone who has spent any time in a laboratory knows, you don't always get what you expected, but that doesn't mean that the choices you have made have no impact on the set of unexpected outcomes that result. And if something completely unexpected were to happen, one which would have no framework in the concepts available to chemists at the time, then it would surely meet with a lot of resistance, as one finds with the way established chemists objected to the discovery of noble gases. (Mendeleev initially thought argon had to be N_3, since the notion of an element that was chemically inert made no sense to him.) Generally, when an unexpected finding comes along in the historical record, closer investigation reveals that a certain group of chemists made a concerted effort to claim that it was a revolution in the science, and argued for thinking about this "unexpected" discovery as a confirmation of their prior theoretical arguments. This interplay between the serendipity of discovery and the hopefulness of theoretical speculation is one of the wellsprings of scientific creativity.

***SB:** You have taught a course on pseudoscience. What did you cover in that course?*

MG: I find the topic of pseudoscience fascinating, and when I've taught this course I've covered a large variety of topics of things that have been variously classified (not without controversy) as pseudosciences: astrology, alchemy, phrenology, mesmerism, spiritualism, creationism, cold fusion, Lysenkoism, eugenics, and others. In the course, we emphasized what we can learn about how science works from these rejected domains of knowledge. After all, no one calls themselves a "pseudoscientist"—every single person so designated thinks that they are engaged in real scientific work. They don't have to be right about that, but there is a lot of interest in trying to understand them in their own terms.

***SB:** Although scientific fraud is much less seen in chemistry than in the life sciences, cases like the one of Hendrick Schön, from Bell Labs, shook the chemical community several years ago. Schön had published numerous articles before it was discovered that he had submitted the same data repeatedly. Many of his papers had to be retracted, including ones that were published in reputable journals such as Science and Nature. How can scientific fraud be prevented or, once it has happened, punished? Do you consider peer review capable of filtering bad science?*

MG: With regard to Schön, there is an important distinction to be made. On the one hand, we have the category of "pseudoscience," which can be roughly defined as something that is not science but tries very hard to look like science and adopt its methods and approaches. That is not quite the same thing as "fraud," which connotes a level of insincerity that one doesn't find, for example, among seventeenth-century alchemists. (There is a third category, the hoax, which is something else again.) Now, as to what can be done about any of these things, I do not have any particular insights. Wherever you find science, you will find something that scientists label pseudoscience; the two always come together. Fraud, if one subscribes to a particular model of psychology, is a matter of incentives, and it is possible that with intensified safeguards, one can reduce its occurrence. But we almost certainly can't eliminate it altogether. Peer review, as you mention, is often put forward as a solution to this problem, and it is likely better than having no safeguard at all — at least this guarantees that a few scientists read over the piece before it is published — but the evidence of recent years has shown that it is far from foolproof in catching fraud. But, as in the case of Schön, eventually the misdeeds come to light. Time seems to be our best tool in this matter.

***SB:** There are some historians who are very passionate about "The Kekulé Riddle".[7] To chemists, the notion that it was Archibald Scott Couper and not Kekulé who found out that carbon is tetravalent and that it was Johann Josef Loschmidt, who drew the benzene ring for the first time, is quite surprising. What do you think of the claims that Kekulé has received credit for concepts in structural organic chemistry that had actually already been developed by others—such as Couper, Loschmidt, Ladenburg, Frankland and Butlerov? And does it matter, from a science historian point of view, who was the first to make the discovery?*

MG: Being first in making a discovery certainly matters to the scientist! And, in that sense, it does matter for historians of science, since the passions and debates of the scientists are one of the most important things we investigate. Personally, I have spent a lot of time researching the priority dispute over the periodic system between Mendeleev and Julius Lothar Meyer. I am not interested in deciding who was "right" — I don't think historians are in the business of awarding prizes or credit — but the fact that this fight took place, and the kinds of arguments Mendeleev and Meyer used to argue for who was first, makes for a fascinating story to uncover. For better or worse, our system of assigning credit in the sciences centers on priority, and the historian is obligated to explore why that particular system emerged, and what its consequences have been. With respect to Couper, Butlerov, Kekulé, and others — I'm afraid I am a spectator in that historiography and am not going to weigh in on one side or the other, but I can tell you my own particular approach to this kind of question. The fact remains that Kekulé was awarded the credit by his peers. I am personally more interested in why they thought he should receive the credit, rather than in adjudicating whether they were correct or incorrect in doing so.

***SB:** How are the current conditions in academia (I have in mind such things as wider collaborations, struggling for grants, requirements for tenure that include publishing in high-impact journals, pre-prints, open-access, etc.) changing the way research is performed, reported and credited?*

MG: It's generally a bad idea for a historian to speculate on the future, but there is no question that there have been significant transformations in the way of doing science both inside and outside academia that are bound to have important implications for how various disciplines develop in the future. One obvious factor has to do with funding. On the one hand, science is continually becoming more expensive, and there are more scientists competing for a fixed (or in some cases shrinking) pool of funds. On the other hand, the linkages between academia and industry are becoming tighter now than they have typically been (at least in the American context, with which I am most directly familiar), and this is shaping questions that are asked within universities as well as those asked in industrial laboratories. Conditions of publication are also changing in interesting ways. The problem of "information overload" has been with us as long as we have had journals (which is over three hundred years), and probably even longer than that. There is simply so much information for researchers to keep abreast of, so many venues where it appears, and not enough time in the world to track it. Managing this volume of information is a tremendous challenge, and the Internet has both provided tools for addressing this issue and in other ways also compounded the problem. We are seeing strains in the peer review system — exemplified in the use of the pre-print server among physicists, as well as other experiments in open-access — and also mounting costs for libraries. Without sufficient funding for research and access to information, science will suffer, or at the very least be forced to adapt. But I am not a pessimist on these questions. One of the most inspiring things about the history of science is how flexible scientists have been in adjusting to different conditions, and I am confident that while science will look different in thirty years than it did thirty years ago, the developments are going to be quite exciting.

SB: *What projects have you been working on recently? What are you going to work on in the near future?*

MG: I'm now beginning a large research project that connects with your query about the changes in chemistry in recent years. One of the most significant transformations in science over the last two hundred years has been the replacement of a polyglot community with an increasingly monoglot one. To take the example of chemistry, which is the focus of my research, in 1850 a chemist would be expected to be able to read, and to a lesser degree speak, German, English, and French. Today, almost no PhD program in chemistry requires any foreign-language competence at all, as the global production in chemistry becomes increasingly Anglophone. This is an extremely important development, and I believe there has not been enough attention to it aside from a dedicated group of sociological linguists based mostly in Germany. I am planning to write a history that spans from the decline of Latin as a language of scientific communication in the early eighteenth century, through the rise of national languages (including Russian), experiments with artificial languages like Esperanto, the fate of German (almost certainly the most important language in chemistry in the early twentieth century), and the current ascendancy of English. Before embarking on that, however, I am finishing another project related to my interests in pseudoscience as a way of exploring the history of science, with a book on the debates over the theories of Immanuel Velikovsky in Cold War America.

SB: *Thank you for promoting the history of chemistry to a broad audience and for agreeing to discuss these interesting topics.*

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