part 3

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Figure 9. The Flewelling Antiphonary
SECTION 3.1

Fakes and Frauds
The “Flewelling Antiphonary” and Galileo’s Sidereus Nuncius

I. The Flewelling Antiphonary

Doheny Special Collections at University of Southern California has a book (or a book object) catalogued as “The Flewelling Antiphonary.” It is difficult to describe this, so I will provide a photo before proceeding.

It was recently restored and placed in a foam box. This restoration, as far as I can determine, consisted of rebacking (that is, replacing the spine) and possibly resewing of quires, although there are no detailed records of exactly what was done (and no reason such records should have been kept). I wish I could remember what this object looked like ten years ago when I first saw it. I remember that it was not noticeably fragile, and I assume it had not much changed since it came into the library in the 1920s. The question I had then, one I still have now, and one all my students ask when they see this is, “What exactly is this?” and, as a variant, “Is it genuine?” These are much like the questions students ask about the Voynich Manuscript or the Vinland Map. Are they genuine or authentic? Or are they fake? What
is the difference between these two questions, and are they the only alternatives?¹

Provenance

Ralph Tyler Flewelling was a professor of philosophy at USC in the early twentieth century, begetter of the “personalist” movement, which seems to have been complacent, Christian, and opposed to most things German, although steeped in that philosophical culture.² In his ambitions to form an important school of philosophy at USC, he also made many purchases for the library, often in philosophy (2000 volumes from the library of Theodor Gomperz in 1937) but also of early books and manuscripts. I had assumed, looking through the collection, that the incunables (some forty or fifty of them) had entered the library somewhat haphazardly. Most, however, were purchased by Flewelling himself on several European trips in the 1920s and from various local dealers in Los Angeles.³

What is now the “Flewelling antiphonary” is an oddity. It is not mentioned in Nethery’s biography nor in Flewelling’s own autobiographical Forest of Yggdrasill, even though this book is one of his more striking purchases. The book came from a cer-

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² Wallace Nethery, Dr. Flewelling & the Hoose Library: Life and Letters of a Man and an Institution (Los Angeles: University of Southern California Press, 1976), and, more extravagantly, Ralph Tyler Flewelling, The Forest of Yggdrasill: The Autobiography of Ralph Tyler Flewelling, ed. W.H. Werkmeister, with an introduction by Wilbur Long (Los Angeles: University of Southern California Press, 1962), e.g., “We pitch our camp in our symboled Jotunnheim and seek the wisdom of its waters in reflections upon memories past” (19). The Personalist, edited by Flewelling, was published until 1980, when it became the Pacific Philosophical Quarterly.

³ Nethery, Dr. Flewelling, 101–8, and Flewelling, The Forest of Yggdrasill, 88–93.
tain Meynial, whose letter of sale should provide some evidence as to the nature (or perceived nature) of this volume. But to me, it does not:

Monsieur Ralph Tyler Fleweling
Monsieur
J’ai l’honneur de vous informer que la reliure avec ferrures que vous avez vue est du prise de 700 fr.

Veuillez agréer Monsieur mes salutations distingués
M Meynial

The date (which I believe is 1929) is of some importance, since the reports of exchange rates vary from 25/1 in some sources to 1/1 in others. That is, between 70 and 700 USD in the late 1920s, roughly between 1,000 and 10,000 USD today. That range, unfortunately, is almost exactly the range distinguishing “cheap” from “expensive.” And whether it was cheap or expensive might well inform what the word “reliure” means or was meant to mean: was this being sold simply for the clever binding? Or did this suggest a rebound manuscript that was itself “genuine”?

Once this book came into USC’s possession, it seems always to have been regarded with suspicion (or bafflement) by librarians. It was never catalogued until very recently, although this was also true of much of their rare book collection.4 I first saw it in the early 1990s when it was handed to me by a now retired rare book librarian, John Ahouse, who wondered what the library should do with it. Since then, it seems to have gained in prestige: the catalogue now has a “local note” suggesting it is “16th c.” (the binding? the book block?) and it now has a new foam box in honor of a retired staff member.5 Nonetheless, the official main-entry description contains little more. Even today,

4 Until the Incunabula Short-Title Catalogue project, USC’s incunables were more or less identified through whatever typed inscription they had when they came into the library.
5 From USC public catalogue: “Publication info: 16th century Local note: USC Libraries Special Collections’ copy restored in honor of Courtney Suri’s service to USC, May 1, 2012.”
no one seems to know how to describe this, and, as is the case with any book or art work, each restoration removes a bit more of the evidence required for reconstructing its history.

So I know “where it came from” — that is, the last stage of its provenance. On other basic questions, I am on shakier ground, and I find myself looking for experts: on paleography, bookbinding, clasps, vellum, painting, restoration, ink composition, musical notation, the liturgy (etc.). I do not have the competence to offer definitive judgments on any of these subjects. I certainly know those who could, but having dealt with many such experts in the past, I have learned that a good many of them pronounce with equal assurance on things they know well and things about which they are uncertain. You don’t get the reputation of expertise by saying too often, “I really have no idea.” (The default response seems to be, “I’ll get back to you.”)

I look at this book: I see bejeweled binding, probably visually “like” other such bindings (from where?) with, to me, extraordinarily large clasps in far better shape than others I have seen. At first glance, I would say this was a binding manufactured for some early film on Robin Hood. The vellum is discolored, and the colors unlike those I have seen elsewhere….I dream of having the radiological equipment that was used in the case of the Sidereus Nuncius (see discussion in part two of this section) to confirm whatever conclusion I might draw from this.

I would like to know more about the music notation in the manuscript, since I do not know whether it can be read at all. But this question too is not an easy one to deal with: to argue that the music is genuine (whatever that means) would likely require the assumption that it was copied from some other source (a manuscript of this same text?) and therefore reproduces an earlier representation of music which the manuscript creator may or may not have understood.

The same thing might apply to the writing, but here at least I have a few material notes to go on. The writing misuses the convention distinguishing round r from ordinary r (but so does, say, early printing). There are also numerous errors in Latin: p for the abbreviation pro-, for example. But none of that is pe-
period specific; scribes could misuse or misread or misunderstand Latin and basic writing conventions at any period of history. More amusing, comparing the opening to the later pages, it is as if one were viewing a summary history of medieval writing, from legible, late Carolingian (with clear letters, marked right slant), to a more formal textura. What we have is the record of a “scribe” (or workman or young child) working on imitating a textura hand during a period when no one was trained to write it. This version of paleographical history is constructed in a teleological fashion with textura as its goal, as if there had been no Renaissance or Enlightenment. But all this of course cannot be the case; the creator of this book was not a serious student of paleography. It’s simply a projection of the way I am accustomed to view any series of differing hands that are used in real or faux medieval works.

I turn back to the minimal catalog notes, and see that what is dated as the sixteenth century is the manuscript, not the binding, and it is said to have possible eighth- or ninth-century illustrations. The first time I looked at this record, there were no notes available, but I state that only from memory. I distinctly remember that year (or was it two years later?) remarking in the presence of someone (who?) that those full-page illustrations were not characteristic of late liturgical manuscripts and that these were...did I say “like?” “in the style of?” illustrations from Carolingian manuscripts? Looking at this book today, I cannot help but wonder whether that remark is now embedded in this description. But the word “today,” even in that last sentence, is a flexible thing: this very paragraph is based on notes taken in 2013/14. “Today” (now meaning “the summer of 2017”), all that remains is what I quote in footnote 5 above; the earlier notes have been expunged. And for good reason: for these illustrations are nothing at all like Carolingian ones. The figures are vertical; the faces are staid, completely unlike the curvaceous and brooding faces one finds in Carolingian manuscripts or their reproductions. They are reminiscent of the stodgy, spare reproductions of medieval manuscripts found in various publications of T.F. Dibdin. They copy something. And maybe that source was itself
“genuine” in some sense. But the same thing could be said not only of late copies and reproductions, but of the vast majority of manuscript paintings and miniatures themselves.

II. [Galileo’s] Sidereus Nuncius

About a decade ago, a copy of Galileo’s Sidereus Nuncius came on the market. The Sidereus Nuncius is not a rare book: some eighty copies are known. But this one was unique in that it contained or purported to contain Galileo’s signature and illustrations of phases of the moon drawn by Galileo himself. In other copies, these are etched; some ordinary paper copies leave blank spaces where these illustrations were to appear. The book became the subject of a number of scholarly projects; the most important was a two-volume work edited by Horst Bredekamp (2011). The Bredekamp volumes gave the scientific and bibliographical evidence authenticating the book; numerous skeptical studies and early reviews, as well as a 2013 New Yorker article by Nicholas Schmidle, provided the counterargument. Following the New Yorker article, a third volume of the Bredekamp project appeared (2014), renouncing the conclusions reached (or assumed) in the first two.6

What I am concerned with here is not the authenticity of the volume, but rather the process of authentication. The crowning achievement was Bredekamp’s 2011 *Galileo’s* O, involving a number of technological institutions with formidable titles, and some of the best scholars I know. Their verdict, based on paper analysis, radiological analysis of the ink, bibliographical data, handwriting analysis, and binding was that the book was genuine, even though it required a great deal of ingenuity to make that case. The rhetoric of certainty in the Bredekamp volume is striking:

This collaboration between specialists of various institutions, exemplary in its unbureaucratic and precise interplay....

Through comparison of the NY copy with the two other paradigmatic copies from Graz and Washington and numerous other copies, a level of knowledge has been gained previously achieved only for the Gutenberg Bible...

The New York copy was never removed from its seventeenth-century binding; it consists of proof-copy paper; it is the proof copy of the printed book; it has the stamp of Federico Cesi’s personal library on its title page and elsewhere in the text; the title page carries the signature of Galileo; its writing and drawing materials are typical of the early seventeenth century; the black material on top of the drawings indicates a transfer process to the copperplate, and the style of the drawing is the same as that of Galileo’s sunspots and the Florentine drawings.

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7 Stiftung Preussischer Kulturbesitz; Kupferstichkabinett Berlin; Staatliche Akademie der Bildenden Künste, Stuttgart; Bundesanstalt für Materialforschung und Prüfung, Berlin; Technische Universität Berlin; and Rathgen-Forschungslabor.

8 Bredekamp, *Galileo’s* O, 1:11, 1:15.
This confidence is likely as much a product of editing as of the certainty of individuals, and scholars should not be criticized simply for stating their opinions in the strongest, most direct form. Yet even here, the shaky logic that led to the conclusions is visible in several rhetorical sleights-of-hand. For example, the book is in *Sammelband*, that is, bound with other contemporary books in a seventeenth-century binding. That the book “was never removed from its seventeenth-century binding” seems, when expressed that way, to confirm its authenticity. But those who claimed this, mis-described the evidence and did not formulate the correct question. It obviously does not matter whether the book was ever removed from an early binding; all that matters is when it was put there. In a similar fashion, those who investigated the ink concluded the ink of Galileo’s supposed signature matched the ink of the drawings. But this again is the wrong question. It did not deal with the more important question of what the date of that ink was, and even more simply why it differed from other examples of seventeenth-century ink. In addition, some of those scholars (not all of them) who invoked paper evidence claimed their conclusions “matched other historical evidence.”\(^9\) Yet that “historical” evidence was not always from one of the impressive scientific organizations involved in the study; it was often purely aesthetic.\(^10\) In 2011, even though many of the scientific instruments seemed to agree, the basis of that agreement seems now to be shared error.

This copy is unique in a number of bibliographical ways. The type impressions are deeper than in any other copy, the paper

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9 Ibid., 1:127.

10 See, e.g., ibid., 1:38: “Each element of the inscription in the New York copy, including its whole ambience, is so inseparably connected to Galileo’s style of writing that its authenticity is beyond doubt — No counterfeiter could have imagined it.” Even in 2014, lapses in logic were not conceded: “on the one hand, nobody denied the logical rigidity of the newly presented facts. On the other hand, the conclusion that the SNML was authentic had been founded on such a firm basis that it seemed unimaginable suddenly to change one’s mind” (ibid., 3:10).
differs from that in any other copy. At times, such contradictory evidence is cited as if it were supportive:

In fact, the watermark shapes found in the sheets of the NY copy differ sufficiently enough from those in the regular copies to suggest that they stem from separate twin moulds.11 (They were “in fact” made from the same modern mould.) That very uniqueness, which should have raised suspicions, somehow contributed to the aura of authenticity (and of course value as well): because it was different from all copies known to be authentic, therefore it was even more valuable and thus by implication more authentic.

The most intricate explanation of these differences (given by Paul Needham) was that it was a proof copy; this conclusion, unfortunately, could both support the nature of the authenticity of the book as well as explain away any differences found between this and the other genuine copies of the book.12 In order for such a copy to exist, it would have to result from a procedure such as the following: the printers created proofs sheet by sheet as they ordinarily would during the printing process, and the entire print run was printed off without illustrations. They kept these sheets, collated them, and gave them to Galileo, who then drew in the designs, and gave them back to the printers to be the basis of the engravings or etchings. After these illustrations were printed in most copies of the book, all these proofs were gathered up and given back again to Galileo. Did he sign it then? From there, it found its way into the modest seventeenth-century binding that contains it today. I believe I am summarizing this false argument correctly, but I am not certain of that. It is never explained with clarity and, given what are thought to be

12 These would be among the rarest of things in book history; Needham cites two others. I’ve never seen them. There may well be more. Needham, Galileo Makes a Book, 173–74.
the standard procedures of seventeenth-century printing, this would be difficult in the extreme to accomplish.\textsuperscript{13}

The explanations are ingenious, but some of the arguments are easier to refute than to understand. The evidence concerning type impressions is basic. The depth of the impressions is clearly visible even in the reproductions provided in Bredekamp’s volumes.\textsuperscript{14} The way you fake a letterpress leaf is simple, as a Boston book dealer once explained to me. You go to the Widener or Houghton and get a photo of whatever page is missing from your copy. You then make from that a relief plate, and you print a copy from that using the ordinary procedures of letterpress printing. This particular dealer completed books in his shop by using stray seventeenth-century paper (this is likely why so many seventeenth-century books in reputable libraries lack blanks; see the various copies of Suckling’s \textit{Fragmenta Aurea} noted in chap. 6 above). The Galileo forger could have followed this standard procedure, but instead, he simply made his own paper; that way, at least, the paper would be consistent (the paradox is that genuine seventeenth-century paper that did not match would raise more suspicions than forged, twentieth-century paper that was consistent).

In standard letterpress printing, there is a lot of stray ink. Those who make a photographic copy either clean this up or they don’t; that is, every area in the photograph shows black,

\textsuperscript{13} The procedure is easier for us to accept, since the notion of book-length proofs seem quite natural to us. But type was not kept standing through a complete print-run in the seventeenth century (typecases did not contain enough type to make this possible). See the contemporary description of the proofing process in Joseph Moxon, \textit{Mechanick Exercises on the Whole Art of Printing (1683–4)}, eds. Herbert Davis and Harry Carter, 2nd edn., (London: Oxford University, 1962), 249. Retaining a complete “proof-copy” for most printing projects would have been pointless.

\textsuperscript{14} I had once assumed that “faked” leaves in early books were lithographic, and that genuine leaves would have the greatest “three-dimensionality.” The only leaves I have seen so deeply impressed as the ones in the New York \textit{Sidereus Nuncius} made me revise this erroneous opinion: they were forged leaves tipped in to a fifteenth-century St. Thomas. Needham’s explanation is that the leaves, being used for proof only, were not sized, nor were they pressed together after collating in a book block.
or it does not. There are no shades and all areas to be printed in black will show impressions of equal depth, since the plate-making process interprets each area of black the same way. In an original copy, stray or unprinted ink sits on the surface, leaving no impression; if this is reproduced in a forgery, this ink will be deeply impressed. The telling detail in the Galileo involved stray ink left by the shoulder of type sorts at the edge of the forme. In an original copy, these marks (if they were there) would be lightly impressed; in the forgery, they are as deeply impressed as everything else.

A second piece of evidence concerns supposed press-variants and corrections, this one involving a broken typesort. If a bibliographer can identify a particular typesort, it is possible to follow its history through the printing process. I’m skeptical about this, since those typesorts are not as clearly defined as is claimed by the sometimes enthusiastic researchers who have found them.15 And I imagine a similar skepticism must have pervaded Needham’s mind, who did not notice that the typesort evidence he had painstakingly assembled absolutely contradicted his proof-copy theory.

A broken sort is visible in three places in all copies of the book. Yet in the proof copy, there are no broken typesorts. If there were only one instance of this, it would be easy to explain: a sheet of the proof copy was printed, a typesort then broke, and the broken sort appeared in all other copies of that sheet. Here, that same broken sort re-appears in subsequent sheets; it should, by all rights, appear in the proof copy sheets as well (since it was presumably created during in the printing of the first sheet in which it appears). But it never appears in the proof copy. You would have to imagine a perfectly good sort printed clean in a sheet of the proof copy; it then was damaged; when that same broken sort appears again, it is a second sort that happened to

be damaged in exactly the same way following the printing of the second perfect proof sheet. It was then replaced, and after the third proof-sheet was printed, remarkably enough, it broke again, precisely as it had after printing earlier sheets of the proof copy. Obviously, this explanation is absurd: the broken typesort is the same in all three places. The reason it appears unbroken in the forged copy is that that copy was made from the Graz facsimile and in that facsimile, the impressions of this broken sort (which appears in all other copies) have been retouched.

The Retractions

Paul Needham’s extensive “Final Thoughts” in the third Bredekamp volume is a model of scholarly introspection: to understand how errors are possible requires understanding the process by which they took place, which often is detailed, personal, and not at all in the genre of evidence allowed by most scholarly discussion. But Needham’s introspection is not shared by all contributors. The same rhetoric of certainty I noted earlier pervades Bredekamp’s volume of retractions as well, where several of the scholars who confidently authenticated the Galileo copy now condescendingly take the forger to task for misunderstanding the details of, say, paper construction, and for arrogantly assuming he could fool professionals like themselves (Bredekamp’s volumes 1 and 2 prove, of course, that he had done just that!):

Given the effort of making of the forged paper, what led the forgers to use a fibre [cotton] that was all but unheard of in the seventeenth century? A lack of real understanding of his-

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Historical papermaking and a confusion of terminology are two probably explanations.\textsuperscript{17}

Professional hand papermaking requires expert skills, even though a reasonable sheet can be made after a short introduction to the process. We can safely say that the forger did quite well in this respect. To make a sheet that looks old requires more specialized knowledge, and here the forger became creative. He closely matched the laid and chain pattern of the mould surface with respective watermarks. But to fully imitate a stock as it would be found in a historic book requires more knowledge than single sheet forgery, and this is where the forger made mistakes.\textsuperscript{18}

The poor forger! Think what he could have done had he had the same modern scholarly knowledge and expertise that led to the authentication of his work only three years earlier. In other sections, scholars are more circumspect:

The forgers’ knowledge of which areas of a book should be dirty was originally convincing….Awakened, as our eyes were in the latest examinations, we saw what was unnoticed before. The surface soiling through is the result of a manual manipulation of the page surfaces.\textsuperscript{19}

Finally, the forger's motives come into question:

The fake restorations were probably done in an attempt to distract from the crude look of the forgery.\textsuperscript{20}

\textsuperscript{18} Ibid., 39.
\textsuperscript{19} Ibid., 58.
And from there, a new set of conclusions is formed. The forger has met his match, and that must have been what motivated him. Because finally, this isn’t about making money, and it isn’t about the challenge of making an object of art; like everything else, it is all about us:

Taking all elements together, it seems as if the forger…was working against a fictive enemy, an enemy that might incorporate the combined knowledge of specialists. It is our thesis that the book is a projected duel with the community of specialists. The hidden agenda of the making of the book might have been a clandestine satisfaction regarding the incapability of specialists to detect the forgery as such.21

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