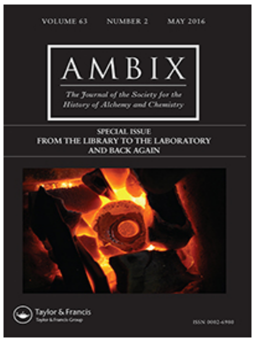

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Chymical Exotica in the Seventeenth Century, or, How to Make the Bologna Stone

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The Bologna Stone, a mineral that became luminescent after chymical treatment, represents one of several "chymical exotica" eagerly sought by natural philosophers of the seventeenth century. Curiously, by mid-century the way to make it luminescent was considered a "lost secret" even though several methods had been repeatedly published. This disconnect between published recipes and experimental failures was explained in part by the investigations of Wilhelm Homberg (1653–1715), later the leading chymist of the Académie Royale des Sciences, and in part by the present author's modern reproduction of Homberg's process. This paper describes both endeavours, and explores the often-overlooked difficulties presented by even "trivial" materials involved in experimentation, and how practical reproduction of historical processes (including visits to important locales) can provide a deeper and more vivid understanding of texts as part of our project to better understand the past. It concludes by reflecting on the importance of maintaining a balance between the material and the intellectual when writing the history of chemistry.

The central place of materials and material production—the making of *stuff*—and the importance of secrecy form two key features of early modern chymistry. These two features coalesce in the case of materials whose origins and/or means of preparation were kept secret. Obviously, such materials might well be objects of regular commerce and manufacture where financial interests, such as maintaining a monopoly, call for secrecy; indeed, similar proprietary interests still enforce a level of secrecy around the production of some special materials by chemical industries today. However, in the early modern period an aura of secrecy also surrounded some materials that were far removed from the usual items of commerce, materials that had no—or at least no obvious—commercial value or utility. I refer here to various celebrated and chymically prepared substances that carried predominantly

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philosophical rather than general commercial value. This value was based on their status as curiosities coveted by natural philosophers and collectors because they displayed unusual properties, exhibited striking phenomena, or promised to reveal something about the hidden workings of nature. Since such chymical materials resemble in several ways the natural and artificial exotica sought by collectors and virtuosi of the period for their cabinets of curiosities, I refer to them as "chymical exotica." The chymical exotica represented a striking union of the natural and the artificial; they were natural in terms of their starting materials and the phenomena they promised to reveal, and artificial in terms of their chymical means of preparation. These materials were rare, and the knowledge of how to prepare them was limited to a small number of chymists, sometimes to just one. Such chymical exotica thus provided an opportunity for trade in both the physical materials themselves and in the secrets of how to make them.

Perhaps the best-known example in the secondary literature of such chymical exotica is white phosphorus.[1] First prepared by Hennig Brand in the mid-1660s, and thereafter publicised by Johann Kunckel, Daniel Krafft, and others, phosphorus quickly evoked the interest of natural philosophers across Europe, and a lively trade in samples of the weirdly flammable and glowing substance soon developed. For many years both its source in human urine and its method of preparation therefrom were kept as guarded secrets, communicated only in exchange for another secret considered to be of comparable value. Knowledge of the secret could also provide social rewards: in 1682, the mathematician and technologist Ehrenfried Walter von Tschirnhaus (1651–1708) was made a member of the Parisian Académie Royale des Sciences after he presented its members with a recipe for making white phosphorus (he had obtained the process from his friend Gottfried Wilhelm Leibniz by negotiating its exchange for two other secrets possessed by the Académie's chymists).[2] The academicians, who were fervently interested in obtaining and experimenting (not to mention *playing*) with white phosphorus, were apparently unable to get Tschirnhaus's process to work, and so had to rest content with buying samples of the material from more successful preparers.[3] Only in 1691 did they obtain a workable process, and did so by admitting a new member who was not only in possession of the secret, but also (unlike Tschirnhaus) had actually successfully prepared the material himself. The disconnect between the Académie's failure in 1682 and its final success in 1691 highlights the difficulty of reproducing

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[1] See in particular, Jan V. Golinski, "A Noble Spectacle: Phosphorus and the Public Culture of Science in the Early Royal Society," *Isis* 80 (1989): 11–39 and references therein.

[2] *Procès-verbaux de l'Académie Royale des Sciences* 10, fol. 104r (1 July 1682; Tschirnhaus's presentation); 9, fols. 165r–166r (4 July 1682; recipe); 10, fol. 104v (8 July 1682; reading of Leibniz's recipe, translated from German by Blondel); Bernard de Fontenelle, "Éloge de M. Tschirnhaus," *Histoire de l'Académie Royale des Sciences* (1709): 114–24; *Mercure galant*, July 1682, 308–09 (Tschirnhaus's admission to Académie).

[3] Tschirnhaus sent another recipe in December 1682, but this too failed or gave at best only mediocre results; *Procès-verbaux* 10, fol. 116v (9 December 1682), fols. 117r–v (16 December 1682), and fol. 118v (30 December 1682). For more details on the fervent quest for white phosphorus at the Académie Royale during 1681–83, see my long-delayed *Wilhelm Homberg and the Transmutations of Chymistry at the Académie Royale des Sciences*, to be published by the University of Chicago Press.

chymical results even when in possession of a recipe, and underscores the crucial role of immediate practical experience when making materials. The importance of such first-hand knowledge—the practical know-how of processes—forms an easily underappreciated dimension not only of early modern chymistry generally, but also of early modern chymical secrecy. Indeed, sometimes the failure to reproduce chymical processes, such as the one recounted in the balance of this paper, were too-readily attributed to missing details that were kept secret when the problem was really a lack of direct practical experience.

This paper deals predominantly with the issue of reproducing chymical results, both in the past by early modern actors themselves, and in the present by historians as a tool for better understanding the past. A chief historical actor here is Wilhelm Homberg (1653–1715), the same character who shared his successful preparation of white phosphorus with the Académie in 1691, and who was unusually well-stocked with both chymical exotica and the practical knowledge of how to make them. The exotic substance in question is the famed Bologna Stone, a material that gained great celebrity long before the discovery of white phosphorus for its ability—when rightly prepared—to glow in the dark after having been exposed to light. The Bologna Stone illuminates multiple issues regarding the nature and notoriety of chymical exotica, the trade both in the materials themselves and in the knowledge of how to make them, the problems of reproducing their preparation and the importance of first-hand practical knowledge, as well as the role of secrecy both real and imagined. The Bologna Stone likewise provides the opportunity for showcasing the utility and value of the experimental reproduction of historical processes as a source of information that can be used in parallel with more traditional sources in order to answer historical questions.

The *Lapis illuminabilis*, or Bologna Stone

The Bologna Stone was a chymical wonder of the seventeenth century.[4] It is recognised today as the first artificially prepared phosphor—or more correctly, the first *persistently luminescent* material—that is, a substance that after initial exposure to light glows in the dark for an extended period of time. Around 1603, a Bologna shoemaker and hopeful alchemist named Vincenzo Casciarolo found heavy, glittering stones in the hills south of the city. Hoping that they contained precious metal or might be useful for making the Philosophers' Stone, he brought them home and began experimenting with them. His hopes for precious metal remained unfulfilled, but his processes—undoubtedly to his great surprise and wonder—made the stones persistently luminescent. He told a fellow aspiring

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[4] For previous treatments of the Bologna Stone, see H. Newton Harvey, *A History of Luminescence* (Philadelphia: American Philosophical Society, 1957), 306–20; Susana Gómez, "The Bologna Stone and the Nature of Light: The Sciences Academy of Bologna," *Nuncius* 6 (1992): 3–32; and Francesca Baldi, *La pietra fosforica bolognese* (Bologna: Bononia University Press, 2013). Gómez's article is especially useful for the eighteenth-century history of the stone, and Baldi's booklet contains a valuable bibliography.

transmuter, Scipione Begatello (or Beccatelli), about this discovery, and news of this "solar stone" or "sponge of light," as it was initially called, began to spread.[5] Casciarolo gave a quantity of prepared stones to the professor of mathematics and astronomy at Bologna, Giovanni Antonio Magini (1555–1617), who then distributed samples widely to other natural philosophers.[6] At least one sample went to Galileo Galilei (1564–1642), who was fascinated by the glowing stones for what they might reveal about the nature of light, and later considered them to be "among the greatest marvels of nature."[7] Members of the Accademia dei Lincei—Federico Cesi, Marc Welser, Giambattista della Porta, Galileo, and others—discussed the stone between 1611 and 1613, but apparently had difficulty in acquiring samples of it.[8] The Jesuit natural philosopher Giulio Cesare La Galla (1571–1624), was the first to mention the Bologna Stone in print in 1612. He described a demonstration by Galileo of the luminescent qualities of the stone and revealed for the first time that the stone as found does not retain light, but rather "it is necessary that it be calcined, or burned skilfully in the fire so that it is turned into a calx."[9] A longer account appeared a decade later appended to the *Pharmacopoea spagirica* of Pierre Potier, a French physician living in Bologna. Potier reported in great detail exactly where the stones are found, even identifying the roads leading out from the Porta San Mamolo that those seeking the mineral ought to take, and described two methods for making the stones luminescent. According to Potier, the native stone can either be powdered very finely and then strongly heated in a crucible, or the powder can be formed into little cakes or pastilles with water or egg white, dried, and then exposed to fire for five hours (Figure 1).[10] Another important early witness regarding the Bologna Stone is the 1634 pamphlet *De illuminabili lapide Bononiensi*—consisting of a pair of letters about the mineral and its properties—published by Ovidio Montalbani (1602–1671), professor of mathematics, physician, and after 1657, keeper of Aldrovandi's museum at Bologna. Montalbani records his own work on preparing the luminous stone, and cites the experiments of his "very dear friend" Carlo Antonio Manzini (1600–1677),

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[5] Very little is known of Casciarolo. He is first mentioned by name in letters about the Bologna Stone published by Ovidio Montalbani as *De illuminabili lapide Bononiensi, epistolae familiares duae* (Bologna, 1634), 3–4 and 5. Some details about the early history of the stone and its initial connection to transmutational experimentation were provided by Scipione Begatello to Pierre Potier and published in the latter's *Pharmacopoea spagirica* (Bologna, 1622), 277–78; Potier does not cite Casciarolo by name, displaying instead a rather contemptuous attitude toward the discoverer who "deserted the cobbler's art for vain chrysopoeia." The change of "Begatello" to "Beccatelli" is suggested by Lorenzo Legati, *Museo Cospiano* (Bologna, 1677), 179, although he himself erroneously cites Casciarolo as a tailor (probably due to a confusion of the Latin *sutor* with the Italian *sarto*).

[6] Montalbani, *De illuminabili lapide*, 5, and Fortunio Liceti, *Litheosphorus, sive de lapide Bononiensi* (Udine, 1640), 12–13.

[7] Galileo Galilei to Cesare Marsigli, 29 August 1626 in *Le Opere di Galileo Galilei*, ed. Antonio Favaro et al., 20 vols. (Florence: Barbèra, 1890–1909), Vol. 13, 338–39. Here and throughout, all the English translations from Italian, French, and Latin are my own.

[8] Antonio Neviani, "Il fosforo di Bologna," *La chimica* 17 (1941): 191–95.

[9] Giulio Cesare La Galla, "Dissertatio de luce et lumine," 57–72, in *De phoenomenis in orbe Lunae* (Venice, 1612), on 58–59 and 71–72. He notes that the luminous stone had also been shown to him by Marco Antonio Prosper, "vir eruditissimus, & naturae arcanorum accuratissimus investigator."

[10] Potier, *Pharmacopoea*, 272–80.

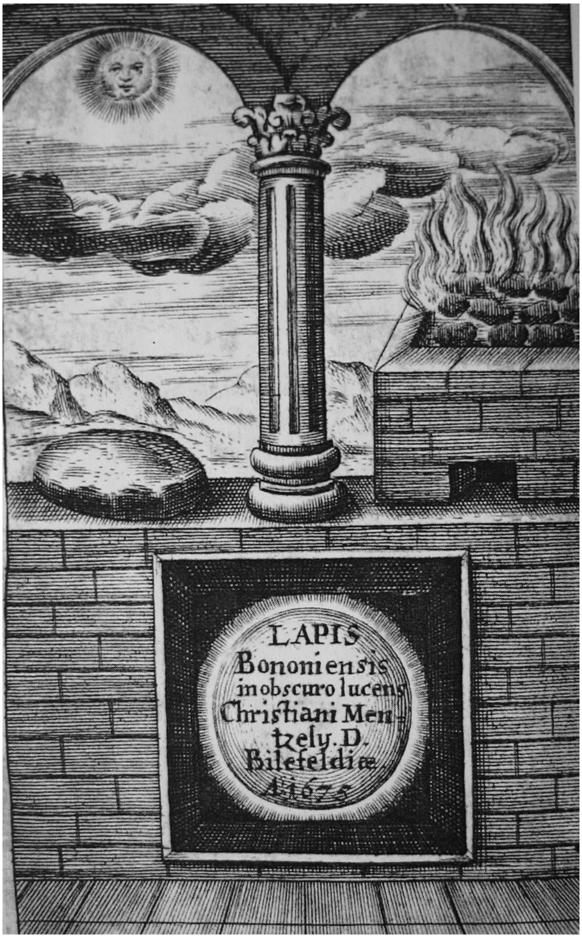

FIGURE 1 Bologna Stones being calcined in an open fire, as illustrated on the titlepage to Christian Mentzel, *Lapis Bononiensis in obscuro lucens* (Bielefeld, 1675).

an astronomer and a student of Magini with interests in chymistry, who "by means of many and painstaking labours prepared for himself a great supply of these stones."[11] This remark is the first (and virtually the only) clear indication that anyone was able to prepare the luminescent stones in "great supply" in the early seventeenth century. While in Italy in 1645, John Evelyn visited Montalbani in Bologna, saw examples of his prepared Bologna Stones and remarked that "he had several" of them. It was probably from Montalbani that Evelyn later obtained the luminous stone that he presented to Robert Boyle (1627–1691). Robert Southwell also obtained both prepared and uncalcined Bologna Stones (again for

[11] Montalbani, *De illuminabili*, 6; note that Manzini is generally referred to contemporaneously as *Mangini*, often leading to a modern conflation with his teacher Magini (e.g. Baldi, *Pietra*, 13).

Boyle) from Montalbani in 1660.[12] Montalbani and Manzini thus seem to have been the most successful early preparers of the luminescent stone, although virtually nothing is known about the details of their practical work save for a few very brief comments made by contemporaries.

Most later accounts of the Bologna Stone rely fairly heavily upon Potier's and Montalbani's descriptions, often lifting entire lines from their texts. The very lengthy *Litheosphorus*, *sive de lapide Bononiensi* published in 1640 by Fortunio Liceti (1577–1657), repeats and reorganises virtually everything that had been written previously about the stone, and augments this material with information Liceti obtained directly from Manzini. Liceti also repeats Potier's two methods for calcining the stone, either as a powder or as pastilles, and adds those of Manzini and Montalbani who calcined the stone whole.[13] These methods of calcination were repeated again by Lorenzo Legati some years later, who noted that Montalbani had himself published some short pamphlets (*fogli volanti*) about the subject—although I have not located any surviving copies of them.[14] Thus it would appear that the process of making the stone luminescent became well-known during the early seventeenth century, since multiple methods of calcination were published repeatedly, and that several people in Bologna became proficient in doing so. Modern commentators, particularly chemists, relying upon the apparently simple character of the chemical conversion needed to turn the non-luminescent mineral into the luminescent material, have generally adopted this view.

In reality, however, by the middle of the seventeenth century it was clear that virtually no one (at least outside of Bologna) could get the calcining processes to work, and the correct method came to be considered a "lost secret." In 1666, for example, the *Philosophical Transactions* lamented "the loss of the Way to prepare the *Bononian Stone* for shining," even though "several Persons have pretended to know the Art of preparing and calcining it." According to "Letters from *Italy* and *France*," the article states, the only person to have the "true secret" of how to prepare it was "an *Ecclesiastick* who is now dead, without having left that skill of his to anyone," and therefore the marvel of the Bologna Stone is now "not like to be found any where, but in Books."[15] In 1668, Boyle wished that he could acquire a fresh stone, and later noted how it had "now grown very rare, even in its own Countrey." The situation was aggravated by the fact that the calcined stone's ability to glow slowly

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[12] *Diary and Correspondence of John Evelyn*, ed. William Bray, 4 vols. (London, 1854), Vol. 1, 193; Evelyn to Boyle, 1 December 1659, in *The Correspondence of Robert Boyle*, ed. Michael Hunter, Antonio Clericuzio, and Lawrence M. Principe, 6 vols. (London: Pickering and Chatto, 2001), Vol. 1, 394–96; Southwell to Boyle, 10 October 1660, *Correspondence of Robert Boyle*, Vol. 1, 428–31.

[13] Liceti, *Litheosphorus*, 120–23.

[14] Legati, *Museo Cospiani*, 179; he also cites Ovidio Montalbani, *Curae analyticae aliquot* (Bologna, 1671) as a further source. Benedetto Mazzotta, *De triplici philosophia* (Bologna, 1653), part 1, 48, asserts that Montalbani is the clearest expositor of the stone and its preparation, a claim that probably depends upon the now-unknown *fogli volanti* rather than upon the practically rather uninformative *De illuminabili*.

[15] *Philosophical Transactions* 1 (1665–66): 375. The article contains a hopeful marginal note that one of the members of the Accademia del Cimento may have "secured this Secret."

deteriorated, and so even well-prepared stones no longer luminesced after a few years.[16] The astronomer and early member of the Académie Royale des Sciences, Adrien Auzout (1622–1691), reported from Italy in 1668 that "the priest who had the secret has died … now no one knows how to calcine it so as to retain light."[17] In 1673, Marcello Malpighi (1628–1694) reported that one person in Bologna (possibly a reference to Manzini) had "laboured strenuously" on how to prepare it, and had shown Malpighi glowing statues and pictures painted with luminescent powder made from the stone.[18] Yet the process remained elusive at best; attempts to reproduce the published manipulations for preparing a luminescent product seem to have invariably proved fruitless. Instructions obtained orally appear not to have been any more helpful: when John Ray was in Bologna in 1664 he visited a chymist named Giuseppe Bucemi who gave him instructions on how to calcine the mineral on a grate over an open fire. But Ray was obliged to remark that "we believe there is somewhat more of mystery in it; for some of us calcining part of the stone we purchased of him according to his direction, it sorted not to make it shine."[19] Even witnessing a demonstration of the calcination could prove inadequate. Christian Mentzel (1622–1701), a German physician educated in Italy in the early 1650s, wrote in 1675 that even though Montalbani himself had demonstrated his method of calcining whole stones to him at Bologna, "still I was not able to make luminous stones from entire stones even though I burned up and consumed much charcoal at Bologna in calcining this stone"; however, he did have somewhat more success with pastilles made from powdered stones.[20] Strikingly, the only authors who claim any degree of success are those who had been resident in Bologna and learned the process directly while there; I have not encountered any report of success achieved by following the published processes. This disconnect underscores the wide gap that exists between the written description of a chymical process and the successful carrying out of that process in practice. Thus the Bologna Stone became not only a celebrated curiosity but also a great rarity; in short, it emerged in the second half of the seventeenth century as one of the most highly coveted of the chymical exotica.

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[16] Robert Boyle, "New Experiments Concerning the Relation Between Light and Air," *Philosophical Transactions* 2 (1668): 581–600 and 605–12, in *The Works of Robert Boyle*, ed. Michael Hunter and Edward B. Davis, 14 vols. (London: Pickering and Chatto, 1999–2000), Vol. 6, 3–25, on 10. The deterioration of the stone's powers is due to the decomposition of the persistently luminescent material under the influence of air and atmospheric humidity, thus the stones Evelyn obtained for Boyle in 1659 and Southwell in 1660 had certainly ceased to luminesce by 1668.

[17] Henri Justel to Henry Oldenburg, 13 February 1669, in *The Correspondence of Henry Oldenburg*, ed. A. Rupert Hall and Marie Boas Hall, 13 vols. (vols. 1–9: Madison, WI: University of Wisconsin Press, 1965–73; vols. 10–11: London: Mansell Press, 1975–77; vol. 12–13: London: Taylor & Francis, 1986), Vol. 5, 401–3. It is unclear to what "priest" Auzout might have been referring, but his report resembles the earlier one in the *Philosophical Transactions*.

[18] Malpighi to Oldenburg, 30 April 1673; *Oldenburg Correspondence*, Vol. 9, 636–37.

[19] John Ray, *Observations … Made in a Journey Through Part of the Low-Countries, Germany, Italy, and France* (London, 1673), 235–36.

[20] Christian Mentzel, *Lapis Bononiensis in obscuro lucens* (Bielefeld, 1675), 61–62. Menzel also notes that Montalbani had published his method, but "I cannot insert that [publication] into this book because I do not have it at hand" — presumably a further reference to Montalbani's now-unknown *fogli volanti*; see note 14.

Wilhelm Homberg and his success with the Bologna Stone

In the last quarter of the seventeenth century the Bologna Stone attracted the interest of Wilhelm Homberg. Homberg would eventually make significant discoveries about the preparation of the stone and learn to render it more reliably and more brightly luminous than anyone had done before. Despite the celebrity he enjoyed during his lifetime, Homberg is not well-known today.[21] He was born in 1653 in Java to a refugee German father and a (probably) Dutch mother. The family returned to Europe around 1660, and Wilhelm eventually studied law at Jena and Leipzig, graduating from the latter in 1676. He began to practice law in Magdeburg, where he met Otto von Guericke (1602–1686), then mayor of the city, and famous for his air-pump, the Magdeburg spheres, and many other wonderful devices. Homberg was apparently fascinated by Guericke's technical and scientific knowledge, and Guericke seems to have encouraged Homberg's interests in return. As a result, Homberg left the practice of the law around 1678 and began his *Wanderjahre* that would continue for much of the next thirteen years. He travelled first to Italy, then back through Germany in 1679 and on to England, then across Central Europe to Hungary and the Balkans, then north to Sweden in 1680. From Sweden he travelled through France, ending up in Paris in 1682 where his accumulated skills, knowledge, and secrets brought him quickly to the attention of members of the Académie Royale des Sciences, with whom he soon began collaborating. After being made known to Louis XIV's minister Jean-Baptiste Colbert, who had founded the Académie in 1666, Homberg naturalised in France in 1683, presumably hoping for a permanent place in the institution. Unfortunately, Colbert died later that year, and Homberg was forced to seek his fortunes elsewhere. In 1685 he returned to Italy, until finally in 1691—in the context of the Académie's revitalisation upon the death of Colbert's successor Louvois—Homberg was made an academician, a position he held until his death in 1715. During his years at the Académie, Homberg became one of Europe's most celebrated chymists and a favoured person with France's royal family.

Homberg's work on the Bologna Stone dates predominantly from his early years. During this time, Homberg was far from the image of the iconic natural philosopher and sober academician that Bernard de Fontenelle (1657–1757), the Académie's perpetual secretary, endeavoured to depict in his "Éloge de M. Homberg."[22] Having acquired Guericke's appetite for the curiosities of nature and his flair for the showy and the dramatic, the young Homberg began collecting and trading secrets and exotica, becoming a merchant of the marvellous. He spent his years travelling through Europe in acquiring natural philosophical and technical knowledge, and in particular amassing—usually by means of negotiated exchanges—a remarkable

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[21] I hope Homberg will be better known after the publication of my *Wilhelm Homberg and the Transmutations of Chymistry at the Académie Royale des Sciences*. The biographical details that follow here, which often diverge from standard accounts, are covered more fully in that book and are the result of substantial new archival findings.

[22] Bernard de Fontenelle, "Éloge de M. Homberg," *Histoire de l'Académie Royale des Sciences* (1715): 82–93.

treasure chest of rare knowledge, technical expertise, and exotic chymical materials. Homberg showed special interest in luminescent substances, an interest Guericke shared. In 1679, Homberg traded the secret of constructing Guericke's *Wettermänchen* (a celebrated type of barometer) to Johann Kunckel for the secret of making the newly discovered white phosphorus, thus becoming only the fourth person in the world with the know-how to prepare the most celebrated chymical marvel of the seventeenth century (He soon after traded this secret to Robert Boyle in exchange for a chrysopoetic secret.). At about the same time, he acquired the secret of preparing the *phosphorus hermeticus*—a persistently luminescent substance made from a local earth and *aqua fortis*—from Christian Adolph Balduin.[23] Later, Homberg invented two further light-emitting substances that bore his name for centuries: "Homberg's phosphorus," a form of fused calcium chloride that emits light when struck, and "Homberg's pyrophorus," a powdery material originally prepared from human faeces (as a side-product of an attempt to transmute mercury into silver) that spontaneously bursts into flames when exposed to air.

But Homberg's very first chymical endeavour seems to have been to uncover the apparently lost secret of making the Bologna Stone luminescent. It is probably for this purpose that he travelled to Italy, possibly having being sent there on the mission by Guericke, who was interested in the Bologna Stone.[24] We have no account of what happened when Homberg arrived in Bologna, probably in 1677 or 1678, but he eventually learned how to prepare excellently luminescent Bologna Stones. When he displayed his prepared Bologna Stones to the members of the Académie Royale in 1687 they found them "incomparably brighter than the ordinary ones."[25] Homberg never published anything about the Bologna Stone on his own; instead, he gave a substantial report on his findings to the apothecary Nicolas Lemery (1645–1715), who published this material in the 1690 edition of his famous *Cours de chymie*. Rather than being a sign of generosity and openness with a prized secret, this action represents part of Homberg's endeavour to establish his reputation in Paris during the time he was angling (with the support of the academicians) for a position in the Académie.[26] The gambit eventually succeeded when Homberg was made an academician the following year.

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[23] On Balduin's luminescent material, see Harvey, *Luminescence*, 321–23; and Vera Keller, "Hermetic Atomism: Christian Adolph Balduin (1632–1682), *Aurum Aurae*, and the 1674 Phosphor," *Ambix* 61 (2014): 366–84. The material was composed predominantly of fused calcium nitrate.

[24] Fontenelle, "Homberg," 84, says that Homberg's purpose in travelling to Italy was to escape his family and to study medicine at Padua, but this claim is probably only the first of several attempts by Fontenelle retroactively to justify Homberg's later medical practice by giving him formal medical training he never had. For the first indication of flaws in Fontenelle's account, see Alice Stroup, "Wilhelm Homberg and the Search for the Constituents of Plants at the 17th-Century Académie Royale des Sciences," *Ambix* 26 (1979): 184–202; for further explanation (and correction) of Fontenelle's errors, see Principe, *Homberg*, chapter 1. Nicolas Lemery, *Cours de chymie*, 7th ed. (Paris, 1690), 658, reports that Homberg travelled to Italy specifically for the purpose of learning about the Bologna Stone.

[25] *Procès-verbaux de l'Académie Royale des Sciences* 12, fols. 60r–60v (7 May 1687) and 38r–39r (28 May 1687).

[26] Nicolas Lemery, *Cours de chymie*, 7th ed. (Paris, 1690), 657–85. See also *Journal des sçavans* (22 May 1690): 230–34 for a special article on Homberg's findings about the Bologna Stone published by Lemery, which contains some details not present in the *Cours*. On Homberg's attempts to gain the Académie position vacated by the recent death in 1689 of Jacques Borelly, see Principe, *Homberg*, chapter 1.

The long-term result of allowing Lemery to publish this material, however, has been to occlude Homberg's role, since later authors have tended to cite Lemery as the leading late seventeenth-century authority on the Bologna Stone rather than presenting him as no more than the editor of Homberg's work. Lemery had previously mentioned the Bologna Stone in the 1683 edition of his *Cours*, remarking then, like others of the period, that

> he who prepared it died without having revealed his secret, such that now no one is to be found who can make it luminous; he calcined it a certain length of time, perhaps according to a method we do not know.[27]

Homberg's most important statements about his work on the Bologna Stone are contained in an unpublished memoir entitled "Experiences sur la piere de Bologne" that he read to the Académie in 1694. There he remarks that "part of the observations which I have made upon this stone have been inserted in the last edition of the *Cours de chymie* of Monsieur Lemery."[28] The material that Lemery published lays out Homberg's procedure in detail. The information about where and how to find the stones is clearly drawn from Potier and Liceti, but Homberg's chymical processing of the stones is completely different. He first cleaned the stones with a file, soaked them in spirit of wine, covered them in the powder made by grinding small Bologna stones, dried them, and then calcined them in a specially designed furnace. The text also underscores that several authors (citing Potier, Montalbani, and Liceti, among others) have described the method of calcination, but these "descriptions are of no use, for they do not succeed at all when one follows what they have written."[29]

A nagging question highlights more about the difficult nature of recipes for chymical exotica. In 1680, the year after Homberg left Italy and ten years before Lemery's publication of Homberg's account, the Roman astronomer and instrument-maker Marco Antonio Cellio published a small book on the Bologna Stone.[30] Cellio describes a calcination process and furnace so nearly identical to Homberg's that it is impossible that the two accounts were arrived at independently (Figure 2). Homberg and Cellio were in fact well acquainted with one another. Fontenelle reports that after Homberg discovered the means of preparing the stone in Bologna, he went to Rome where he spent time with Cellio, learning how to make lenses and other instruments.[31] Other sources indicate that Homberg associated with Cellio at the Accademia Fisicomatematica Romana in 1685–1686 after he left Paris for Rome.[32] So, did knowledge of the preparation pass from

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[27] Nicolas Lemery, *Cours de chymie*, 5th ed. (Paris, 1683), 552.

[28] Wilhelm Homberg, "Experiences sur la piere de bologne," fol. 1r, Archives de l'Académie des Sciences, Paris, pochette de séance 1694: "une partie des observations que j'ay fait sur cette piere ont esté inserées dans la derniere edition du cours de chimie de Monsr. l'Emery."

[29] Lemery, *Cours* (1690), 665.

[30] Marco Antonio Cellio, *Il fosforo, o'vero la pietra bolognese* (Rome, 1680).

[31] Fontenelle, "Homberg," 84–85.

[32] See Principe, *Homberg*, chapter 1.

FIGURE 2 The special furnace for calcining the Bologna Stone. On the left, as depicted in Marco Antonio Cellio, *Il Fosforo o' vero la pietra Bolognese* (Rome, 1680), 103; on the right, as depicted according to Wilhelm Homberg and published in Nicolas Lemery, *Cours de chymie*, 7th ed. (Paris, 1690), 661. Author's collection.

Homberg to Cellio or from Cellio to Homberg, or was there a common informant for both?

It appears impossible to answer this question with certainty. Fontenelle's report that Homberg learned the secret in Bologna before ever meeting Cellio may be correct. Given Homberg's relatively recent turn towards natural philosophy at the time and his lack of experience in practical chymistry, however, it is very unlikely he "rediscovered" or developed the process entirely on his own. It is much more probable that he found someone in Bologna who knew how to calcine the stones and obtained the details from him. Montalbani had died in 1671, but Manzini survived until 1677, so if Homberg went to Bologna in that year he might have met Manzini, although unnamed others, perhaps associates of Manzini, may have possessed a workable process after Manzini's death. According to this scenario, Homberg then moved on to Rome where he met Cellio, presumably to learn lens-grinding and machine-building from him—Homberg had an early interest in astronomy, as did Guericke, so this seems reasonable. Presumably, he arrived with letters of introduction, perhaps from Guericke, but also with a wondrous material and the secret of its preparation, and he may have traded information about the stone to Cellio in exchange for Cellio's mechanical expertise. Having "purchased" the secret in this way, Cellio would then have been free to publish it as his own, and the rules of such an exchange might explain Homberg's subsequent reluctance to publish the same material under his own name. A possible piece of evidence for this scenario is that whereas Cellio merely stipulates the need to cover the stones with a powder made from other Bologna Stones prior to calcination, Homberg actually described to Lemery how he discovered this technique:

> During a voyage that he made while carrying some of these stones, they rubbed against each other and produced a powder that stuck to them in places. When he calcined them without separating the powder, he found after the calcination that the places where this powder had stuck were far more luminous than elsewhere.[33]

A second possibility is that Homberg went directly to Rome, and learned about both lens-grinding and the preparation of the stone from Cellio. A third possibility is that Cellio and Homberg learned the process independently from the same informant in Bologna, and may have merely compared notes and results when they met. Finally, it is also possible, although considerably less likely, that Fontenelle's chronology of Homberg's travels is faulty, and that Homberg learned about the stone from Cellio only much later, either from his book or directly during his residence in Rome in 1685–1686.

Whatever the correct chronology of events and pathway of transmission, there is no question that Homberg experimented and speculated far more extensively about the composition, preparation, and mechanism of action of the Bologna Stone than Cellio, and indeed than virtually anyone else. Lemery is correct to assert that "Mr. Hombert, the German gentleman … has gone far beyond everything which has been published about it."[34] For Cellio, the Bologna Stone is little more than a curiosity, one that he uses primarily to amaze friends and frighten servants. For Homberg it is not only an important part of his treasury of chymical exotica, but also a vehicle for exploring and speculating about the nature of light, matter, and their interactions, topics that would come to occupy centre stage in his fully developed theory of chymistry.

Reproducing the luminescent Bologna Stone: Homberg's discoveries

A more significant historical question surrounding the Bologna Stone has proven more resolvable with the aid of experimental reproduction. Why, despite being published multiple times in multiple venues, was the method of its correct

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[33] Lemery, *Cours* (1690), 666.

[34] Lemery, *Cours* (1690), 658.

preparation considered a lost secret? Modern writers, both historians and chemists, tend to gloss over this issue as if it were unproblematic. Did the publications intentionally leave out a crucial direction or ingredient? Was there necessary but uncommunicated "tacit knowledge"? Were those who attempted to follow the published methods merely poor experimentalists? The answer turns out to involve much more than any of these possibilities, and was revealed only through reworkings of the process—first by Homberg himself in the early 1680s, and then by the present author in recent years. Homberg recorded two important discoveries that bear on these questions in his unpublished 1694 memoir, although he did not apply them towards explicitly resolving the question of the apparent irreproducibility of the published processes. A further crucial part of the problem was resolved without Homberg having been aware of it, and was identified only through my reproduction of his process. The results of this latter reproduction, both in terms of its failures and its successes, reveal much not only about making the luminescent Bologna Stone but also about the material aspects of the practice of chymistry in the seventeenth century and about reproducing experiments in general.

Homberg recounts his two fundamental discoveries about the Bologna Stone in a vivid anecdote about his unexpected failure to reproduce his own previously successful results and about the equally unexpected way in which he discovered the source of the problem. Homberg's anecdote also reminds the historian of the difficulties (and subsequent discoveries) that are sometimes encountered even when historical actors themselves endeavour to repeat their own experiments.

> An accident that greatly surprised me is that after having calcined and rendered luminous a great quantity of these stones in more than a hundred different operations both in Italy and elsewhere, I wanted to calcine in Paris some of the stones that I had brought raw from Italy. But I did not succeed, and I did not know to what to attribute this failure, for it seemed to me that I carried out the operation in the same fashion that I had so often performed it when I succeeded. I repeated this process ten times in a row with the closest attention possible, yet I did not succeed. What chagrined me more was that I had promised to teach one of my friends the method of making the stones luminous, and he was pressing me strongly to keep my word to him. After many excuses, I ran into this friend one day on the street in his neighbourhood, and he led me to his house and showed me some raw Bologna stones and a furnace which he had had made expressly for this calcination according to the design I had given him, all the time seriously begging me to make the stones luminous in front of him. Being thus pressed, I began again the operation which had so often failed, and to speak the truth, I was trembling all the while, for I had not told him that I had always failed at it in Paris. When the operation was finished I found the stones the most brilliant and luminous that I had ever seen. My astonishment was enormous, for I had changed nothing in the operation. These were the same stones as mine, for I had given them to him. After having examined everything well, I found no difference except that in this last operation I used a bronze mortar, as I had done everywhere else, to grind a certain

> powder which is used in the process in place of the iron mortar which I had used in my laboratory in Paris.[35]

Homberg then returned to his own laboratory and tried again using a bronze mortar, and the process succeeded. Not content to leave so surprising a result unexplored, he then undertook an extended set of combinatoric experiments to identify the exact source of both failure and success. He prepared separate batches of the powder by grinding Bologna stones in mortars made of marble, porphyry, iron, bronze, and lead as well as by crushing stones on silver and tin platters and in a copper bowl. After calcining stones that had been covered in each of these powders separately, he found that only the powders prepared in the bronze mortar or crushed in the copper bowl provided luminescent stones. He then took each of the prepared powders and ground them a second time in a bronze mortar—now all of them gave luminescent stones except for the powder that had been initially prepared in the iron mortar.[36] He thus discovered that the least bit of iron—in this case rubbed off into the powder from the iron mortar during grinding—entirely prevented the stones from luminescing: "the tiny particles of iron that are detached from the mortar ... are undoubtedly the cause of the failure." His conclusion was strengthened by his subsequent observation that those Bologna stones he had collected that showed "brown and ferruginous veins or spots ... never become luminous."[37] What Homberg discovered here in the early 1680s is that iron is what is now called a luminescence "poison" or "quencher": only a minute trace of iron, sometimes only .005%, is enough to prevent persistent luminescence.[38] This high degree of sensitivity to iron partly explains the failure of so many attempts. Most workers probably used common iron furnaces or iron grates or iron mortars that contaminated the stones or pastilles. In other cases, workers may have collected the correct mineral, but the particular stones they

[35] Homberg, "Piere de bologne," fols. 3r–v: "... un accidens qui m'a fort surpris, qui est qu'apres avoir calciné et rendu lumineux une grande quantité de ces pieres en plus de cent differentes operations tant en italie qu'alieurs, j'en ay voulu calciner aussy à paris de celles que j'avois apporté bruttes d'italie, mais je ne savois à quoy attribuer pourquoy je ne reussissois pas, car il me sembloit avoit fait l'operation de la mesme maniere comme j'avois fait si souvent lorsque j'avois reussi, j'ay reiteré cette operation dix fois de suite avec le plus d'attention qu'il m'estoit possible, cependent je ne reussissois pas, et ce qui me chagrinoit le plus estoit que j'avois promis à un de mes amis de luy enseigner la maniere de les rendre lumineuses, qui me pressoit fort pour luy tenir ma parole, apres plusieurs excuses cet amis me recontrant /3v/ un jour dans son quartier me mena chez luy et me montra des pieres de bologne crües et un fourneau qu'il avoit fait faire expres pour cette calcination, selon la mesure que je luy en avois donnée me priant fort de rendre ces pieres lumineuses devant luy, estant ainsy pressé je recommencay l'operation si souvent manquée, à la verité en tremblant, car je ne luy avois pas dit qu'à paris je l'avois tousjours manqué, l'operation finie je trouvay les pieres les plus brillantes et les plus lumineuses que j'avois jamais vu; mon etonnement estoit fort grand veu que je n'avois rien changé dans l'operation, et que c'estoient les mesmes pieres que les miennes, car je les luy avois donné. apres avoir tout bien examiné, je n'ay trouvé aucune autre difference, que de m'estre servi dans cette derniere operation, comme par tout ailleurs, d'un mortier de bronze pour piler une certeine poudre qui y sert, au lieu que dans mon laboratoire à paris je m'estois servi d'un mortier de fer."

[36] Homberg, "Piere de bologne," fol. 4v.

[37] Homberg, "Piere de bologne," fol. 4r: "les petites parties du fer qui se detachent du mortier, pendant que l'on pile cette poudre, sont indubitablement la cause de la non reussite"; "il se trouvent parmi ces pieres quelques unes qui ont des veines ou des taches brunes et ferrugineuses, et celles la ne deviennent jamais lumineuses, au moins dans les endroits ou se trouvent ces taches."

[38] Humboldt W. Leverenz, "Luminescent Solids," *Science* 109 (1949): 183–95, on 189; *An Introduction to the Luminescence of Solids* (New York: Wiley, 1950), 333–36.

gathered contained naturally occurring iron contamination in the form of the "brown or ferruginous veins or spots" that Homberg observed. By good fortune, possibly only because it was what happened to be available, Homberg had used a brass grate in his furnace, had formed the furnace from clay, and must have made his first attempts with a bronze or brass mortar.

This result points to the often-unrecognised importance of utilising specific materials, even in the construction of the vessels in which, or the implements with which, chymical processes are carried out. A similar discovery was made during my work to reproduce the early modern *vitrum antimonii* described by Basil Valentine. In that case, the sand incorporated into early modern clay crucibles, and absent from more modern porcelain crucibles, played a major role. Silica, either present naturally in the antimony ore used as starting material or dissolved from the sand in the walls of clay crucibles during fusion, was essential for success. Similarly, I found that Valentine's celebrated medicinal "red oil of antimony," made by "extracting" the prepared antimony glass with vinegar and spirit of wine, was actually an iron compound originating predominantly from contamination by corrosion of the iron utensils he employed during the preparation of the glass.[39]

At the same time as Homberg discovered the pernicious effect of iron, he made a further discovery of equal importance. He observed an equally minute quantity of copper, introduced by grinding the powder in a bronze mortar, and to a lesser extent by using a copper bowl, was necessary for the luminescence. He thus discovered what chemists now call doping: a small impurity, generally of a heavy metal, must be diffused through the structure of the host compound in order to create the phenomenon of persistent luminescence. In fact, we now know that the lingering light (the afterglow) is actually emitted by these impurities as a result of the delayed recombination of electrons dissociated from the heavy metal dopant during the material's excitation when it is exposed to light.[40] By grinding the powder in the bronze mortar for shorter or longer periods of time (in order to alter the amount of trace copper present), Homberg also discovered that while a minute addition of copper enhances the luminescence, larger amounts decrease and then prevent it.[41] The amount of copper dopant actually needs to fall in an extremely narrow range, later determined to be between one part in 20,000 and one part in 5,000, that is, between .005% and .020%; any less and the stone will not glow, any

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[39] Lawrence M. Principe, "Chemical Translation and the Role of Impurities in Alchemy: Examples from Basil Valentine's *Triumph-Wagen*," *Ambix* 34 (1987): 21–30, and *The Secrets of Alchemy* (Chicago: University of Chicago Press, 2013), 141–42 and 240 n. 11. On crucibles, see Marcos Martinón-Torres, "Post-Medieval Crucible Production and Distribution: A Study of Materials and Materialities," *Archaeometry* 51 (2009): 49–74.

[40] Although Homberg proposed a chymical explanation for his observations about iron and copper, he freely confessed that "il n'est pas facile de donner une raison conveincante pourquoy le cuivre contribue à rendre la piere de bologne lumineuse pendant que le fer empeche absolument qu'elle ne la devienne [it is not easy to give a convincing reason why copper contributes towards making the Bologna Stone luminous while iron completely prevents it from becoming so]"; Homberg, "Piere de bologne," fol. 6r. The cause of persistent luminescence was not discovered until the electronic characteristics of solid materials were elucidated in the mid-twentieth century.

[41] Homberg, "Piere de bologne," fol. 6v: "j'ay voulu voir si la piere ne deviendroit pas plus lumineuse en augmentant la quantité de cuivre dans cette poudre, pour ce faire j'ay broyé la poudre plus long temps dans le mortier de bronze … et j'ay observé plus je l'avois broyé long temps moins elle estoit bonne, jusques à ne valoir rien du tout."

more and the stone will not glow. This second discovery of Homberg's further explains the failure of so many earlier attempts: the exact amount of trace copper naturally present in the mineral varies from stone to stone, thus some will contain quantities that fall outside the required range, and will therefore fail to become luminescent. The two facts regarding iron and copper that Homberg discovered in the 1680s were rediscovered independently only at the end of the nineteenth century during the extensive researches of Virgil Klatt and Philipp Lenard on the luminescence of alkali-earth sulphides, and those of Ludwig Vanino and E. Zumbusch specifically on artificially prepared persistently luminescent materials related to the Bologna Stone.[42] The key role of trace amounts of copper specifically in the luminescence of the native Bologna Stone was rediscovered (again) in 2011, and a further electronic mechanism for its action proposed.[43] Homberg's discoveries also explain why, although barite is a fairly common mineral found all over the world, only those samples taken from its occurrence around Bologna can be made luminescent. The mineral can be found there free of iron *and* with the essential trace amount of copper. Had Vincenzo Casciarolo lived anywhere other than Bologna, he might still have found the same heavy mineral, but it would not have become luminescent for him. Clearly, for some materials, being in the right place is crucial.

Reproducing the luminescent Bologna Stone: modern reproductions

Contamination by iron and the need for a very precise quantity of copper impurity were not the only problems that stymied success in rendering the stones luminous. There was an equally substantial problem that Homberg—or whoever first designed the furnace—solved without knowing it. This issue could be convincingly recognised only during work I undertook to reproduce the process. In order to carry out this reproduction, it was first necessary to make the calcining furnace. In the late 1640s, several writers on the Bologna Stone began referring for the first time to the need for a specially designed furnace for calcining the stone. This requirement, and presumably the exact design of the furnace, may have been an innovation of Montalbani or Manzini, the most successful preparers of the stone in the period.[44] Both Homberg and Cellio likewise assert the need for a specific furnace,

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[42] Virgil Klatt and Philipp Lenard, "Über die Phosphorenzen des Kupfers, Wismuths und Mangans in den Erdalkalisulphiden," *Annalen der Physik und der Chemie* 274 (1889): 90–107; Lenard and Klatt, "Über die Erdalkaliphosphore," *Annalen der Physik* 320 (1904): 225–82, 425–84, and 633–72; Ludwig Vanino and E. Zumbusch, "Über die Bologneser Leuchtsteine," *Journal für praktische Chemie* 80 (1909): 69–86, on 73: "Diese Metalle waren, da sie sich oft als geringe Verunreinigungen in vielen Mineralien vorfinden, lange Zeit die unbekannte Ursache der Phosphorescenz"; ibid. 82 (1910): 193–204; 84 (1911): 305–17; and 88 (1913): 77–79. The major findings of the foregoing chemical papers are usefully summarized in Alessandro Bernardi, "Il 'Fosforo di Bologna' o 'Lapis Solaris' o 'Spugna di Luce' e i solfuri fosforescenti," *Annali di chimica farmaceutica* 17 (1939): 1–30.

[43] Mika Lastusaari et al., "The Bologna Stone: History's First Persistent Luminescent Material," *European Journal of Mineralogy* 24 (2012): 885–90.

[44] For example, Bartolomeo Ambrosini, *Ulyssis Aldrovandi musaeum metallicum* (Bologna, 1648), 688: "in furno ad id constructo." Since Montalbani was at the time the keeper of Aldrovandi's museum, he probably contributed to this publication.

and they emphasise its precise measurements and method of construction. Their emphasis on these points suggests that their knowledge may have been transmitted to them ultimately from the same person or source that first began asserting the need for a special furnace—perhaps again something contained in Montalbani's now-lost *fogli volanti* published at Bologna. Cellio and Homberg both provide engravings of their furnaces (Figure 2) and describe their exact dimensions. My reproduction of the process verified that the specificity of the furnace construction was in fact a crucial element for success, as described later in this paper.[45]

Some discrepancies exist between the measurements given by Cellio and those given by Homberg, as well as between the depictions of the furnaces in their engravings, and particularly between the measurements recorded in Homberg's 1687 report to the Académie and the corresponding engraving published in Lemery's *Cours*. Since measurements provided textually are more fully under their authors' control than the making of engravings, I chose to follow the precise measurements provided by Homberg in his presentation to the Académie Royale when he demonstrated how to prepare the stones there in 1687.[46] I acquired the clay from a local (Baltimore) pottery shop. The completed furnace, shown in Figure 3, was 14 inches (35.5 cm) high, not including the dome, and 5.8 inches (14.7 cm) in internal diameter. The difference in its appearance from the Lemery engraving (Figure 2, right) should be noted, as this turned out to be an important detail for the eventual success of the process.

Fortunately, I chose to test the thermal resistance of the furnace before trying to calcine stones. After allowing the furnace two weeks drying time, I followed Homberg's instructions for using the furnace. I placed a few live charcoals on the grate in the furnace to warm it up, and after they were half-consumed, poured unlit charcoal on top, and covered the furnace with its dome. As more charcoal took light and the temperature increased, the section of the furnace above the grate began to shatter, and soon exploded into fragments. Several more attempts gave the same result. Thereupon other types of clay were tried—with advice regarding the differences between clays obtained from local potters and from a colleague in archaeology—and finally I succeeded in producing a furnace shell that could withstand the heat.[47]

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[45] Some later authors deny the importance of a special furnace, see Luigi Ferdinando Marsigli, *Dissertazione epistolare del fosforo minerale* (Leipzig, 1698), 20. The source for Marsigli's knowledge of the Bologna stone presents an interesting question. His elder brother Antonio Felice demonstrated the stones and their calcination to the Académie Royale in Paris in July 1683; only six out of twenty-five stones became luminous after a day of soaking in a solution of saltpeter followed by a calcination lasting many hours: *Procès-verbaux de l'Académie Royale des Sciences*, Vol. 11, fol. 4r, and Cornell University Library, Lavoisier Bound MSS, QD B76++, 6 vols., Vol. 6: "Recueil d'expériences et observations chymiques," 489–90: "Calcination de la Pierre de Boulogne." The method published by Luigi Ferdinando in 1698 differs, being heavily indebted to the Homberg/Cellio method despite his dissent regarding the specificity of the furnace.

[46] *Procès-verbaux* 12, fol. 38v.

[47] My thanks to Professor Marcos Martinón-Torres (University College London) for his advice on producing a resistant clay for the furnace. The final version of the furnace was usable for about five to seven firings before becoming too structurally compromised; there is no record of how reusable Cellio's or Homberg's furnaces were. One difference between Homberg's furnace and Cellio's is that the latter built up the walls of his furnace gradually by smearing wet clay on the interior of a cylinder made from brass sheet (Cellio, *Fosforo*, 79–82), while Homberg makes no mention of a brass sheet mould, and leaves the exact method of forming the clay cylinder unspecified.

FIGURE 3 The special furnace for calcining the Bologna Stone, as reconstructed according to the directions and dimensions given by Wilhelm Homberg in 1687.

The next step was to obtain samples of "Bologna Stone," that is, the mineral barite that occurs in the region around Bologna. My inquiries at mineralogical shops, even one located in Bologna itself, were met with the response that since the Bologna barite is a relatively unattractive grey stone, it does not represent an object of commerce for mineral collectors and is hence unavailable commercially. It was therefore necessary to locate and visit the places where Casciarolo, Potier, Homberg, and others collected their samples of the mineral in the seventeenth century. Fortunately, Potier describes the local roads as they existed in the early seventeenth century and names the best locales for finding the stones; later authors repeat some of this information. The most promising locales of the seventeenth century are cited as Monte Paderno and the areas of Roncaria and Pradalbino. Of course, names and roads change over the course of the almost 400 intervening years, not to mention sites of construction; Roncaria is the place now known as Roncrio, and although no settlement or locale by the name Pradalbino exists presently, there remains a partially unpaved Via Pradalbino running to the southwest of Bologna.[48] The historical authors uniformly cite the area around

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[48] These three sites are repeated constantly by successive authors: Potier, *Pharmacopoea*, 273–74; Cellio, *Fosforo*, 22–23; Lemery, *Cours* (1690), 659. Liceti (*Litheosphorus*, 31) describes his visit, on 20 November 1638, to Monte

Monte Paderno as the best locale to find the stones, and so my search began there. I found the appearance of the exact spot in the area to begin searching hard to conceptualise from the slightly puzzling textual descriptions. Authors uniformly described this spot as a place where the high ground was extremely "fertile with fruit trees, vines, and pastures" while the lower ground, where the mineral is to be found, was "completely sterile." Nevertheless, turning a corner on the Via dei Colli—which is confidently identifiable as the "higher road" described by Potier in 1624—near the Parco di Monte Paderno suddenly revealed an unexpected landscape that fit the descriptions exactly and immediately explained their meaning.[49] Here, as in other locales in the southern environs of Bologna, there exist *calanchi* —steep gullies eroded from the local grey clay—whose walls are completely barren of vegetation, strikingly unlike the lushly verdant and uneroded higher ground (Figure 4). Amazingly, very little construction had taken place in the area and therefore much of the seventeenth-century landscape remains both unaltered and accessible. The subsequent exercise of climbing through these *calanchi* to look for the mineral provided me with a vivid illustration of the warnings about the dangers of treacherous footing expressed by Potier, Cellio, and others.[50] Identifying and experiencing the original locale was thus itself a contributing part of reproducing the process of the Bologna Stone, and like laboratory reproductions, the direct experience of the place provided a better understanding of the historical texts. The physical visit to the original site thus played a significant role in explaining the historical texts and in the overall experimental/experiential reproduction of the Bologna Stone.

Finding the Bologna Stone in the *calanchi* at the foot of Monte Paderno proved somewhat challenging. The clay matrix, the Bologna Stone, and many other stones (which enormously outnumber the desired barite) are similar shades of grey. The several pieces of the Bologna Stone that I found on my first forays initially signalled their presence by the sparkle of their crystals—an observation that aligns with Homberg's suggestion to look for the "petits brilliants" they have on their surface (Figure 5).[51] Nevertheless, several other minerals in the area exhibit more or less similar crystals, but the Bologna Stone distinguishes itself by its much greater density, which is strikingly apparent upon picking it up off the ground. Here again, the visual and tactile experience of reproducing the *search* for the stones gave vivid insight not only into the texts, but also onto why the cobbler Cascariolo's attention was fixed on this particular mineral

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[48] *Continued*
Paderno with Montalbani and Manzini as guides to verify the descriptions of the place given by Potier. Marsigli (*Dissertazione*, 6) erroneously refers to "Piedalbino" rather than Pradalbino.

[49] Potier describes the "superior via" leading from the Porta San Mamolo, and the surroundings of Monte Paderno in *Pharmacopoea*, 274–75. His "inferior via" is certainly the road now called Via di Roncrio. The description of the sterile/fertile dichotomy also appears in Lemery, *Cours* (1690), 659 and Cellio, *Fosforo*, 24.

[50] Cellio, *Fosforo*, 27.

[51] Lemery, *Cours* (1690), 659.

FIGURE 4 A photo showing the stark difference between the sterility of the *calanchi* at the base of Monte Paderno and the verdant vegetation above them, as described in the seventeenth-century texts.

rather than the many other and far more common stones that coexist alongside it and that greatly resemble it.

Back in Baltimore, I prepared the stones according to Homberg's directions. About 5 g of the most transparent crystals were chosen, and ground to fine powder in a bronze mortar. I then chose several whole stones, broken to the size of hazelnuts, which I soaked briefly in *eau-de-vie*, rolled them in the powder repeatedly until they were entirely covered, and set them aside to dry for a day.[52] According to Homberg's directions, I then set up the furnace, threw a few ignited coals onto the grate, and, once the furnace was warm, filled it with unignited charcoal up to the bottom edge of the top vents. The stones were then gently placed on top of the charcoal, further unlit charcoal used to fill the rest of the furnace, and the dome set in place.[53] The charcoal gradually took light from the bottom up, and, after about ten to fifteen minutes, pale purplish flames appeared at the upper vents, and continued there for approximately one hour (Figure 6). After the burn was complete, the furnace was allowed to cool, the dome and upper section lifted off, and I found the calcined stones resting on the grate with a small amount of ash.

On the first operation, only one of the five stones showed any luminescence, emitting an orange light in the dark (strikingly similar to a slowly burning ember) after having been exposed to bright light for several minutes (Figure 7). The afterglow

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[52] I had previously produced the eau-de-vie by distilling wine in an alembic; the distillate was approximately 60% ethanol.

[53] Note that the charcoal used had, according to Homberg's warnings, first been ignited, allowed to burn until it released no more visible smoke, and was then smothered, cooled, and broken into hazelnut-sized pieces.

FIGURE 5 A sample of the Bologna Stone (barite) collected from the *calanchi* near Monte Paderno, in the area between via dei Colli and via di Paderno.

persisted, gradually diminishing, for roughly fifteen to twenty minutes.[54] With experience and multiple repetitions of the process I managed to get about an 80% success rate reliably. A significant improvement came from using up the first bag of charcoal and having to buy a different brand. This second bag proved to be entirely oak charcoal, while the first bag was of mixed woods; the oak charcoal, burning hotter and longer, greatly improved the results. Thus two "trivial" materials, the clay out of which the furnace was constructed and the charcoal used to calcine the stones, turned out not to be so trivial; the right type of clay and of charcoal were important factors.

I also prepared some of the stones into pastilles, according to the method described by Potier and Mentzel. When these pastilles were calcined in a heap of charcoal according to the authors' directions (as shown in Figure 1), with the added feature of avoiding contact with any iron, they failed to become luminescent even after multiple trials. However, when I calcined them in the Homberg furnace, they did become luminous (although they tended to break into pieces spontaneously after a few weeks). Clearly then, Homberg and Cellio were correct to stipulate the use of a specially designed furnace. What's right about this furnace?

The necessary reaction, as we now know from later studies, is the transformation of the native barite, barium sulphate ($BaSO_4$), into barium sulphide (BaS). In chemical terms, this is a reduction reaction, and there must therefore be a reducing agent to remove the oxygen from the sulphate. The chemical "explanations" generally put

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[54] An intriguing and yet-unresolved mystery is that several seventeenth-century observers report the existence of Bologna Stones that after calcining glow *blue* rather than the expected orange (Montalbani, *De illuminabili*, 8; *Evelyn Diary*, 193; Lemery, *Cours* [1690], 659 and 669). Although dopants other than copper as well as other variables can modify the colour of the afterglow, for barium sulphide based phosphors the colour nevertheless remains in the yellow to red range; blue-glowing barium phosphors are unknown (see the range produced by Lenard and Klatt, "Erdalkaliphosphor" [1904], 446–48). It therefore seems likely that such claims are based on the calcination of a different but related mineral—Homberg's description suggests as much (Lemery, *Cours* [1690], 669), as does a remark by Marsigli (*Dissertazione*, 23)—but its identity remains unknown. Calcium sulphide phosphors do typically exhibit a blue persistent luminescence, and abundant calcium as well as strontium and mixed sulphate minerals do exist in the same area (Giorgio Dalrio, *Mineralogia del Bolognese* [Bologna: Caccari, 1980]), but I have not yet succeeded in obtaining a blue-glowing stone. Research into this mystery is continuing.

FIGURE 6 The calcining furnace in action.

forward state that seventeenth-century workers mixed the powdered stone with charcoal as the reducing agent, or assume implicitly that the physical contact of the stones with burning charcoal was sufficient to bring about the reduction. But this idea is drawn from modern chemists' knowledge, not from the texts. It is

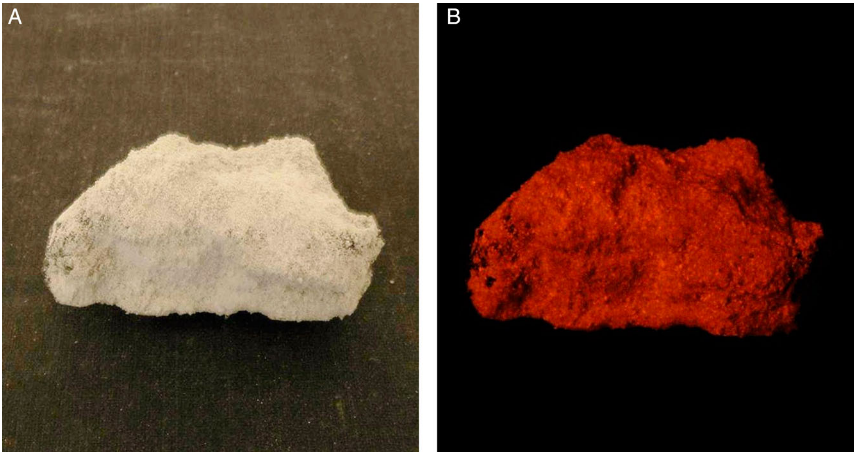

FIGURE 7 A Bologna Stone prepared and calcined according to Homberg's method. On the left, in full light; on the right, glowing in the dark after exposure to light.

modern chemical knowledge—probably based on the standard industrial process of heating barite with coke or coal in order to form impure barium sulphide (commercially known as "black ash")—read backwards onto the original processes in a conjectural and over-confident attempt to say what "must have" happened. I refer to this very common practice as "armchair reconstruction." It makes several assumptions that may be quite unwarranted, the chief of which is that theoretically simple chemical reactions are also simple in practice. There is also often the lurking sense that historical workers and conditions were somehow too primitive to have accomplished anything but the simplest of reactions, and therefore even the most cursory evaluation in the light of "superior" modern knowledge suffices to explain them. (Homberg's remarkable combinatorics with various mortars and his accurate identification of the effects of trace amounts of iron and copper some two hundred years before modern chemists rediscovered these effects should adequately explode that notion.) Modern chemical knowledge is often too easily assumed to outweigh the witness of the original historical texts, and such texts are sometimes ignored entirely or at least not interpreted with the degree of historical sensitivity and openness that historians are obliged to bring to such tasks. While a retrospective chemical analysis can often represent a useful way to gain an *initial* conceptual handle on the historical processes, its conclusions must be considered as no more than provisional until the process is actually reproduced and investigated in practice. In the case of the Bologna Stone, I have found no seventeenth-century text that prescribes grinding the stones together with charcoal, and in my experiments, pastilles made of such mixed material have *never* become luminescent.[55]

The reaction going on in the specially designed furnace is quite different. The pale purplish flames at the vents represent crucial witnesses. These flames exist only in the air outside the furnace and not inside; they result from the furnace effluent, specifically carbon monoxide, burning in air. The furnace described by Homberg and Cellio has vents that are simply too small to allow an adequate supply of atmospheric oxygen to enter the burning chamber fast enough for complete combustion of the charcoal to carbon dioxide. As a result, the combustion within the furnace remains incomplete and instead produces carbon monoxide, which burns further into carbon dioxide only when in contact with the oxygen present in the external air. Note that the size of the vents in the Lemery engraving (Figure 2, right) are depicted far too large in comparison to the measurements given by Homberg, and in fact Cellio underscores the importance of keeping the lower air-vent small.[56] The gaseous carbon monoxide, in direct contact with the Bologna Stone at the furnace's internal temperature of 800–900 degrees Centigrade acts as the needed reducing agent, according to the formula:

$$\mathrm{BaSO_4} + 4\,\mathrm{CO} \rightarrow \mathrm{BaS} + 4\,\mathrm{CO_2}$$

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[55] The failure of a mixture of carbon and barium sulphate to provide a luminescent material, even when doped, was also observed, without reference to historical material, in Vanino and Zumbusch, "Leuchtsteine" (1909), 83.

[56] Cellio, *Fosforo*, 81–82.

The necessary reducing atmosphere rich in carbon monoxide cannot be formed during calcination in an open charcoal fire, or inside a crucible placed on top of the coals, where a free supply of oxygen allows the charcoal to burn completely to carbon dioxide. In fact, at high temperatures carbon dioxide can actually send the reaction in the wrong direction, producing barium carbonate instead of the necessary barium sulphide. I would suggest then that Potier's initial trials, which he claims were successful, must have been done with his crucible or pastilles buried deep in the centre of a very large pile of coals, which were perhaps burning in a place with restricted airflow such as an especially narrow hearth or furnace. In such a case, carbon monoxide sufficient for the necessary transformation could have been produced. Potier did not stipulate these conditions in print, probably because there was little reason for him to think them important.[57]

Conclusions

The conversion of native barium sulphate into a persistently luminescent barium sulphide—although *theoretically* simple—is so sensitive and so subject to so many minute variables that it is no wonder that seventeenth-century workers had so much trouble replicating it, to the extent that most gave up and concluded that the true process was lost. Contamination by iron from various sources, the presence of inadequate or excessive amounts of trace copper, the incorrect temperature from different sorts of charcoal, and the absence of the necessary carbon monoxide as the reducing agent due to unnoticed differences in how the charcoal was burned, all lead to failure. The immediate lesson is that chemical processes, even the "simple" ones, frequently turn out to be far more complicated in practice than one would imagine, and this is often the case because of subtle or unnoticed differences in materials: the wrong kind of clay or charcoal, an iron mortar or grate instead of ones made of bronze or brass. In the case of chymical exotica, part of the reason they are sought-after precious materials is that they are difficult to prepare: their value comes not only from their alluring properties but also from the aura of secrecy or difficulty that surrounds their production and generates their subsequent rarity. Thus in this sense, the more its production is prone to failure, the more precious an exotic substance becomes.

Materials are not simple terms in some historical or cultural equation. They have their own histories, which include the problems of finding starting materials that are both correct and consistent, and developing methods of preparation, many (perhaps most) of which will fail, more often with some practitioners than with others. Crucially, materials cannot be separated from the human beings who handle, prepare, and attach value to them, for the relationship between material and practitioner is a dynamic one that changes both parties. Homberg successfully used chymical

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[57] Paolo Boccone, *Osservazioni naturali* (Bologna, 1684), 229, is the only author to specify that the stone must be surrounded, top and bottom, with burning coals for the calcination to be effective.

exotica for his own social and intellectual advancement, and also by working with them greatly improved their preparation and extracted new meanings from them. The reproduction of material procedures, like that of making the Bologna Stone, carried out both by Homberg in the seventeenth century and by me in the twenty-first, serves as a reminder of how surprisingly contingent chymical processes really are. They are not like simple mechanical physics experiments that rely on readily reproducible and isolable phenomena such as falling bodies or pendula. Because chemical reactions depend upon the behaviours and interactions of countless myriads of invisible entities, they are far more complex, far more subject to minor and easily overlooked or uncontrollable variables. A minute and unsuspected component can significantly change macroscopic behaviour and results. Considering the enormous variation in materials such as purity, particle size, and origin, as well as more obscure factors like scale, climate, and the reactivity of vessels and instruments, it is amazing that anything beyond the most trivial chemical reaction actually works the same way twice. Good experimental chemists are generally aware of this fact. But this awareness should also be part of historical studies that involve chemical materials—it should prevent us from over-abstracting or over-intellectualising chemical work at the expense of its manual, material dimension, or imagining that material manipulations and their difficulties are of relatively minor importance.

Yet there is also danger of going too far. Given the current and growing focus within the history of science on materials and so-called "material culture," there is the potential danger of falling into unnecessarily materialistic and commercial outlooks and conclusions and thereby overlooking or minimising the intellectual. Homberg's chymical exotica had no general utilitarian or commercial value: they were of interest only to a small circle of *curiosi* who valued them primarily for what they could reveal about the hidden workings of nature and for the sheer pleasure they gave to the intellect. These materials—the Bologna Stone, white phosphorus, his pyrophorus and phosphorus, as well as many other substances—all formed part of Homberg's long-term fascination with the interaction of light with matter. Indeed, his interest in this topic led him to focus on collecting, preparing, and studying specifically phosphorescent and luminescent materials. All of these materials in turn played a combined role in directing him towards the formulation of his mature and highly influential theory of chemical combination that he first presented publicly in 1705. In that year, as part of his endeavour to write a comprehensive textbook based on a coherent theory of chymistry, Homberg argued that the sulphur principle was not only the sole cause of chymical reactivity, but was actually the "matter of light" incorporated into otherwise inactive matter.[58] What seems of

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[58] Wilhelm Homberg, "Suite des essays de chimie: Article troisième: Du souphre principe," *Mémoires de l'Académie des Sciences* (1705): 88–96. Discussed in Principe, *Homberg*, chapter 3; Lawrence M. Principe, "Wilhelm Homberg et la chimie de la lumière," *Methodos: Savoirs et textes* 8 (2008), http://methodos.revues.org/1223 (accessed 16 May 2016); and "Wilhelm Homberg: Chymical Corpuscularianism and Chrysopoeia in the Early Eighteenth Century," 535–56, in *Late Medieval and Early Modern Corpuscular Matter Theories*, ed. Christoph Lüthy, John E. Murdoch, and William R. Newman (Leiden: Brill, 2001).

special interest then is how the material and the intellectual *interact*, where the sensual experience of materials meets the intellectual development of theory. For example, reproducing Homberg's process provided unexpected and plausible insight onto how the physical experience of making the luminescent stones may have fed into or sparked the formulation of his mature theory. When reproducing Homberg's process I made an incidental observation that he too could scarcely have failed to make, namely, that when the stones are rightly prepared, they stink of sulphur. In fact, I found that I could accurately predict how brightly a stone would glow based on how strongly it smelled—the more pungent the stone, the brighter the glow—as if the emission of light and the emission of sulphureous effluvia were in fact correlated. Indeed, when the calcined Bologna Stone ages and ceases to luminesce, the odour of sulphur likewise vanishes. This very sensual experience was surely one of the observations that must have influenced the development of Homberg's mature theory about the identity of light and sulphur. Thus the experience of reproducing the experiment pointed towards an unsuspected contributor to Homberg's formulation of his theory. While Homberg did use materials and secrets to collect new treasures—and deployed them wisely to advance his career —these materials were nonetheless firmly situated within his broader intellectual programme. It would be a mistake to consider him as no more than a collector or a showman of marvellous materials.

The modern reproduction of historical processes can provide otherwise unobtainable information for the historian, especially in the realm of chymistry where sensual data and experience play so major a role. The reproduction of such material processes, when properly conducted, should serve to answer particular historical questions. It bears mentioning that reproducing experiments is not a type of antiquarianism or just a "gee-whiz" type of endeavour but rather is a historical tool, an additional source of information to be incorporated into our historical investigations, and one that needs to be deployed and interpreted with the same sensitivity and circumspection that is required for more traditional sources of information. Reproductions should elucidate something about the history of science, in particular how the history of materials interacts with, and indeed is ultimately inseparable from, the history of ideas. It would be counterproductive to swing from over-intellectualising to over-materialising the past. The goal should be to strive to reconnect in our historical narratives and analyses the constantly interacting aspects of mind and hand, of spirit and material, that constitute what I consider to be chemistry's essential nature throughout its long and continuing history.

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