

## ANALYSING THE VINLAND MAP: A CRITICAL REVIEW OF A CRITICAL REVIEW\*

K. M. TOWE†

*Smithsonian Institution, Department of Paleobiology, 157 Broadlands Drive, Eatonton, GA 31024, USA*

R. J. H. CLARK

*Christopher Ingold Laboratories, University College London, 20 Gordon Street, London WC1H 0AJ, UK*

and K. A. SEAVER

*3638 Bryant Street, Palo Alto, CA 94306, USA*

*In an attempt to validate the Vinland Map as a genuine 15th century work, Garman Harbottle (2008) claims to review critically, and thereby discredit, the archaeometric research undertaken on the Map's ink by Walter McCrone (1974–1999) and Brown and Clark (2002). In this paper we outline and discuss the numerous arguments that are presented by Harbottle, all of which we find to be misleading or at best highly questionable.*

**KEYWORDS:** VINLAND MAP, MEDIEVAL PARCHMENT, CALCITE, SYNTHETIC ANATASE, IRON GALL INK, CARBON-BLACK INK, TITANIUM DIOXIDE, CLAYS, FORGERY

### INTRODUCTION

Garman Harbottle (2008) claims to validate and vindicate the controversial Vinland Map. Presented as an objective critical review, his paper is a forthright but seriously flawed and misleading attempt to test and invalidate the important chemical and mineralogical evidence of Walter McCrone (1974, 1976, 1988, 1999) as well as that of Brown and Clark (2002). At the heart of his review is a totally new conjecture that the calcium and titanium in the ink particles studied by McCrone, and later by Brown and Clark (2002), represent calcite–anatase ‘composite crystals’. This is an entirely novel conjecture because McCrone showed the two minerals to be a simple mechanical mixture (McCrone 1988, fig. 3). He considered them brought together from separate sources with the calcite completely unrelated to commercial titanium pigment production. Harbottle (p. 178) even quotes McCrone (1974): ‘the particle was identified as a *mixture* of calcium carbonate . . . and anatase’. Harbottle thus falsely attributes this new composite crystal conjecture directly to McCrone and then, in ‘straw-man’ fashion, attempts to dissect and eliminate it. He offers all this as invalidation of McCrone’s chemical evidence, asserts that McCrone’s arguments are ‘irrelevant to the map’s authenticity’, and concludes by stating ‘we are no longer encumbered by the suspicion that the Vinland Map has been proven to be a forgery’. Harbottle asserts that the measurements of the Cahill group (Cahill and Kusko 1995) ‘convert a major chemical proof of forgery into a minor chemical curiosity, one

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†Corresponding author: email towek@si.edu

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that is quite irrelevant to the main question of authenticity'. These ideas and statements should not go unchallenged.

#### INVALID POINTS

Harbottle argues that McCrone was wrong to say that pure anatase (99% TiO<sub>2</sub>) was commercially available in the 1920s. He asserts that the industrial anatase of the period must have had barium sulphate extenders in it (e.g., Titanox-B: 25% TiO<sub>2</sub>, 75% BaSO<sub>4</sub>). He argues that no significant barium has been found in the Map ink by anyone and that McCrone should have easily seen the barite in the ink, but did not. Harbottle maintains this curious position even though he admits that pure anatase was, in fact, being manufactured and sold in 1921 and could have been obtained by 'the forger . . . had he desired it' (p. 183). Harbottle fails to tell the reader the fact that Titanox-B and the 99% pure Titanox-A (discussed briefly, pp. 183–4) are *both* described as early 1920s products in the *same* company brochure (Titanium Pigment Company, 1933). Clearly, *both* products were being sold in the early 1920s. Harbottle then finds significance in the fact that the mean aggregate diameter for the 1933 Titanox-A of 0.3 micron (µm) was not the same as the average 0.15 micron (µm) crystals studied by McCrone. There are no size data in the brochure for the 1921 product described as 'of an inferior grade' or for the 'much superior' 1925 anatase. Harbottle ignores the kinetic crystal growth studies of Mackenzie and Melling (1974, fig. 1) where crystallite sizes for pure anatase are shown to vary from 0.1 to 0.5 µm at calcination temperatures ranging from 680 to 900°C. The anatase studied by McCrone is within this range and is thus completely consistent with non-barium-containing products being made by several firms in the 1920s in both Europe and America. Commercial production began even as early as 1916 (Kronos, Inc. 2008). The Kronos website history page states

*NL Industries, Inc. was organized as a New Jersey corporation in 1891 and predecessor companies of its wholly-owned subsidiary, Kronos, Inc., have been producing titanium dioxide pigments (TiO<sub>2</sub>) since 1916. 1916 was the year in which the Titanium Pigment Corporation of Niagara Falls, New York, and the Titan Co. AS, of Norway simultaneously began commercial production of this new white pigment that was principally used in paints. The original process pioneered by these operations was the sulfate process, utilizing sulfuric acid, and producing anatase pigments.*

Harbottle suggests (p. 181) that instead of using, for example, a calcite–anatase composite, a forger could have 'simply cooked up a batch of iron gallo-tannate ink [or a carbon-black based ink] . . . cut a few feather quill pens and gotten to work'. He overlooks the very obvious fact that the use by a forger of such ink on a medieval parchment would have been a 'red flag' warning to certain detection. In 1904 Carvalho (1999, Ch. XVII) pointed out that . . . *the forger cannot always know of the circumstances which surround the placing of original ink on paper and . . . he cannot manufacture the TIME which has already elapsed . . . it is not strange that attempted fraud can often be made evident and complete demonstrations given of the methods employed.* McCrone and McCrone (1974, 214) concluded: 'The two-step inking is a deliberate attempt to simulate a much older ink.' It is thus quite contradictory for Harbottle to argue now (p. 185) that a two-step inking device would 'go so far from a simple replication of an authentic medieval ink recipe [that it] could be, for the forger, an invitation to detection'. The record shows that detection is precisely what happened as soon as professional scientists having the appropriate experience examined the Vinland Map. Using only a low-power

microscope, Baynes-Cope (1974, 210, *iii*) quickly detected the presence of two different ink components in January of 1967, five years before McCrone had been given the Vinland Map for his microanalytical study.

Harbottle continues his 'review' by telling the reader that anatase is common in nature. He does not explain that most natural anatase is black, reddish brown, or blue. Native anatase is not white until it has been very finely ground (i.e., its streak is white). He does not review the fact that McCrone had ruled out ground anatase (McCrone 1988, fig. 10, 11). Harbottle cites a number of references to a variety of natural sources of anatase (but not calcite-anatase composites). He concludes that the existence of these sources means that the anatase in the ink of the Map might also be explained in this way. He fails to review that Clark (2004) has never found pigment anatase on any medieval European document. He does not mention and disregards the fact that *all* of these are *minor* natural sources of anatase and *major* sources of aluminosilicate clays and/or iron oxides. The anatase in each is present only as a minor or trace component (Weaver 1976). Despite being aware of this, Harbottle does not provide even one plausible, realistic explanation for how natural, trace-level anatase crystals *having the appropriate size and shape* could have been *freed* from the predominant clay or iron oxide minerals that make up the bulk of these clays, soils and laterites to appear in either the ink of the Vinland Map or in a Roman mural artist's palette (Edwards *et al.* 2006; Clark *et al.* 2007). In this connection it is important to emphasize that Weaver used extraordinary technological steps to separate the anatase from the kaolinite clay for his study. He used Calgon, ultrasonic treatment, centrifuges, blenders, and magnets, and emphasized the need for physical energy. The kaolin mining industry spends considerable mechanical and chemical effort to break down and remove these unwanted materials in processing their clays as received from the mines. Lacking significant clay minerals or iron oxides, or a *plausible* explanation for their absence, a natural source for the anatase on the Vinland Map is out of the question.

Harbottle (p. 184) questions whether the Titanium Pigment Co. could have calcined 99% pure anatase above 700°C without converting it to rutile. He not only ignores literature to the contrary (e.g., Mackenzie and Melling 1974; Sheinkman *et al.* 1984; Gouma and Mills 2001), he forgets that he had, himself, previously told the reader (Item 8, p. 180) that *commercial* anatase formation required 'the routine step of calcination at 800–1000°C, which augments the crystal size of the TiO<sub>2</sub>'. He seems to have read, selectively, about nanostructural anatases (i.e., materials prepared using titanium tetraisopropoxide and/or metalorganic chemical vapour deposition). These are very specialized procedures, which are not relevant to the production of industrial or commercial pigment anatase.

Harbottle speculates (pp. 184–5) that an arcane hydrothermal topotactic crystallization of calcite on anatase under laboratory conditions (Ovenstone *et al.* 2003) could also operate under natural conditions at ambient temperatures over time. Although this is pure speculation, with no size, shape or compositional comparisons presented, he simply asserts that this laboratory procedure could 'clearly have generated the composite crystallites seen by McCrone' on the Map. He fails to mention that some aragonite is also produced by this procedure and that none has been seen on the Map. Neither does he present any *natural* occurrences of such anatase/calcite/aragonite mixtures to support his speculations, nor explain how such materials 'fortuitously' appeared on the ink of the Map.

Harbottle argues (p. 187) that shortly before 1957 a clumsy, misguided 'conservationist' crudely and brutally cleaned the much-handled and well-soiled parchment. He does nothing to explain this process, apart from suggesting the use of hypochlorite bleach (e.g., Clorox™). He does not comprehend that such a brutal procedure would have destroyed the iron gall ink

itself! Plenderleith (1937, 48) warns about bleaching: 'Any signatures in iron-gall inks will disappear unless they are protected beforehand.' Harbottle does not explain how such 'conservation damage' could have selectively removed some of the telltale iron gall elements (Fe, Cu, S) from the ink yet left the titanium that 'came with the ink' (p. 186). There is no evidence that the ink of the Vinland Map was *ever* iron gall ink. In particular, there are no signs that the chemical bonding process, which makes iron gall inks so permanent on parchment, ever took place (Baynes-Cope 1974; McCrone and McCrone 1974; Bradbury 2004, 2005; Seaver, 2004; Towe 2004). Nevertheless, if any genuine iron gall ink had been drastically altered, as posited by Harbottle, the 'clumsy "conservation" treatment' that supposedly dramatically reduced the Fe, Cu and S content must not have affected the 'genuine' Ti. This would indeed be hard to explain.

#### FURTHER ANALYSIS OF THE HARBOTTLE SCENARIO

A determined reader faced with, and accepting as valid, each of Harbottle's wide-ranging opinions and assertions, can eventually separate the 'wheat from the chaff'. Harbottle's various lines of reasoning in support of the Map's authenticity may be reduced to three implicit and fundamental positions:

- (1) A medieval scribe used a typical iron gall ink to draw the Vinland Map in the 15th century. The ink did not contain significant titanium and therefore had no significant anatase.
- (2) The iron gall ink subsequently suffered major chemical losses as a result of a drastic 20th century cleaning of the Map.
- (3) 'Composite crystals containing anatase and calcite' are simply the result of a fortuitous contamination from an environmental source or sources unknown. They explain the elevated titanium content found by both McCrone and Brown and Clark (2002) on the ink lines of the Map.

Several implausible, improbable corollaries follow from this basic scenario.

- (1) Like Cahill (2005), who suggested that flakes of modern paint had fallen from the ceiling, Harbottle surmises that his novel calcite-anatase composite crystals 'landed on the map fortuitously, probably from the environment'. This is improbable because McCrone, as well as Brown and Clark (2002), found the anatase in the *underlying* yellow-brown ink. They found none on the bare parchment. The PIXE data (Cahill *et al.* 1985, appendix A) show that the titanium content is enhanced in the ink relative to the parchment in detailed transects across a single ink line (Cahill *et al.* 1987, fig. 4) as well as on ink lines over the entire Map (Towe 1990, fig. 5). Thus, whether paint flakes, composite crystals or topotactic mixtures, any such contaminant must have fortuitously landed on and adhered to only the yellow-brown ink lines. These contaminants could not have adhered to either the *overlying* carbon-black of the ink or to the blank parchment itself. A 'fortuitous' landing is inconceivable.
- (2) Not even one of the 159 sample locations in the 'global' non-destructive PIXE elemental analyses of the Vinland Map's ink and parchment (Cahill *et al.* 1985) contains enough titanium to be *quantitatively* consistent with that found by McCrone in 1974, or by Brown and Clark in 2002, following up on Cahill. Not even once, by chance, did any of the 159 PIXE analyses overlap any of the locations where McCrone obtained his 21 microscopic anatase-containing samples, nor could they have overlapped any of the five anatase-containing locations of Brown and Clark (2002) in their non-destructive *in-situ* Raman study. Brown and Clark (2002, fig. 1) specifically tried to investigate some of the locations marked on Cahill's guide map (Cahill *et al.* 1987, fig. 2). This could hardly be a remarkable, improbable 'statistical sampling error'.

(3) Harbottle forcefully argues (p. 181) that his 'composite crystals must have been of natural origin' and (p. 182) 'a natural origin for anatase crystals is strongly suggested'. Not one natural source is cited for such a bizarre composite. It is improbable because calcite–anatase composite crystals, if such exist anywhere in nature, would render laterites, kaolins and soil sources impossible to defend. None of these natural sources contains calcite–anatase *composite crystals*, or even mechanical mixtures of calcite with significant anatase. Furthermore, laterites contain major iron and aluminium oxide minerals while kaolins and soils contain predominantly aluminosilicate clay minerals (>95%), none of which was found by McCrone in the Map 'contaminants' by X-ray or electron diffraction, nor seen by transmission electron microscopy. With but one exception, Cahill *et al.* (1985, appendix A) found neither aluminium nor silicon anywhere on the Vinland Map above the minimum detectable limit. It is self-evident that if there is little or no aluminium and silicon, there can be no significant clay minerals.

(4) If the calcite was, indeed, present together with anatase as an environmental composite contaminant then *two* separate sources for calcite on the map are needed: (i) calcite that formed during the standard liming process used in the preparation of parchments; and (ii) calcite in Harbottle's contaminating calcite–anatase composites. The former is found on both the front and the back of the Map's parchment (Larsen *et al.* 2005). The latter wrote: *The major part of the calcite is found on the recto side bearing the text and drawing. This is in concordance with the fact that calcite has been used in parchment making to make the parchment surface suitable for writing.* Cahill *et al.* (1985) wrote: *Calcium is a good tracer of the parchment itself and is universally seen in large amounts in every parchment and paper tested at Davis.* Clearly, any 'contaminant' calcite–anatase composites would have to have been added to the initial parchment source of calcite. Yet, the PIXE data for calcium in the ink–parchment pairs studied by Cahill *et al.* (1985, appendix A) do not support this. These data reveal that there is *no added* calcium . . . the inked parchment and the bare parchment have essentially the same calcium content, about 90 ng/cm<sup>2</sup> according to Cahill *et al.* (1985, appendix A). The parchment of the *Tartar Relation* is similar, containing 93 ng/cm<sup>2</sup> calcium. It is self-evident that if there is no *added* calcium, there can be no *added* calcite–anatase composites!

(5) Harbottle (p. 187) suggests that shortly before 1957 (and *before* a probable cosmetic laundry of the parchment), a clumsy 'conservationist' attempted to protect the drawing and lettering by permeating it with a fixative such as nitrocellulose in a solvent. Obviously, if protected before laundering, the losses envisioned by Harbottle of the signature iron gall ink elements (Fe, Cu, S . . . but not Ti?) must have taken place prior to this date. Therefore, at least *two* cleanings of the Map would have been required, one that was not protected and permitted the putative drastic alteration to the content of typical iron gall ink elements (but not Ti?) and a second that was protected from such loss. Clearly, no composite crystals of the 'contaminant' calcite–anatase were removed by *either* of the cleanings, nor could they have adhered to the yellow-brown ink once it had been 'permeated' with nitrocellulose.

(6) Harbottle states, with added emphasis (p. 186): 'the trace levels of titanium found by the Davis group were very much in line with those they had already observed in many other medieval documents agreed to be genuine'. This statement stands in contrast to other quotes from the Cahill group, e.g., Cahill *et al.* (1985, p. 12): '[Titanium was] the most frequently found element in the [Vinland Map] ink'. Titanium is never the most frequently found element in undisputed medieval inks. The amount of titanium, which the Davis group found on the other two undisputed medieval documents, was below the minimum detectable limit, while the amounts of iron, copper and sulphur were much higher than on the Vinland Map. It is both noteworthy and surprising that *all* of these values were reported at *nanogram* per cm<sup>2</sup> levels

(Cahill *et al.* 1987, fig. 5). Other genuine medieval inks contain *microgram* per cm<sup>2</sup> levels of Fe and Cu (Olin 2000; Budnar *et al.* 2001). A difference of three orders of magnitude is hardly 'very much in line' with the findings for other medieval documents. It is much more in line with the *quantitative* differences between the findings of Cahill and McCrone, which have been so heavily debated as an important discrepancy.

#### CONCLUSIONS

The critical review of the archaeometric research on the authenticity of the Vinland Map by Garman Harbottle is replete with errors of fact and omissions of relevant data. There are several conflicting, contradictory interpretations. The reader is left to decide whether any of his implicit arguments, speculations and corollaries thereto, constitute the *plausible, credible* archaeometric challenges needed to resurrect the Vinland Map from being a forgery to an authentic medieval document of historical significance. We submit that they do not. The sum total of the archaeometric evidence on the ink continues to support and confirm the modern forgery conclusion drawn by Walter McCrone over 30 years ago.

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