

Horst Bredekamp, Irene Brückle, and Paul Needham, eds. *A Galileo Forgery: Unmasking the New York Sidereus Nuncius*.

Galileo's O 3. Berlin: De Gruyter, 2014. 102 pp. \$70. ISBN: 978-3-11-035464-5.

Reviewing volumes 1 and 2 of Galileo's O in these pages (RQ 65.1 [Spring 2012]: 217–18) I expressed considerable doubt concerning the authenticity of the central object under scrutiny, a now-infamous copy of Galileo's 1610 *Sidereus Nuncius*. This copy, known as SNML for the name of Martayan Lan, the New York company that purchased it in 2005, contained, it was claimed, five sketches of the moon in Galileo's own hand, as well as an autograph inscription and two stamps from the library of Galileo's patron, Prince Federico Cesi. These and other elements were authenticated by Horst Bredekamp and his team of experts in the original volumes, but each struck me as too good to be true. While offering praise for the meticulous scholarship displayed by Paul Needham in the second volume, *Galileo Makes a Book*, whose findings are largely unaffected by SNML's fall from grace, I ended my review with a challenge: "Needham's conclusion nicely reminds us that many individuals were involved in the making of an early modern book: some of them may still be active."

Soon after, and working in conversation with Needham, I uncovered incontrovertible material evidence that the copy was indeed a modern forgery. The forger, Massimo Marino De Caro, confessed to this and other forgeries during his trial for massive thefts from the Girolamini library in Naples, of which he was then director. De Caro is currently serving seven years under house arrest for those thefts, though no trial has been brought, to date, for the forgeries. Much of the story has been told wonderfully by Nicholas Schmidle in a *New Yorker* article ("A Very Rare Book," 16 December 2013), and has also been covered by the *New York Times* as well as the German and Italian press. In short, the review and related research were the first tug on Oz's curtain.

What happens when senior academics are told that the object they have spent years authenticating and researching is, in fact, a fake? I ask the indelicate question not to elicit yet another round of *schadenfreude*, but to encourage discussion on the best course of action, both political and ethical, in academic disputes. Several routes are visible in this awkward volume, *A Galileo Forgery*, volume 3 of Galileo's O, described by the publishers, presumably without irony, as "perhaps without peer in the history of the book."

Needham generously narrates our sometimes fraught, but ultimately productive, collaboration; he goes on to expose the blind spots in his earlier analysis and revisit the lapses that allowed him to reconstruct SNML as a unique proof copy (a category that scarcely existed in the seventeenth century) rather than an anomaly laden forgery. Central to Needham's position is the recognition that historical research, even descriptive bibliography, is always hypothetical in nature, and that fresh conversations and contexts might absolutely overturn an object's ontological status. For reasons I cannot determine, the chapter describing our joint evidence, which in itself provides certain proof of the forgery, is called "Fruitful Doubts," as though bumbling amateurs had happened upon evidence whose true meaning might only be divined by the professionals. In fact, as the strange trajectory of the rest of the volume shows, what was really needed from the original team was not so much a reanalysis of SNML, but one of Galileo's O 1 and 2.

The central question should surely have been, for all those involved in the initial study: under what conditions was it possible for such a clamorous error to have been made? Were the scientific tests correctly conceived or executed? Was the evidence well selected? Was the expertise in and between each discipline of a high enough standard? In each case, though the volume does its best to avoid such direct introspection, the answer is no.

Most revealing is Bredekamp's contribution: his refusal to recognize his own methodological failure is not only saddening, but also counterproductive. To put it bluntly, as has been done in the German press: if one of the world's foremost art historians is incapable of seeing the difference between a 1610 drawing by Galileo and a 2005 forgery, what does that say about art history (or art historians)? Bredekamp's disappointing response is not to investigate and critique his own technique, but to defensively claim that the forgery was so good, even he was fooled. This position is not without repercussions. In order to protect his reputation, the status of the forgery has to be raised. Yet, as Needham argues, the forgery is simply not that clever: we are not dealing with uncanny *doppelgängers* or perfect replicants, just one of several (at least a dozen) attempts to forge well enough to get by. SNML is not a masterpiece, but merely the most hardheaded example yet detected of a series of highly individuated facsimiles produced in order to pass local tests. Only some of these forgeries were destined for the open market: their primary function was to substitute for stolen copies. This wider context is denied by Bredekamp, yet it was precisely through my identification of replications of incidental details between copies that the internal impossibilities of SNML first became apparent. The deeper issue is whether academic study is to be conceived as

a lofty communion between experts and elevated, discrete, isolated material objects, or a socially engaged conversation with other ways of knowing. It was precisely because dealers, collectors, librarians, and criminals were left out of these conversations that the real nature of the object's production and meaning was missed. Stylistic analysis, or perhaps even connoisseurship, is certainly a crucial skill for an art historian, but it is clearly not enough, and must be supplemented by trade gossip, a wide sampling of comparative copies, or rather an adequate network of human and nonhuman informants. Adequate contextualization is perhaps an unhelpful tautology, but the dictum of historians of science that knowledge is socially embedded is prescriptive as well as descriptive. Local cults of academic elitism are perhaps to blame here, and certainly erected barriers in this particular story that prevented early warnings (and there were many) from being taken seriously.

Bredenkamp has long been the champion of a new kind of art history, *Bildwissenschaft*, which seeks to write a nonelitist history of visual culture, embracing in particular the role of the visual in scientific thought. This has been wonderfully suggestive, and Bredenkamp's own contributions span a dazzling range of subjects, from Hobbes to Leibniz to Darwin. Yet in researching SNML, as though aping Galileo's own use of speed and secrecy in publishing the *Sidereus Nuncius*, exclusionary tactics were deployed. The same is sadly true of volume 3, where Schmidle's revealing interviews with De Caro and his location of other forgeries were not admitted as evidence because the book was already in proof stage. Why the rush to produce such a flawed and mumbled nonapology? The answer seems to lie in the politics of German academia rather than a genuine interest in transparency.

Some of the most striking claims in the volume concern the status of scientific tests undertaken for the earlier volumes: paper analysis, for example, now shows that SNML is printed on modern paper, with cotton fibers clearly visible. What, then, were all the graphs and microscopic images doing in volume 1? Here, retrospection is revealing: "In 2006, we had not considered fibre samples of SNML because the originality of the paper had not been disputed. . . . That we had decided against sampling the paper in the first investigation seems puzzling in retrospect, but was a rational choice at the time when both the paper and printing were believed to be genuine, and invasive testing was not warranted" (36–38). This is probably the most damning statement in the volume, an admission that the science of the first two volumes was not actually to test anything, but just to appear scientific, to authenticate with the shimmering aura of the scientific image. Given that Bredenkamp's current goal is to explore the relationship between the scientific image and thought, this is an extraordinary strategy.

In fact, one of the recurrent findings of this volume is that scientific tests on the ink and paper are generally inconclusive. Moreover, even though this was not the procedure deployed by the forgers, it is argued that recycling genuine paper into correctly watermarked new paper would probably pass all known tests. This is one of the volume's most interesting contributions, its tacit admission that what the forgery has exposed is not so much its own status, but also the limits of expertise. Are we, then, at a methodological impasse, where forgeries are unidentifiable?

The answer is a definite no. As the volume shows, what matters is the right approach. Nicholas Pickwood, a new addition to the group, shows in his illuminating and characteristically brilliant chapter on the book's structure that this analysis alone would have been sufficient to cast deep doubt on SNML's authenticity. We now have a clear methodological directive: analyze bindings first, as this is where mistakes are most visible to the trained eye. It's a short-term solution, though, that will disappear once the knowledge gap between binders and Pickwood diminishes.

What, then, do we learn about the techniques used for the making of SNML? Here the volume is actually quite reticent. The only other forged copy to which SNML is directly compared is one of Galileo's 1606 *Compasso* from the Biblioteca del Seminario Vescovile in Padua. The process leading to the detection of this copy (by me, leading in turn to the discovery that the copy of the Biblioteca di Montecassino had been replaced by another forgery) is passed over in silence. I write this not to air my vanity, but to stress that it is precisely the use of and trust in such silences that produced the wrong result in the first place. More serious was the refusal to acknowledge the potential importance of the identification of three forged copies of the *Compasso* in 2006 by Owen Gingerich, two of which were also studied by J. Franklin Mowery. Unless something changes in the way academics work with each other and the rare book trade, all this will happen again.

The technology used to print SNML and its siblings is cheap, accessible, and easy to use. A similar object could be produced by a decent printer for a few thousand dollars, though the Berlin team did not attempt such a simple test. Bredekamp's self-aggrandizing "masterpiece" argument supposes, by contrast, that SNML was an elaborate and expensive weapon in a primarily intellectual duel. This is also De Caro's current argument, but it is untrue. Needham accuses himself of "unconscious collaboration in forgery," and there is a sense in which the proof-copy argument forgave SNML its many sins and obscured the possibility of it being considered a fake. But the more profound socioepistemological error lay in constructing a team around Bredekamp, who had already formed and published his conclusions elsewhere: objectivity was already not so much lost, as owned. A forgery differs from a facsimile not in its mode of production, but in its mode of reception.

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