Facing the Music: How Original Was Borodin's Chemistry?

Michael D. Gordin
Department of History, Princeton University, Princeton, NJ 08544; *mgordin@princeton.edu*

Aleksandr Porfir'evich Borodin (1833–1887) was a remarkable individual. Born illegitimate in St. Petersburg, Russia, to a Russified nobleman from the Caucasus and his maid, he was educated in that city as a medical doctor at the Medico–Surgical Academy (later home to Ivan Pavlov), studied abroad on a postdoctoral trip in Heidelberg (1859–1862), began a career teaching at his alma mater in 1862, lobbied for higher education for women, and along the way managed to compose two and a half symphonies, roughly twenty songs, two string quartets, a symphonic poem ("On the Steppes of Central Asia"), and an unfinished operatic masterpiece, *Prince Igor*. (The music for the latter formed the basis for the score of the Tony-award-winning *Kismet*.) Although much has often been said about the connection between music and the sciences—especially the physical sciences and mathematics—there are all-too-few historical cases that illustrate a practicing scientist who was a significant musical composer. It is the fascination of this hybrid figure that has drawn a great deal of attention to the man, mostly focusing on whether there was some sort of "conflict" between his music and his science *(1–13)*.

This essay does not address that issue of conflict—or, for that matter, whether Borodin's musical reputation is justified vis-à-vis other Russian composers—but, rather, evaluates the basis of the scientific reputation later commentators *(1, 6–9)* have assigned to him by returning to the original sources. The intrinsic charm of Borodin's case has prompted some to argue that his status as a chemist was equivalent to those of his more well-known contemporary countrymen—men such as D. I. Mendeleev (1834–1907, of the periodic system of chemical elements), A. M. Butlerov (1821–1886, of the structure theory of organic compounds), V. V. Markovnikov (1838–1904, of the eponymous rule), or F. K. Beilstein (1838–1906, of the indispensable *Handbuch*). Although Borodin began the 1860s as one of the more promising young chemists of his generation—a reputation based on some striking research while he was abroad at Heidelberg and Pisa—for a variety of reasons his research programs dwindled largely on their own (and certainly not, as has been claimed, due to subterfuge and priority theft by foreigners—although issues of priority were raised and continue to be pressing issues for chemists internationally today). Borodin seems to have left active chemical research owing to a diminution of interest and an increasing desire to devote his time to other issues. The claims for Borodin's status as an exceptional—as opposed to merely good—chemist end not with a bang, but a whimper.

Figure 1. Photograph taken in Heidelberg ca. 1860: Borodin is the second from left, and the seated person third from left is Dmitrii Mendeleev. (Source: Dobrotin, R. B. et al. *Letopis' zhizni i deiatel' nosti D. I. Mendeleeva;* Leningrad: Nauka, 1984; page 65.)

The Heidelberg–Pisa Period

The fact remains, however, that Borodin was very much at the cutting edge of organic chemistry during his stay in Heidelberg and then after his return to Russia for the rest of the 1860s (Figure 1). His productivity while abroad was impressive—a circumstance that was not atypical of Russian students engaged in postdoctoral study abroad and freed from onerous teaching and administrative duties. Borodin's research throughout his career was characterized by two prominent features of this early period: an emphasis on empirical laboratory work on the basic families of organic compounds and a resistance to theoretical speculation. (The second in particular marks him as quite different from the Russian figures with whom he is usually compared.)

Borodin began his career in Heidelberg by quickly settling in at the laboratory of Emil Erlenmeyer (1825–1909)—a *Privatdozent* with a separate laboratory from the much more famous local chemical professor, Robert Wilhelm Bunsen (1811–1899)—to perform research on benzene derivatives, and this research was soon published in Erlenmeyer's house organ, the *Zeitschrift für Chemie und Pharmacie (14–18)*. This work is precise and highly competent and helped to establish his credentials as a laboratory chemist.

When his fiancée, for health reasons, had to travel to Italy, he followed her to Pisa, where he worked in the laboratory of de Luca and Tassinari. This was in many ways a relationship born of convenience, since he could continue his research in close proximity to his fiancée; the originality of his chemical research there had similar contingent origins. When he arrived in the laboratory unannounced to ask for a place to do research, he noticed that it was provisioned with extremely expensive platinum retorts, indispensable for working with highly corrosive substances. Delighted at any visitor expressing interest, the directors granted him broad discretion, and he conducted a series of experiments on organic halogen compounds. As Borodin commented to his supervisors back in Russia about his move to Italy, "Italian scholars have not yet become accustomed to an influx of for-

eigners who arrived for the purpose of working, and, with quite different experiences than the German scholars, they have not yet become accustomed to that system of exploitation which is leading science to the level of a craft" (*9*, p 145). In his most noted work, Borodin reacted benzyl chloride with potassium bifluoride to produce the first (nucleophilic) replacement of a halogen with fluorine. Given that the carbon bond to fluorine is the strongest of its bonds with any other element, and given the difficulty of doing any work with fluorine (owing to its toxicity), this research managed to generate for Borodin a modest but quite respectable reputation among empirical organic chemists. This work was published in Italian—a language Borodin acquired during his brief sojourn in Tuscany—in the *Nuovo Cimento*, but he also sent it to Erlenmeyer so it would be abstracted in the *Zeitschrift*, which was standard practice for Russian chemists *(19–20).* (This reaction has been retrospectively identified with the Hunsdiecker reaction, published in 1939, apparently without knowledge of Borodin's work. The original publication by Borodin was rather cursory and does not elucidate the mechanism clearly, and thus the reaction did not become widely referred to until after Hunsdiecker's publication, which accounts for the failure to make this particular discovery a feather in Borodin's cap until rather late in the Soviet period.)

Interestingly, in his Italian publications, he fluctuated between older equivalent weights (C = 6, O = 8), and post-Karlsruhe Congress atomic weights (C = 12, O = 16), even though he had attended the Congress in September 1860 and Erlenmeyer himself tended to insist on using the "modern" weights in his journal. This offers a further indication that, contrary to Mendeleev, Borodin was not attracted to the theoretical debates prevalent in organic chemistry, and preferred to use whichever was most ready to hand—a fairly common stance *(21).* After Borodin's death Mendeleev would describe his friend's prominence abroad during the Heidelberg years, telling music critic and Borodin biographer Vladimir Stasov that Borodin was "a first-class chemist, to whom chemistry owes much" and that when he (Mendeleev) went abroad, chemists would say, "Well, what new thing has your Borodin done?" *(22).* Although Mendeleev's comments are somewhat unreliable given the lapse of twenty years and the dimming of memory, they do indicate that Borodin began the 1860s as a young chemist to be watched closely.

The Return to St. Petersburg

When Borodin arrived back in St. Petersburg and his post at the Medico–Surgical Academy in 1862, he was well-positioned to follow his chemical mentor Nikolai Zinin (1812–1880) as a central figure in empirical organic research. And so he did, even after his initial meeting with his complementary mentor in music composition Milii Balakirev (1836–1910) and beginning work on his first symphony (completed in 1867).

Besides assuming the reins of the chemical laboratory at the Academy, he undertook the fairly common task of chemical consulting for industrialists and municipalities. In May 1866, Borodin was invited to the town of Khilovo in the Pskov region (southwest of Petersburg) to investigate local mineral water resources and evaluate their possible medical benefits, and he traveled there for an extended stay during 6–17 September of that same year. Borodin had experience in this area. Zinin had recommended Borodin to Russian industrial magnate V. A. Kokorev (best known for his development of the Baku oil fields) in 1858, and Kokorev engaged Borodin to travel to the salt lakes of Soligalich, about 215 kilometers northeast of the provincial city of Kostroma, where Kokorev owned a property and wanted to exploit its supposedly sulfuric waters to open a spa. Although he enjoyed flirting with local ladies, Borodin did not find the region particularly suitable for a Russian Baden-Baden, largely owing to the lack of sufficient expertise to guide treatments on a case-by-case basis: "Here, as with all treatments, first of all one must have in view the general state of health of the patient, because in the strict sense we never treat the *sickness*, we treat the *sick*. By this, it is also impossible to delineate strictly determined rules for the methodical direction of any kind of mineral waters: for each specific case it is necessary to change these rules, taking into account the given conditions" *(23).* Borodin's goal in this early work, as in his later trip to Khilovo, was to establish himself as an expert in practical consulting in analytical chemistry—once again a standard and reasonable approach for a chemist beginning to establish a local reputation to complement his budding international one.

The Aldehyde Project

The period from 1864 until 1873 was the period of Borodin's most ambitious research project—into the nature of aldehydes—and the source of the strongest claims by later writers that he was truly a chemist of equal standing to his illustrious peers *(6, 10, 11).* There are three points at issue in historically assessing this research project: Borodin's original contributions, the role of international priority disputes, and his reasons for abandoning it. The first is less significant than typically supposed, the second is virtually nonexistent, and the third is often misunderstood. All revolve around the issue of priority.

The priority dispute over aldehydes is complicated to unravel, because it unfolded in two stages: one concerning the German chemist August Kekulé (1829–1896) and one concerning the French chemist Adolphe Wurtz (1817–1884). Borodin and subsequent Russian-language literature emphasize the first, German stage—even blaming the fight with Kekulé delaying the completion of Borodin's unfinished opera *Prince Igor* (although that work was incomplete at Borodin's death *fifteen years* after the conclusion of the priority dispute!) *(24)*. English-language historiography emphasizes the second stage. Both were fairly typical cases of individuals working in related areas coming across similar findings, and all claims to Borodin's priority stem from the fact that he was working on aldehydes before Kekulé or Wurtz were, and they should have respected his terrain. Avoiding someone else's area was indeed standard practice at the time, but its bounds depended on the size of one's claimed domain. Was claiming aldehydes as a topic analogous to claiming a specific issue (like the photoelectric effect), or a general area (electrical conductivity)? The dispute—abortive and inconsequential as it was—revolved around this issue more than results.

Kekulé had begun a research program in the late 1860s on the condensation of aldehydes. His first publication on this topic (in 1869) was a rather laconic announcement of preliminary results *(25).* Since Borodin had already published articles in 1864 *(26, 27)* on the action of sodium on aldehydes, he instantly became agitated, writing to his wife on 3 October 1869:

> I just barely avoided an unpleasant run-in on the chemical field with Kekulé, who in one of his works broached the area in which I am working. True, he came to it from completely different beginnings and was completely not onto the same things as I, but nevertheless, in the future course of his researches, he could easily happen upon the same ideas as I did. As a warning of the possibility of a run-in, yesterday I communicated my work at a meeting of the Chemical Society, although the work was far from rounded off. *[28–30]* All the chemists found it, however, very interesting both from the factual side and from the theoretical development of ideas. These days I have been busy from morning to evening working out the literature collected on this subject. [*31*, I, pp 150–151]

Notice how weak Borodin's claims actually were. He admitted that Kekulé was not intruding on his area and did not find the same results, but expressed agitation at what might potentially happen in the future. Elevating this into a priority dispute might seem unwarranted, but that is just what Borodin was eager to do. Given that Borodin's productivity was so low that one could very easily have assumed that he had ceased to work on the topic, Kekulé responded to Borodin's complaints with uncharacteristic amity:

> In each chemical work one sets up for oneself, and today much more than earlier, the danger that the very same research will be carried out simultaneously and independently in other places and by other chemists. Naturally, one is not speaking of the regrettable custom of seizing others' work, namely, that many consider it appropriate that another person's investigations already begun and made known through preliminary announcements can be seized and carried further.
>
> When I some time ago delivered the news that crotonaldehyde is formed through the condensation of aldehydes, I had also set forth another research with valeraldehyde. I had obtained an aldehyde that boils somewhere over 190°, from which one can obtain an acid through oxidation, which after analysis with silver salts arrived at the formula $C_{10}H_{18}O_2$. A short while ago now [Jacques] Riban and Borodin have simultaneously announced that they have begun to work on the same situation, and I will thus provisionally not continue the research with valeraldehye. *[32]*

And so, it would seem, things turned out in Borodin's favor: Kekulé had retreated.

That was not how Borodin read Kekulé's article. On 9 March 1870, Borodin penned another frantic letter to his wife after returning from a Chemical Society meeting, where he learned something very unpleasant:

> Kekulé (in Bonn) is reproaching me that I stole the work on valeraldehyde (which I am working on now) from him (i.e., not the work itself from the factual side, but the idea of the work). He printed this in the *Berichte* of the Berlin Chemical Society. This step forced me to make an announcement in the same place about the facts I have discovered and to show that I have been studying these questions already since 1865 and Kekulé stumbled on them only in August of last year. There's German honesty for you! Although the [Russian] Chemical Society knew all this, I considered it necessary to state it *[33],* so that it would then be communicated, by the established order, to the Berlin Society. [*31*, I, p 201]

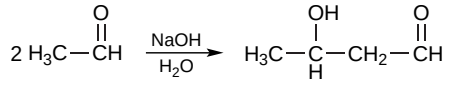
His fury seems to have waned quickly, and Borodin two weeks later wrote to his wife that "[w]ith Kekulé I decided not to answer, but simply to continue the work, and then he will think that I was really scared by his announcement. When the work is finished I will make a note in passing on Kekulé, by the by—that is much more tactful" (*31*, I, p 211). With all the heat of his initial reaction, it quickly became apparent that, in terms of a priority dispute, there was simply no case: Kekulé had not found the same results, and Borodin could not reasonably insist that no one else research in the entire field of aldehydes.

The Issue of Aldol

Nevertheless, his ears were tuned to claims of people stealing his ideas, and he began to react much more quickly. Not that he was publishing articles. It is in fact quite noteworthy that with the turn of the 1870s Borodin's article production essentially dried up to nothing; when he published he did so as abstracts to the Russian Chemical Society, and seemingly never found the time to write up the full articles. One such announcement, in the wake of the Kekulé conflagration, would become the seed of another supposed priority dispute with Wurtz. After a burst of laboratory productivity, Borodin made three announcements at a Russian Chemical Society meeting of 4 March 1872. First, he continued his research on the action of sodium on valeraldehyde—"undertaken with the goal of clarifying the means of appearance of the products of the reaction, described by him already in 1864"—apparently glancing over one shoulder at Bonn. Here he found something interesting: it seemed that an incongruous product emerged that was seemingly an alcohol, but yet had some divergent properties. He described this body's supposed properties without attempting to clarify its structure. He also, in an aside, found that one of the aldehyde reaction products was "obtained simultaneously by Borodin, Riban *(34, 35),* and Kekulé." His second announcement was on the action of sodium on ethanol, and the third "concerns regular aldehyde, from which one also obtains analogous products. A product is incidentally obtained completely similar to that recently described by Wurtz *(36, 37).* The physical properties and conditions of formation are the same," but there was a slight difference in the chemical properties. "However, the circumstantial research of this body is set aside in view of the fact that Wurtz is already studying it, and that its study does not have a direct connection with the rest [of the products outlined above]" *(38).* Borodin conceded priority on all fronts here: he granted to Kekulé equal priority with the earlier product, and graciously backed out of research when he found he was encroaching on Wurtz's. Indeed, to

most organic chemists today—except for the few who have written on Borodin—aldol is credited to Wurtz without mention of Borodin.

Yet this single episode has perplexingly been treated by almost every single writer on Borodin as a case of Wurtz usurping Borodin's legacy *(2–9)*. There are two primary reasons for this. The first is that the finding in question is the discovery of "aldol", so named for having both an aldehyde and an alcohol functional group. In anachronistic modern terms—which were certainly not the terms in which either Borodin or Wurtz was thinking—aldol (3-hydroxybutanal or used as a general term for β-hydroxy aldehydes) is formed by the combination of two acetaldehyde molecules in the following reaction:



(This dehydrates fairly quickly into crotonaldehyde and water.) Aldol is also an important reaction stage of the so-called "aldol condensation". Part of the reason why a priority dispute has retrospectively been staked here is that this was a big prize, and thus vital for a credible claim for Borodin's originality.

There exists a solitary quotation that offers evidence that Borodin was uniquely agitated by the loss of priority over aldol, and this quotation has been replicated in biographical articles on Borodin repeatedly. The quotation, however, is apocryphal. Over a decade after Borodin's death, a former student, M. Iu. Gol'dshtein, published an encyclopedia article on his teacher where he described Wurtz's "theft" of aldol's discovery from the composer–chemist and reported Borodin commenting: "My laboratory scarcely has the means which are [necessary] in its order: I don't have a single assistant, meanwhile Wurtz has enormous means and works with 20 hands thanks to the fact that he is not embarrassed to dump dirty work on his lab assistants" *(39)*. There is no other source corroborating this quotation. The tone does not sound like Borodin, and he made no related or similar comments in his correspondence or to the recollection of any of his peers, including that of his son-in-law, protégé, and successor Aleksandr Dianin. A solitary piece of evidence like this, which so contradicts *all* available original documentation, has to be treated as unreliable. Yet so many have based their interpretations of the man on just this one line. Why? According to Gol'dshtein, it was precisely because of Borodin's continually losing international priority disputes that individuals think of him as a composer primarily and only secondarily as a chemist. These, then, are the stakes: It became important for his contemporary chemists upon his death to see him as great so that they could reclaim his legacy for the history of science, as opposed to merely the history of music. Later commentators have continued this tradition.

Borodin's chemical researches did not stop with the end of his work on aldehydes, although they clearly slowed down. After his disappointment there—a disappointment, again, that had more to do with not being fast enough rather than feeling swindled—Borodin returned to another earlier topic on the reactions of amides. He reported the results of this work in 1873 at the Congress of Russian Natural Scientists and Physicians in Kazan, detailing the transformation of hydramides into bases isomeric with them, following an analogy with August Hofmann's work on the transformation of tertiary amines to secondary and primary amines *(40, 41)*. This Kazan meeting was an important personal moment for Borodin, for it was here, and really only here, where Borodin received public recognition from his peers as one of their most valuable colleagues, based on their evaluation of his recent scientific work and especially on his vigorous activities for women's education, and with barely a comment about his music. As he wrote to his wife on 24 August 1873 from Kazan, "I am surrounded by unimaginable attentiveness, everywhere I meet the most pleased reception, at each step people demonstrate honor and respect with the most flattering signs!" He was later toasted with a cry of "Borodin! Raise [glasses] to Borodin! He is not only a good honest scientist but also a good honest person!" He then found his "fat body" raised in the air and carried about the hall (*31*, II, pp 37–38). Borodin, validated, continued his research on nitrogen compounds, publishing his last original full article in 1875 on this topic, thirteen years before his death *(42)*.

Borodin published very little on the nitrogen research, but his development of a simple but accurate device to measure the quantity of urea in animal urine proved significant. Borodin published a brief abstract in the *Journal of the Russian Chemical Society* in 1876 that described a device consisting of two inverted burets placed inside each other; in the inner buret the urea was reacted with sodium bromide salt and the released volume of nitrogen was measured and thus the quantity of urea accurately calculated, even at small volumes and low concentrations. The finding was so promising that it was reported immediately in the *Berichte* of the German Chemical Society *(43)*. And then, quite simply, Borodin ceased to publish on this topic—or on any other topic, for that matter. Aside from a brief flirtation with hypnotism, reported many years later by the niece of his principal biographer, Vladimir Stasov, essentially all mentions by Borodin and his contemporaries of the man's interest in original scientific research vanish *(44)*.

Conclusion

Despite all the attempts by individuals from the moment of Borodin's death to claim for him an exceptional status in chemistry, one is forced to conclude that his reputation was merely very good, much like many other, less well-known members of the Russian Chemical Society. Had he not also been a distinguished composer—although not, certainly, as innovative as his friend Modest Musorgskii or his acquaintance Petr Chaikovskii—it is likely that his name would be known only to esoteric specialists in the history of chemistry, instead of being invoked in chemistry classrooms around the world as an interesting example of chemical polymathy. Note the ease with which Borodin resigned himself in terms of priority disputes, his nitrogen program, and his urea-detection device. He seemed almost relieved that he would not have to pursue his research further. He established his nationalist bona fides by contesting with Kekulé (however half-heartedly), promptly conceded to Wurtz, and then rapidly faded out of original research. Instead of casting Borodin in the role of original chemist, it is time to recognize that this was not a laurel he particularly coveted for himself. He was

happy to devote the last decade-and-a-half of his life not to the production of science, but to the production of *scientists*, as he engaged in a thorough overhaul of chemical education at the Medico–Surgical Institute and also continued his activism on behalf of medical courses for women—substantial achievements that deserve attention in their own right—and also to the composition of music. Borodin was unquestionably an original man and composer. It is, however, a stretch of the historical evidence to *also* claim for him status as an original chemist. He had striking potential in his youth, but he chose to invest his limited temporal resources otherwise. His reputation stands on the music and his educational activism; his chemistry is at best historically interesting, but not outstandingly so.

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