RESPONSE TO A PEER REVIEW REPORT

report #2 of hgss-2026-2, reviewer: Roel Nicolai

Given that it originates from a colleague who has dedicated the past two decades to investigating the origins of portolan charts and whose doctoral dissertation served as my most influential model when I first began researching this subject, it is unsurprising that this is one of the most comprehensive and lengthy peer review reports I have received regarding my research output to date. For a variety of reasons, my response to the review report is particularly extensive.

The reviewer pointed out specific flaws in the initial draft of the scientific paper, including the absence of certain arguments or details or inadequate explanations. As a result, I tried to give the reviewer and the journal's editors as much detail as I could about my theories and viewpoints.

Furthermore, since the HGSS journal employs a public peer review model, all of these documents inevitably become (and continue to be) attached to the article as a thread of metadata. Consequently, I have endeavoured to provide a more comprehensive and transparent explanation to a broader audience.

Lastly, I purposefully wrote longer answers to some of the reviewer's criticisms and **accompanied them with newly made tables and newly made figures. I plan to incorporate those elements into the revised version because they provide even more convincing evidence for the hypothesis that the spatial data underlying portolan charts originated in classical antiquity.**

In certain instances, I deduced that the reviewer provided feedback simultaneously with his initial reading of the text, rather than performing a thorough preliminary review, and his comments were at times notably sharp and strict. I trust he will not resent me for my cynicism in certain remarks and for deliberately pushing the matter to the absurd to highlight the unimportance of some criticisms.

Since the responses to some specific criticisms are extremely detailed, the most important details are additionally coloured in red, WRITTEN IN ALL CAPS, or highlighted in yellow.

General comments

a) The article proposes an intriguing idea, as expressed in the title. Marinus of Tyre has been suggested by earlier writers as the source of the precise geographical data on medieval portolan charts, but no researcher ever made any attempt at substantiating this idea. The subject of the article is well within the remit of the Journal (HGSS).

   The author appreciates that the reviewer agrees that the article fits within the scope of the journal.

b) The article is too long. The author should consider carefully whether its great length is in the reader's and his own best interest. The article contains an overly lengthy and, with respect to the subject of the article, largely irrelevant introduction with far too much background information: only at page 17 does the author begin to broach the subject proper. The detailed exposé on the origin of the mariner's compass is unneccessary, as is the long section that evaluates cartometric analysis models used by fellow researchers.

   Although the article is on the longer end of the typical journal article spectrum, I intentionally did this for a number of reasons.

1. To begin with, there is no word limit for the journal, and I believe the editors are not convinced that part is unnecessary because if they did, they would have asked me to remove or condense that part of the text beforehand.
2. Secondly, giving a summary of the most important stages in portolan chart research is, in my opinion, far from irrelevant, since the journal's main focus is on the history of geosciences (such as geomagnetism and geodesy). If the reader first gains a brief understanding of how my predecessors analysed them and how the parameters of their methodologies influenced their results, he will definitely have a better understanding of my methodology (especially if this is his first experience with cartometric analyses). In order to make my texts more comprehensible to as many readers as possible—possibly increasing rather than decreasing their number—I don't write articles that are only understandable to a small group of experts whenever possible. Instead, I deliberately include broader material into them.

c) Apart from the above digressions, the author is neither succinct nor precise in his formulation: use of straightforward language is preferable over the, in places, cumbersome descriptions, with many unnecessary adjectives.

If this subject were solely technical or engineering-related, I would agree with the reviewer's evaluation. Nonetheless, it encompasses historical events, thus qualifying it as a component of the digital humanities in the broadest possible sense, I'd say. The article's narrative is primarily organised into three time periods: (1) classical antiquity, (2) the late Middle Ages, and (3) the past century and a half of portolan chart research. Each of these periods has its own set of characteristics (levels of knowledge, science, technology, etc.) that occasionally require clear delineation, while at other times exhibit causal relationships. If that narrative were excessively (technically) dull, I am strongly convinced it would repel more individuals than it would attract. Additionally, the majority of individuals who have perused my prior publications praise the narrative for its concise and aesthetically engaging presentation of events and concepts.

d) A fundamental flaw of a methodological nature persists throughout the article. The author focuses on demonstrating the correctness of his hypothesis. Thus, the article does not follow the hypothetico-deductive approach, which is the norm in science. The author places almost exclusive emphasis on supporting evidence that confirms his hypothesis, after which he concludes that the hypothesis has been "validated", in the sense of: proven to be correct.

This article is (only) an embryonic stage of an idea that seeks to explain the geometry of portolan charts in a new way and offers a fairly convincing explanation of (one of) the hypothetical possibilities of its creation. Its drawback is that it focuses only on the *Carta Riccardiana* in detail, which can serve as a challenge for others (such as reviewers) to subsequently test the method on other portolan charts.

However, the sample of preserved early charts is small, each of them is a unique manuscript that is subject to random errors made by the hand of the drafter—who, apart from al-Shirazi and the author of the anonymous *Rex Tholomeus* chart, did not (clearly) record what their source material was—so the historical framework has more gaps than documented solid evidence, which altogether makes it impossible to guarantee that even cartometric analyses of such a broader sample of charts will necessarily provide more reliable answers.

e) Another methodological shortcoming of the article lies in its neglect of the historical context of the periods considered: supporting information is cited, but the larger context is ignored. Historical sources should be examined critically, not accepted at face value as evidence.

The author's hypothesis about the origin of the typical geometry of portolan charts respects the historical framework in at least three aspects.

- The first aspect is the consideration of three statements from the late Middle Ages regarding their origin based on maps from classical antiquity (section 6.3.1).
- The second aspect is that the proposed empirical model is based on map projections that were established and well-known in classical antiquity (equidistant cylindrical and conic projections) and does not assume the existence of the Mercator projection before Gerard Mercator.
- The third aspect is the explicit acknowledgement of the existence of certain gaps and discrepancies in the documented records (section 7, pages 34-35, rows 806-834).

The article uses the term "plausibility" twice and the phrase "it is plausible" five times in relation to the testing of his hypothesis. Plausibility may have a role in science in the formulation of a hypothesis, but not in its testing; plausibility is not a criterion for drawing scientific conclusions.

According to the *Cambridge Dictionary*, the term "plausibility" describes "the quality of seeming likely to be true, or possible to believe". Since this hypothesis had not yet existed, it has indeed been formulated. It (the hypothesis) has also been formulated by using the proposed empirical model, as demonstrated by the sequential observation of the two-piece figures 7, 10, and 11 (which are actually six images sequentially observed). At the end (section 7.5, page 32) the author concludes that the proposed model seems likely to be true; therefore, it seems plausible.

f) This article lacks scientific rigour.

The response to this (overall) criticism is broken down into multiple (sub-)components in the *Specific comments* section.

Specific comments

Nr	Lines	Comment
1	Title	"… How Pietro Vesconte might have assembled the *Carta Riccardiana* …" Attribution of the *Carta Riccardiana* to Pietro Vesconte is speculative, albeit not improbable. However, it is presented with a degree of certainty that is unwarranted, in the title and elsewhere in the article. It is clearly illustrated in Figure 3 (the upper section) and Figure 9 that the coastline contours of the Mediterranean and Black Sea on the *Carta Riccardiana* and the composite of two portolan charts by Pietro Vesconte (made in 1311 and 1313) are nearly identical, both regionally and (more important) locally. Therefore, it is (highly) probable, rather than merely speculative, that he is the author of the *Carta Riccardiana*. However, and despite that, the title clearly states: "How Pietro Vesconte Might Have Assembled …", not "How Pietro Vesconte certainly Assembled…". Furthermore, in all other parts of the article where his name is mentioned alongside the *Carta Riccardiana*, it is clearly emphasised that he may be the author of the chart. The *Carta Riccardiana* is directly mentioned in several places in the text and compared to Vesconte's charts from 1311 and 1313, which are certainly known to be his, so this may have confused the reviewer. Overall, there is no objective level of certainty in the text regarding Vesconte's authorship; rather, it could be said that it is a matter of the reviewer's subjective impression.

2	63	"The hypothesis is demonstrated …". A hypothesis can (and ought to) be tested, as a result of which it is either *corroborated* and retained as a hypothesis or it is *falsified*, after which it is rejected. It is not possible to *demonstrate*, *substantiate or validate* a hypothesis to indicate that it is correct or true. The last three words in italic are all used in the article in the sense of proving the hypothesis. This approach constitutes a logical fallacy, known as the *post hoc, ergo proper hoc* fallacy. The author acknowledges a poor (i.e. non-Popper) choice of words and will change it to "corroborated" at page 2, row 63. At page 29, row 659, the sentence "This hypothesis is substantiated by two reasons" will be replaced with "This idea is supported by two reasons". On page 32, row 752, "corroborating" will be put instead of "validating".
3	72	The author posits here that his lengthy introduction is "crucial for understanding the fundamental findings of this research". Although some introduction certainly will be required to provide the context for the research reported in the article, the detail with which e.g. the history of the compass is spelled out is not required to understand the main subject of the article. Although the author acknowledges that passages related to the history of the compass are not absolutely necessary in the sense that understanding bare mechanics of the cartometric analysis would be impossible without them, he thinks that this section is more helpful than unnecessary because the text aims to contextualise the analysis's findings in a broader historical framework.
4	91, 92	Petrus Peregrinus did not describe a compass "featuring a floating needle", but a compass, featuring a lodestone in a closed round box that floats in a larger bowl that is partially filled with water. His dry pivot compass did not have "a compass rose affixed to the base of the housing" but the cardinal directions (N, E, S, W) marked on the lid plus a subdivision into 360 degrees. This is sloppy formulation by the author. The reviewer is correct. Sentence is now reworded as follows: *Peregrinus delineated two varieties of compasses: one with a floating oval magnet positioned in a bowl which floats in water within a larger transparent container, and another classified as a dry pivot compass having a magnetised iron needle pivoting around a vertical axis that is affixed to the base, coupled with a compass rose marked with cardinal directions and a degree scale along its edge.*
5	130-379	This extensive description and review of cartometric methods used up to date goes considerably beyond what is required to understand the author's reasoning in the current article, despite the author's claim to the contrary (see comment 3). The purpose of these sections is not solely to present requirements for comprehending the author's reasoning in the current article, particularly for individuals already adept in cartometric analysis of portolan charts and acquainted with the methodologies employed by prior researchers. The text is structured to assist readers with limited knowledge of the subject, providing a comprehensive overview of the fundamentals in one location, and ideally, to enhance their interest in the topic. Those who find this information unnecessary are entirely at liberty to skip and ignore it.

6	249, 334341, 343-350	The article should avoid negative criticism of fellow researchers' methods (Gaspar and Nicolai), especially when delivered without any supporting evidence. The author should limit himself to explaining the merits of his own analysis method used in this article and not claim shortcomings, whether real or imagined, of the methods used by fellow researchers, particularly as they are not, or only indirectly, linked to the subject of the article. Sections 4, 5, and 6 provide an overview of previous research from several angles: what motivated the researchers, what methods they used, what results they obtained as a result, and what the positive and negative aspects of those conducts are. Negative criticism, as the reviewer calls it, is therefore just one of the layers of this overview, but citing (and ultimately chronologically listing) these negative critiques is necessary to explain what progress the subsequent cartometric research represented and why. The reviewer says that negative criticism should be avoided, especially when delivered without any supporting evidence, but Gaspar's zero-redundancy sampling of the western Mediterranean (which hypothesises the existence of two prototypes of portolan charts) was criticised in a similarly superficial manner by the reviewer himself in a recently published article. Regarding the prior works of the aforementioned two researchers, for example: Gaspar's EMP model fails to elucidate the reduced scale of the North Atlantic representation, whereas the author's model in section 7 addresses this issue, establishing a direct connection to this article. Furthermore, given that the distortion variation between any equidistant cylindrical projections along their X-axes is linear, the application of a 6-parameter affine transformation enables, by definition, a map created in any (true-to-scale) equidistant cylindrical projection to be georeferenced to any other equidistant cylindrical projection with zero residuals. Any residuals encountered in practice will strictly be a result of human error in placing ground control points or paper map warping/digitization errors. Given that Loomer (1987) has already assessed the deformations of the carrier material and deemed them negligible, and Nicolai (2014) replicated this test, employing a 6-parameter transformation results in overfitting of the data, as it, unlike Helmert, addresses directional distortion as an aspect to be rectified.
7	431-450	<u>Explicit testimonies of classic origins.</u> The author provides several 'testimonies' from medieval writers to support his claim of an antique origin of portolan charts, accepting these at face value without critical assessment of the historical context. Medieval man held Antiquity in great reverence, both in the Islamic and the Christian world. The statements by Cotrugli and Qutb alDin al-Shirazi reveal that they were convinced portolan charts did not have their origin in their own time, which is about all that can be deduced from their statements. The historical context is not mentioned in section 6.3.1, but it is certainly referenced in section 7 in many places, particularly on pages 34-35, rows 806-834. Furthermore, I appreciate the reviewer's caution, but those three historical sources explicitly claim that the origins of portolan charts are from classical antiquity, and I personally have no reason to believe that they are lying or speaking broadly based on hearsay impressions until this is specifically proven. In line with the reviewer's pursuit of rigour, it can be said that the only way to truly prove (or disprove) this is to travel back in time and ask them personally – but that would only be valid on

		the condition that the time machine took us to our own universe, and not to a parallel universe where history unfolded differently (since it's possible that a multiverse exists). The reviewer (based on his research) reasonably believes that portolan charts are not originally late mediaeval, but he stopped there because he was preoccupied with testing previous hypotheses. This research offers a relatively tangible historical direction, and it might not be a bad idea to remain open to that possibility until new historical records emerge.
8	447-449	The author cites his earlier work which he reported in a different article (Marelić 2025b), in which he described a novel and interesting interpretation of the portolan mile, placing its origin in Antiquity. However, the author's analysis in that article uses the same flawed reasoning, focused on confirmation, that he uses in the current article (see general comment d). While this does not necessarily invalidate his interpretation, the author should not present his idea as if it were a proven fact, which he does in the current article. The author's articles regarding the origin of portolan charts are notably cautious in their presentation, frequently employing terms such as: "perhaps," "likely," "it could have happened," "potentially," "suggesting," "appears to have been drawn," "indicate," etc. There is not a single instance of presenting them as proven facts … the paper (Marelić 2025b) included. In it, the author computed that *50 portolan miles, the standard large interval (spatium) on portolan chart scale bars, corresponds to the length of a one-degree arc along the parallel φ=36°, based on Ptolemy's erroneous estimation of the Earth's dimensions*. This result is not a consequence of confirmation bias; instead, it arises from the relationship between these two historical units, and the author does not grasp the scientific issues noted by the reviewer. Individuals wishing to evaluate and potentially contest this are encouraged to analyse the data and perform calculations and prove it otherwise.
9	457	The properties of portolan charts for which the author attempts to provide a novel explanation are: 1. the approximately gradual increase of the counterclockwise tilt angles of the regional subcharts, going from west to east; 2. the approximately gradual increase in scale of the regional subcharts, going from west to east. The author posits that Pietro Vesconte initiated this with the *Carta Riccardiana*. However, the *Carta Riccardiana* is not the first portolan chart. Older charts exist (the Carte Pisane and possibly the Cortona chart) and have probably existed (the chart on which the *Liber de existencia riveriarum* (~1200-1230) was based). The author does not refer to the construction of those mosaic charts; he places undue emphasis on Pietro Vesconte as the originator of the mosaic form of portolan charts, thus ignoring an important timing issue. Other than the fact that the *Carta Riccardiana* was not the first mosaic chart, an issue the author omits to mention is the considerable variation in rotation angles and regional scale variations between portolan charts. Figure 7 and the corresponding text (pages 19-22) unequivocally demonstrate the existence of earlier mosaic charts. Ultimately, in figure 7, al-Shirazi's matrix and the *Pisane*, *Cortona*, *Avignon*, and *Lucca* charts are distinctly positioned chronologically before the *Carta Riccardiana*, rendering this criticism unfounded. The author posits that it is theoretically plausible that – similar to the authors of (at least some of) those anonymous charts – Pietro Vesconte had the chance to observe, possess, and replicate the original works (from classical antiquity), and may have been a more adept illustrator or, at the very least, a more proficient compiler of segments into a mosaic. However, this does not imply that he was the first or sole individual in history to undertake such actions.

		At the end of the article (page 32, rows 763-771), the author explicitly asserts that the *Cortona chart* may represent an earlier effort to replicate such a composite, and that the chart that once accompanied the *Liber de existencia* manuscript lacked rotation, yet may have employed a comparable logic of stacking segments (sub-pieces) with respect to scale. In those sections 4, 5, and 6 (pages 7-17), deemed unnecessary by the reviewer, the author repeatedly discusses the phenomenon of significant variation in rotation angles and regional scale discrepancies on portolan charts. Also, Figure 5 explicitly illustrates the differences in rotation angles and regional scale variations for the Atlantic and Alboran Sea coasts between eight charts! Additional information on this sub-topic will be provided subsequently in responses to the succeeding comments.
10	465	On Kelley's concept of a trilateration network: "This hypothesis has not undergone quantitative testing until this day". The author overlooks or is unaware of the fact that Kelley's idea was refuted by Nicolai (Nicolai 2014, pp 82-86). Sentence "This hypothesis has not undergone quantitative testing until this day" does not refer to anything made by Nicolai nor anyone else except the author himself. It refers to Marelić's [previously untested] study which – *initially inspired by Kelley's concept of a trilaterated network starting at Gibraltar – posits that it may result from a technique utilised by mediaeval cartographers in which they graphically integrated copies of regional maps in a cylindrical projection, based on Eratosthenes's measurements of the Earth, to reproduce their appearance in a conic projection, orientated such that the meridian intersecting the "beginning of the Mediterranean" (its westernmost point near Gibraltar) is plotted vertically.*
11	489	"… a relative clockwise rotation of the Alboran Sea, positioning the area Gibraltar further north". The author needs to quantify this and explain its significance. The rotations of the Alboran Sea representation are a marginal aspect of this article, and the author perceives no advantages in their quantification. The objective of those sub-topic and Figure 7 is to illustrate and elucidate that the assembly of the mosaic known as the portolan chart underwent distinct historical phases (as exemplified by the Alboran Sea) and that subsequent to Vesconte (1311-13) and the *Carta Riccardiana*, the clockwise orientation of the Alboran Sea became the norm.
12	553-555 580	"7.2 A better geometrical fit to the projection of Marinus of Tyre" In this section the author claims that the subcharts of the *Carta Riccardiana* exhibit a better fit to a mid-latitude chart (equidistant cylindrical) than to the Mercator projection. This is puzzling. The two projections are nearly indistinguishable for portolan charts, as is expressed in Table 1 of the article (line 580) and was earlier shown by Nicolai (Nicolai 2014, pp 248-254). Nicolai made the same comparison but found the opposite: the RMSE values of all sub-areas of the *Carta Riccardiana* were *larger* for the fit to the equidistant cylindrical projection than for the Mercator projection (Nicolai 2014, 233-237). Whatever the reasons for this discrepancy may be, the categorical conclusion that the author's results "refute the anachronistic hypothesis that conformal cylindrical projection or something akin to it existed previous to the early modern period" is unwarranted.

		The most likely reason for the discrepancy is that the reviewer employed a 6-parameter affine transformation (that permits biaxial deformations of the old map to partially adapt it to the reference map image), whereas the author used a 4-parameter Helmert conformal transformation (that does not relatively deform the old map image during georeferencing, thus neither the entire Mediterranean area nor any of the chart's subsections become forcefully biaxially stretched and scaled to better emulate equidistant cylindrical projections of different true-to-scale latitudes). In addition, the author used identical sample of identical points per sub-piece (subsection) for both projections whereas the reviewer did not (as he filtered out the outliers), and the author approximated the Earth using the WGS84 ellipsoid, whereas the reviewer approximated it using the sphere. As a result, these two studies not only yielded distinct RMSE accuracy values for various reference map projections, but they also identified different extents of charts' sub-pieces; for example, the author singles out the *Upper-West Mediterranean*, while in the reviewer's work, this region is the overlap of two subcharts; the *Western Mediterranean* and the *Central Mediterranean* subchart. According to the methodology employed in this paper, the results are as presented and are not inherently "unwarranted". The sole factor rendering them somewhat unwarranted is that the *Carta Riccardiana* is the only chart examined in this study.
13	586-587	The author proceeds to claim that his results are: "… critically significant as they tangibly indicate that portolan charts may be mosaics of regional maps or charts, whose geometries were well-established in classical antiquity and attributed to Marinus of Tyre …" It is enormous leap to go from the figures in Table 1 to the above claim; it is even completely unwarranted; many more conditions would need to be satisfied before such a conclusion can be drawn. The author posits that the results indicate (not prove) that portolan charts may be (not that they are) mosaics of regional maps or charts, whose geometries were well-established in classical antiquity and attributed to Marinus of Tyre. Although, from a technical aspect, this is a conclusion (of a smaller degree), the author considers it (merely) one of the hypothetically possible options, and not a one hundred percent accurate and irrefutable fact. It is important to note here that by the term *Marinus of Tyre's projection*, the author in this paper does refers to the equidistant cylindrical projection in general, not only the version with true-to-scale parallel of 36°.
14	589-590	The author posits the availability of Ptolemaic maps to 13th and 14th century European cartographers. The reception of Ptolemy's *Geography* in 15th-century Europe is a subject extensively studied in the history of cartography and geography. Nothing in all these studies indicates that Ptolemaic maps might have been available to European cartographers in the early 14th century (or even earlier). An extraordinary claim as the author makes requires convincing evidence, but the author does not show any evidence at all. This is evidently unscientific. Science is an evidence-based knowledge creation process and has no place for speculative claims.

		The mentioned paragraph is just a teaser for the proposed mechanics by which late mediaeval copyist-cartographers assembled regional charts into a mosaic based on ancient templates. Detailed step-by-step explanations follow shortly after in sections 7.3 - 7.5. The author refers to historical records that claim Greek influence and mention Ptolemy himself and refers to previously computed geometric elements that (indirectly) connect portolan charts to Ptolemy's work. Those can be found in Marelić (2025b), and in sections 6.3.1, 7.3, 7.4, 7.5, and 8 of this article. On the other hand, the reviewer draws the conclusion that the principles of the conformal cylindrical (Mercator) projection were known to (some unspecified) ancient civilisation based on the findings of his computational research. He considers those findings to be entirely scientific, despite the fact that no historical records even hint at, much less validate, this. Essentially, both studies attempt to hypothesise the course of events in the past based on rather scarce material evidence, resulting in conclusions that are partly scientific and partly speculative.
15	605	The author states that Nicolai contends that distance travelled by a medieval sailing ship could not be reliably estimated "due to the absence of preserved records on the subject". This is incorrect. Nicolai's contention is based on an evaluation of the achievable accuracy of the measurement method for distance sailed proposed (even assumed) in many publications. The absence of preserved records is not *the reason* for Nicolai's contention. The author wanted to make the point that [According to Nicolai] it is difficult for us to determine how distances travelled by sailboats were measured in the late Middle Ages – not exclusively, but partly – due to the fact that there are no preserved records on the subject. To avoid further ambiguity, the sentence will be rephrased as follows: *In his overview of the navigational practices during the late Middle Ages, Nicolai contends that it is difficult to reconstruct how distances travelled by sailboats were measured due to the absence of preserved records on the subject, noting that the earliest documented observations of ship speed and distances sailed occurred only in the sixteenth and seventeenth centuries, employing methods known as the Dutchman's log and the English log.* Nicolai (2014), page 130: *The only way to measure distance sailed in the Middle Ages was by multiplying the estimated average speed of the ship with time lapsed. We do know with certainty that the distances, travelled by ships in the Middle Ages were recorded, or at least remembered and communicated to some extent, but the initial estimates were crude and expressed distance mainly in terms of days of sail, as had been customary since antiquity.* *In spite of the detail so assertively presented by Kelley, we do not really know how ship's speed was estimated in the Middle Ages. Taylor doesn't speak out on the possible method of distance estimation at all. However, she is clear in her view that speed was "a matter of estimate and not of measurement". After all, the so-called English log, which allowed proper, objective measurement of ship's speed, was only invented in the sixteenth century.*

16	611613	Further misquoting, misreading or misunderstanding(?) of (Nicolai 2014) in the sentence indicated. Reconstruction of the hypothetical plane charting technique was *not* the reason for Nicolai's conclusion that the accuracy of portolan charts could not have been achieved with medieval navigation data. Instead, this was based on the accuracy analysis of a (geodetic) network consisting of compass directions and distances between coastal points, using as input optimistic estimates of achievable precision in the observations. The reviewer is correct; the sentence has been rephrased as follows: *Furthermore, he asserts that even if highly optimistic assumptions about achievable navigation accuracy and data-processing capabilities were made, the actual accuracy of sailing traverses—whose distances and bearings could allegedly have been acquired and used to create the charts—significantly lags behind the accuracy demonstrated by the portolan charts, and that they cannot have been constructed using plane charting technique (Nicolai, 2014, pp. 380-383), which proponents of the late mediaeval origin hypothesis claim was systematically employed by mediaeval sailors to compile the spatial data for portolan charts.*
17	641-643	The starting point of the author's hypothesis is the assumption that late-medieval cartographers had access to accurate regional charts on Marinus of Tyre's equidistant cylindrical projection @36°, equipped with graticules. The author's hypothesis assumes that cartographers proceeded to make mid-latitude charts of these regional charts. This required an E-W compression of the region north of 36°N such that the ratio of the intervals of parallels and meridians was equal to their ratio at the midlatitude of that region. The starting point is the assumption that they obtained accurate regional charts **that were already made** in their optimal equidistant projection (i.e. in accordance with their proprietary true-to-scale mid-parallels) by the cartographers from classical antiquity. The objections against this idea must begin by stating that latitude and longitude were not used, even unknown, as geographic concepts in the late Middle Ages. It is even the question whether late-medieval cartographers would have understood a graticule, let alone that they would immediately spot the shortcoming that the interval ratio of parallels and meridians at 36°N did not agree with what it had to be at the mid-latitude of the regional chart. The author's hypothesis further assumes that the cartographer somehow determined the mid-latitude of each region (by measurement or by estimating it from the assumed Ptolemaic map?). The original cartographers from classical antiquity (supposedly) did that – not the late mediaeval copyists! To them, those were just (partial) images of the area whose copies they decided to graphically fit together into a mosaic image. The next step in the hypothesis would be the cartographer noticing that the meridians of the regional charts no longer lined up. That could hardly have been a surprise to him, because he had just compressed the regional charts in the way described. The cartographer is now supposed to have had the brilliant idea that he should stretch the regional charts *equally in all directions* until the meridians line up again. As a result, he now would have had a set of regional charts that were nearly conformal but varied in scale due to the last scale enlargement. This process, according to the author's hypothesis, explains the regional scale variations on a portolan chart. The author assumes that late mediaeval illustrators (or copyist-cartographers, as he calls them) had to figure out some way to merge those images (of differing grids, i.e. graticules) together. Since

		they were unaware of the mechanics of map projections, it is possible that they decided to proportionally enlarge their copies of the upper row maps (Upper West Med, Adriatic, Aegean, Black & Azov) until their longitudinal intervals align with those of the southern row (S Atlantic, W Med, Central Med, and East Med). By doing so, naturally, particular map-scale distortions arise across that upper row of enlarged images, but those copyist-cartographers seem to have been completely oblivious to that fact since they perceived map-scales to be omnidirectionally accurately preserved across the entire chart field. The author wisely exempted the North-Atlantic region from the described process, because that would have conflicted spectacularly with his hypothesis: the hypothesis would have led to expect a considerable scale enlargement of that region, whereas on portolan charts its scale is much smaller than that of the Mediterranean. This is incorrect. The author states: *The territorial coverage on portolan charts for the Mediterranean and Black Sea is predominantly horizontal, whereas the North Atlantic connects to the South Atlantic in a vertical orientation. If the original map of the South Atlantic utilised the equidistant cylindrical projection φ_0=36° and the North Atlantic map employed the same projection with φ_0=48°, it follows that their latitude intervals are identical when both maps are made to equal scale. Furthermore, assuming the cartographer lacked prior knowledge of geodesy—as was the case in late mediaeval Europe—**it is plausible to suggest** that the North Atlantic map was simply positioned directly above the South Atlantic map without conformal scaling, unlike the maps of the northern regions of the Mediterranean and the Black and Azov Seas.* Additional information about this rationale that explains it in greater detail is put in responses to later reviewer's comments. The fundamental problem with this part of the hypothesis is that it assumes a level of cartographic and geographic knowledge that a late-medieval cartographer cannot have possessed. It flies in the face of what we know about knowledge and capabilities in this period, ignoring the medieval context of the whole portolan chart problem. The fundamental aspect of this segment of the author's hypothesis is that it emulates the cognitive reasoning of an individual unfamiliar with the principles of geodesy and cartography, who would endeavour to visually amalgamate a mosaic of images that align in certain graphic elements (latitude intervals globally corresponding if made to the same scale) and diverge in others (longitude intervals on the regional maps becoming more compressed as one moves northward). It is merely one of the theoretically feasible actions, and the author's model ultimately (the lower section of figure 11) substantiates it more than it contradicts it, and it is the only existing model that tangibly explains the scale distribution of sub-parts on portolan charts, albeit solely in on the example of the *Carta Riccardiana*.
18	645	"Subsections of the *Carta Riccardiana* exhibit a superior alignment with the cylindrical projection φ_0=36°". See comment 12 above. This overstates the issue. The alignment is either marginally better or marginally worse than a fit with the Mercator projection. On page 23, rows 569-571 the author posits:

		Georeferencing the Carta Riccardiana subsections to equidistant cylindrical projections with true-to-scale parallels derived from their proprietary mid-latitudes, reveals slightly better overall geometric agreement than in comparison to the Mercator projection (Table 1), with the exceptions of the Adriatic Sea, and the Black and Azov Sea subsections. To exercise even greater caution, the author will rephrase this into a ***slightly better*** *alignment.*
19	669	*Figure 7 illustrates the process described in comment 17. The question is: Does this agree with the best-fit figures calculated for the subcharts of the Carta Riccardiana? These figures ought to be listed in the article, but they are not. How can the author's hypothesis be tested without these numbers?? I am therefore supplying the figures from my own analysis, of which the areas are defined slightly differently; however, it does permit comparison of the figures. I added the Dalorto chart (~1325) to show that other portolan charts may have very different scale differences.*

Subarea	***Article Fig. 7***	***Nicolai Carta Riccard.***	***Nicolai Dalorto chart***
North Atlantic	*0?*	*-2%*	*-24%*
South Atlantic	*0*	*-8%*	*-13%*
Western Med	*0*	*0*	*0*
Upper West Med	*8.9%*	*8%*	*10%*
Central Med	*0*	*Not evaluated*	*Not evaluated*
Adriatic	*10.6%*	*8%*	*-5%*
East Med	*0*	*1%*	*1%*
Aegean	*2.7%*	*16%*	*5%*
Black Sea	*12.5%*	*17%*	*17%*

Although the extents of these nine segments were derived from georeferencing the Carta Riccardiana to a modern map in Mercator projection (Figure 4), the values of +8.9%, +10.6%, +2.7%, and +12.5% show how much those four upper-row segments (of the supposed ancient maps collection) made in equidistant cylindrical projections tailored to their mid-latitudes need to be conformally enlarged to match their longitudinal intervals with four segments of the lower row (in equidistant cylindrical projection φ_0=36°); shown in the lower part of Figure 7.

Below is the table of Least-Squares-Estimation-computed differences in scales of its subsections after georeferencing the *Carta Riccardiana* to the modern map in the Mercator projection and in the best-fit cylindrical projection (NOT PROVIDED IN THE ARTICLE, **BUT NOW THE AUTHOR THINKS IT WILL BE A SUITABLE ADDITION FOR THE REVISED VERSION**). Please notice that since the Mercator map drastically expands areas towards the poles, the apparent scale difference in comparison to the Central Mediterranean becomes 81% (or -9%), whereas in the case of its best-fit equidistant cylindrical projection for the North Atlantic (φ_0=48°) the relative map scale drops to just -3% (97%). Also, when the northern four segments of the Mediterranean basin are conformally stretched, the relative scale differences become nearly identical to those in comparison to the Mercator map.

The reason for the radical map-scale increase of the Aegean sub-part (+13.2 %) is assumed on page 34, rows 791-798.

The sole problematic sub-part, i.e. the one that is somewhat harder to explain is the South Atlantic. The reason for its diminished map scale of about 87.2% (-12.8%) are explained in response to critique No. 22.

	N Atl	S Atl	W Med	Upper W Med	**Central Med**	Adriatic	Aegean	E Med	Black
Mercator	81.0	85.7	97.0	95.7	**100.0**	97.7	112.9	100.5	101.4
best-fit equidistant cylindrical	97.0	87.2	99.4	103.5	**100.0**	107.3	116.3	99.9	114.5
best-fit equidistant cylindrical **after conformal stretching of four segments** (see Figure 7)	97.0	87.2	99.4	**95.1**	**100.0**	**97.1**	**113.2**	99.9	**101.8**

The key question is: are these numbers sufficiently similar to conclude with confidence that the author's hypothesis can explain these scale differences? The author did not specify any objective criteria that would lead to rejection or acceptance of the hypothesis, but feels that "his hypothesis is substantiated" (line 659). I disagree.

The author's response to critique No. 22. addresses this objection as well.

20 | 676-689

<u>The rotation angles of the subcharts – The Gibraltar meridian</u>

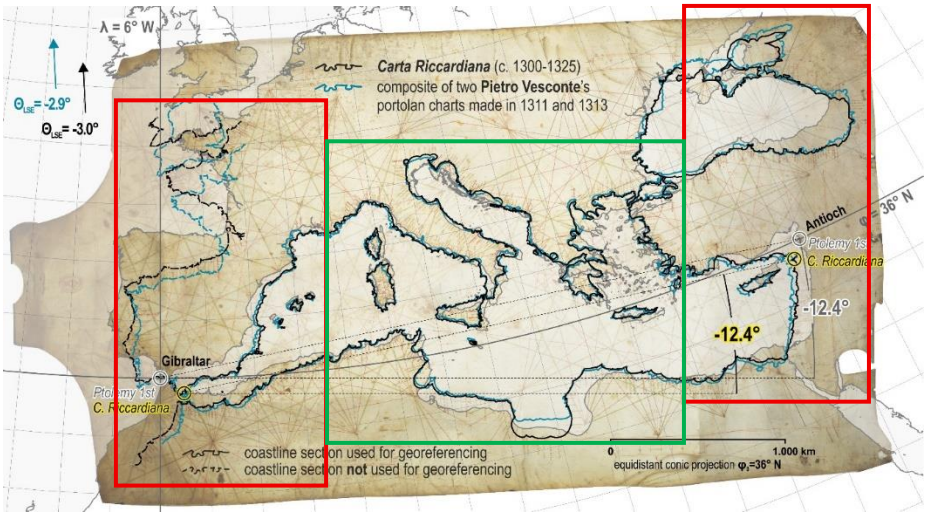
The first issue with this approach is the reorientation of Ptolemy's world map such that the Gibraltar meridian is vertical. Why would this have been done? What would the special significance of this meridian have been to medieval cartographers and mariners? Would it not rather be expected that Vesconte (a Genoese) would have aligned the vertical with the meridian of Genoa?

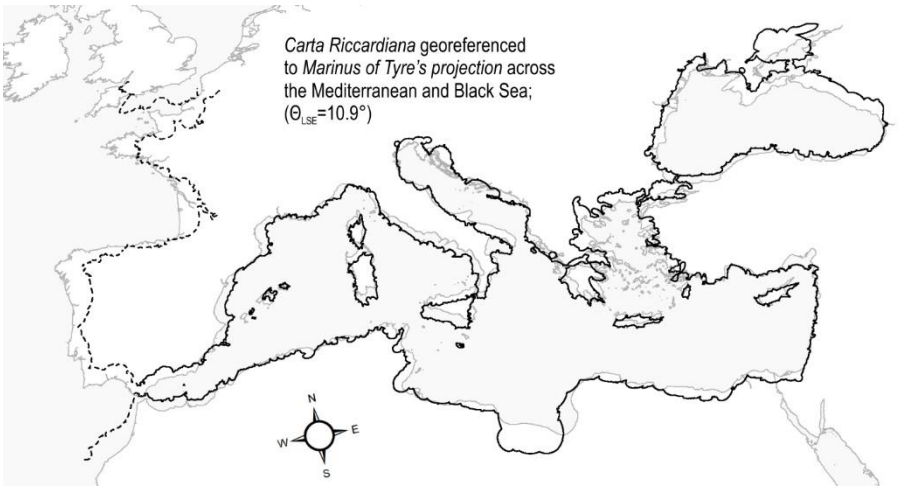
The earliest known charts, such as the *Pisane* and *Cortona*, seem to have been primarily designed to show the Mediterranean and Black Sea, since their Atlantic coasts are overly sketchy and completely useless for navigation.

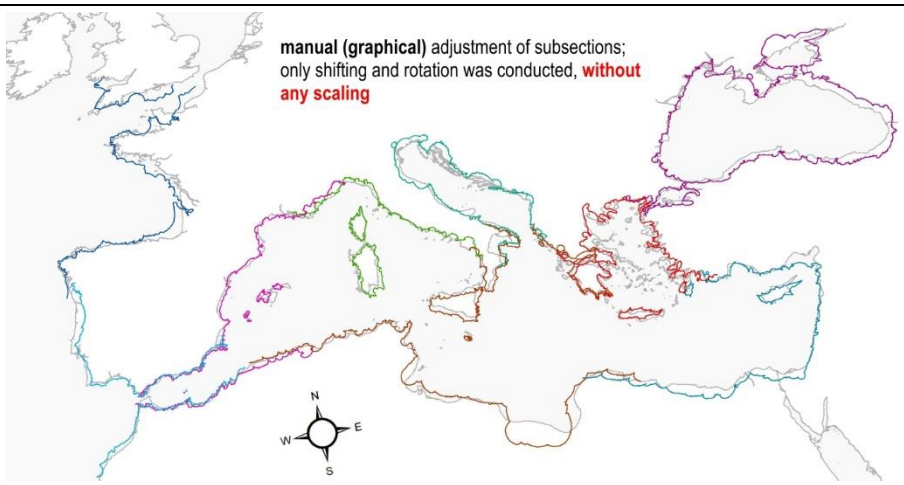
The equidistant conic projection has a radiating display of meridians, so rotating the physical map around their point of convergence and selecting any meridian to be positioned vertically is fairly an easy job for anyone. If someone from the Western Europe (used to reading the Latin script from left to right) and without any knowledge in geodesy wants to assemble the mosaic map of those areas, it would be logical to start from Gibraltar because it is the westernmost point of the Mediterranean (or farthest left when the map is orientated north-up).

In the late thirteenth and early fourteenth centuries, Italians started to navigate to England, and more accurate charts of the region were needed. The author's earlier publications show that Vesconte's charts – and, in extenso, the *Carta Riccardiana* – have large portions of coastline contours of the Mediterranean that were rendered nearly identically to those earliest anonymous charts (see Figure 6 in Marelić 2024a and Figure 3 in Marelić 2025b). Therefore, it seems logical to assume that some of them were copied from the same source material, and even that Pietro Vesconte had access to the original maps or charts (from classical antiquity) as well, and that he was just a more talented illustrator and copyist, being able to reproduce their geometry in more detail.

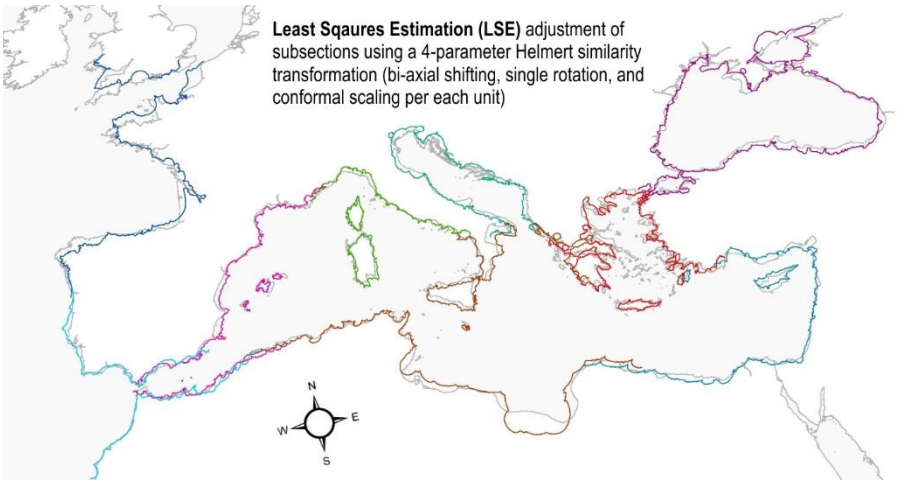
This is just a hypothesis that is only partially supported by three threads of (still somewhat soft) evidence:

		1. two late mediaeval records assert that portolan charts were derived from Ptolemy's maps; 2. metrics of al-Shirazi's matrix show some geometric similarities to cartographic practices from classical antiquity and Ptolemy himself (see Marelić 2025b); and 3. the tilt of the Upper West Med (-8.1°) on the *Carta Riccardiana* fits a modern approximation of Ptolemy's first projection centred at Gibraltar (with the meridian tilt of -9.5°) fairly well (see Figure 8), and the entire central area (roughly two-fourths of its longitudinal extent encompassed in the green rectangle in the image below) shows a relatively good fit, while the westernmost and easternmost one-fourths graphically break off. 
21	676-689	<u>The rotation angles of the subcharts – Agreement with portolan charts tilt</u> Fortunately, the author *does* provide the rotation angles resulting from his cartometric analysis, as well as the rotation angles that would result if the subchart meridians were to be aligned with a map on Ptolemy's first projection, viz. in Figure 8. Figure 8 makes immediately clear that if the rotations of the subcharts of the *Carta Riccardiana* were caused by aligning the grids of the available regional charts with Ptolemy's map, the rotation angles would have been much larger than what is seen on portolan charts. Even without objective criteria for acceptance/rejection it is clear that this much too large and the author's hypothesis would need to be rejected. Below is a cumulative response to both this critique and the subsequent one.
22	731-732	Clearly, the author is not prepared to give up his hypothesis yet. He hypothesizes that the medieval cartographers must have concluded that these rotations were too large and deviated too much from the original regional maps with which they had started, so the author assumes that they arbitrarily halved the rotation angles they obtained from Ptolemy's chart (approximately). This brings them very close to what we see on the *Carta Riccardiana* (and other portolan charts). (figure 10 top and bottom) The impression cannot be avoided that the author uses an *ad-hoc* argument of convenience here to rescue his hypothesis. This smacks of working the data until it fits the hypothesis. The scale and rotation figures that result from his hypothesis ought to have led to its rejection. Halving something arbitrarily – in this case, the angle of tilt of the display – in order to achieve that a composite image devoid of meridian convergence (at regional levels) becomes visually closer to a display with overall meridian convergence likely requires the least possible degree of abstraction

		and technical complexity. Consequently, it is entirely plausible to assert that an individual in the Late Middle Ages could arrive at such reasoning and implement it. At the same time – whether by design or not, we cannot know – the tilt of Ligurian coasts (the entire Upper West Mediterranean section) remains quite close to its tilt on the modern approximation of Ptolemy's first projection and to the magnetic declination across that region according to the CALS3k.4 model. Therefore, the author contends that – considering the late mediaeval Western European level of knowledge in cartography – this is a no-nonsense proposal (hypothesis) that shouldn't be rejected that easily.
	756-757	Nevertheless, the author feels that his results "substantiate the proposed mechanics of the classical antiquity origin hypothesis with outstanding plausibility". It is unclear what it precisely means when a hypothesis is "substantiated with outstanding plausibility". See the general comments on hypothesis testing. When the *Carta Riccardiana* is georeferenced as a single unit across the Mediterranean and Black Sea areas to the Mercator projection its accuracy is 28.3 km (RMSE dLAT) and the overall tilt is 10.6°. In comparison to the equidistant cylindrical map φ_0=36° (the "true" Marinus of Tyre's map), its accuracy is 34.7 km (RMSE dLAT), with the overall tilt of 10.9°. Although the overall accuracy is noticeably lower, (A) on the sub-piece level, the accuracy of its sub-parts is slightly higher (see Table 1 in the article), (B) it better explains the scale of the North Atlantic. Therefore, the author deemed that this idea deserves a deeper scrutiny. Below is the image of the vectorised coastlines of the *Carta Riccardiana* when georeferenced across its Mediterranean and Black Sea areas to the equidistant cylindrical map φ_0=36°, additionally rotated -10.9° altogether to rectify the anticlockwise tilt of the *Carta Riccardiana*.  Let's now assume that the author's hypothesised (nine) regional maps from classical antiquity were real, and that a late mediaeval illustrator (copyist-cartographer) attempted to arrange them to resemble the overall image in the equidistant cylindrical map φ_0=36°. That concept is reflected in the image below, which was **created entirely by hand**; segments were only shifted, some of them were rotated, and no scaling was done (there's no scale reduction of the South Atlantic). The author posits that the majority of people would consider that image to be a fairly good geometric fit if they were only looking at it.



The next image was created through *Least Squares Estimation* (LSE) adjustment of segments; that is, after each segment was individually georeferenced and optimally shifted, rotated, and scaled using a 4-parameter Helmert similarity geometric transformation. Without designations attached to these two images, the majority of modern people – let alone some late mediaeval person who never saw a correct map of the area – would find it nearly impossible to distinguish which one is planimetrically more accurate based only on visual observation.

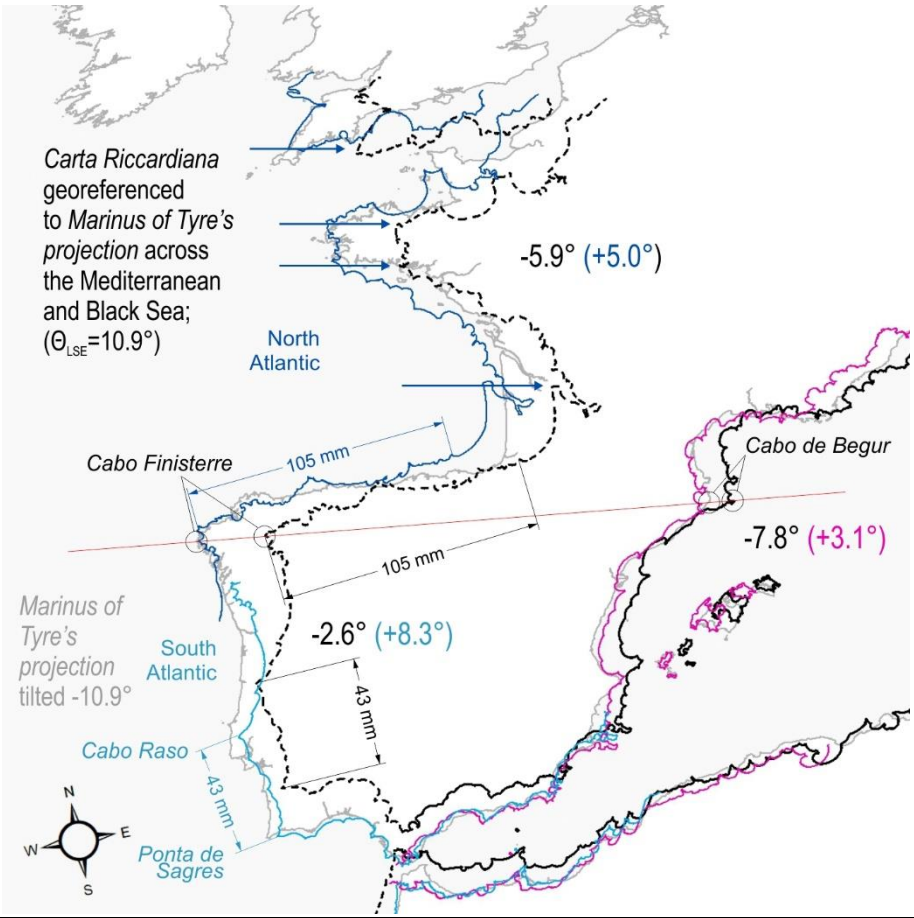


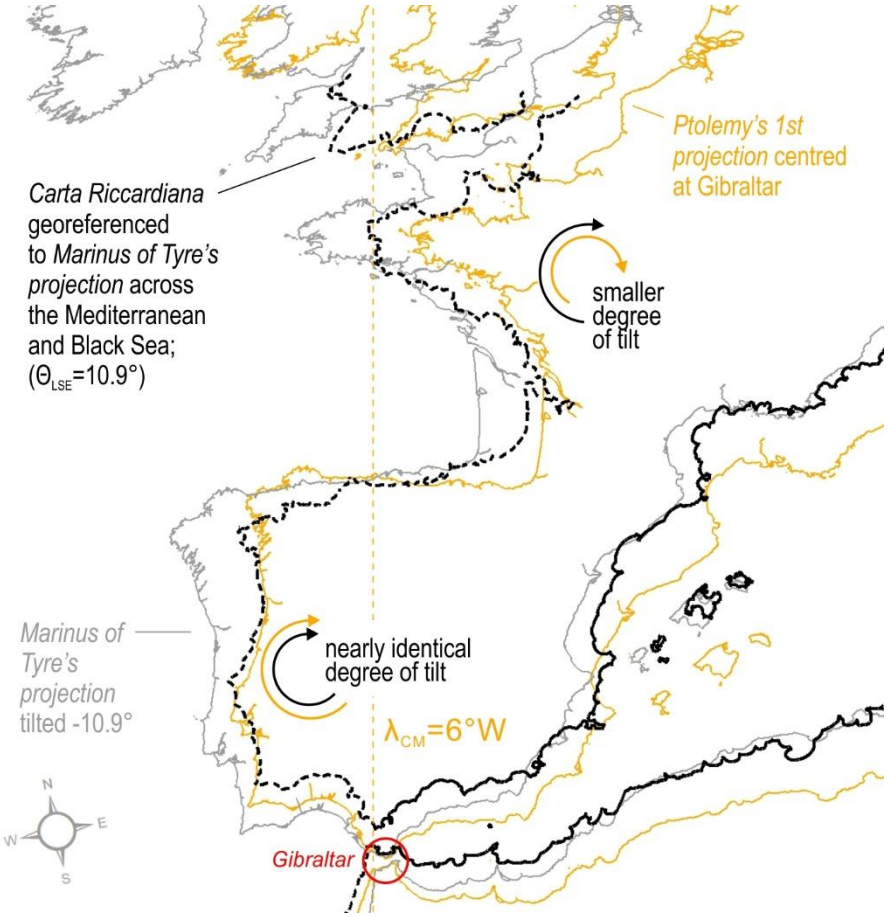
This shifts us away from a computer-assisted methodology and brings us closer to the mindset of the late mediaeval copyist-cartographer, who was likely unaware of the general mechanics of (any) map projection and, consequently, how map scale is distributed across the map field based on (any) specific map projection employed.

Moreover, the author's earlier analysis of portolan atlases (Marelić 2025a) shows that the sheets of atlases made by Pietro Vesconte (1313) and Andrea Bianco (1436) cannot be manually assembled into coherent composites when equalising the lengths of their linear scale bars, and that are significant local and regional differences between Vesconte's portolan atlas and his portolan chart (1311), as well as between Bianco's atlas and his smaller-scale portolan chart bound in the same atlas. This can be interpreted in a sense that despite their efforts to create each chart as accurately as possible, the drafting process itself was significantly conditioned by its manuscript freehand nature (prone to random errors), their copying methods were rudimentary, and their accuracy standards overall were lower than ours.

Therefore, the author contends that dependence on numerical reconstructions is rational to a certain extent; beyond that threshold, no additional insights will be gained, irrespective of the methodological rigour of the numerical analysis. We must adopt the mindset of individuals from the late Middle Ages, who, lacking knowledge of geodesy and cartography and without being independently provided with the correct image of the mapped areas, aspired to visually compile regional representations of the mapped areas into a single cohesive unit.

Next, let's observe the figure below which is NOT INCLUDED IN THE ARTICLE, BUT THE AUTHOR IS NOW CONVINCED IT SHOULD BE ADDED IN THE REVISED VERSION. It clearly shows that – although the North Atlantic coasts of the *Carta Riccardiana* are tilted only -5.9° (+5.0°compared to the overall chart tilt of -10.9° when georeferenced to the equidistant cylindrical map φ_0=36°) – they are nearly identical in both the alignment and the scale to the basemap. In other words, to come to its renderings (black dashed line), its North Atlantic segment (blue line) needs only to be shifted eastward from its appropriate position on the Marinus of tyre's map. Now, this raises questions regarding the rationale behind such a shift, the specific extent of the displacement to the east, and the differential treatment of the South Atlantic, particularly why it is not merely shifted but also rotated and somewhat diminished in scale. The term "seemingly" is deliberately enclosed in brackets to suggest that there may not have been subjected to any intentional scale reduction by the late medieval copyist-cartographer; for instance, the span between Cabo Raso and Ponta de Sagres agrees well with their distance on a modern map in Marinus of Tyre's projection, as well as the remaining coast to the southeast between the Ponta de Sagres and Gibraltar and the Alboran Sea. Millimetres on the figure correspond to the true size of the *Carta Riccardiana*.



		The answer to that question is **likely** provided by the next figure, WHICH IS ALSO NOT INCLUDED IN THE ARTICLE, BUT THE AUTHOR IS NOW CONVINCED IT SHOULD BE ADDED IN THE REVISED VERSION. It shows the *Carta Riccardiana* georeferenced to a modern map in Marinus of Tyre's projection (additionally rotated -10.9° altogether) and overlaid with the modern approximation of Ptolemy's first projection centred at λ=6°W, with Gibraltar used as the contact point for both modern maps. The figure shows that the South Atlantic segment on the *Carta Riccardiana* is nearly identical in tilt to its coasts on the map in *Ptolemy's first projection*, whereas the North Atlantic segment has slightly smaller degree of tilt and is tilted somewhere in between maps in *Marinus of Tyre's projection* and *Ptolemy's first projection*.  Altogether, the figures and information already provided in the initial version of the article, plus this additional sequence of five figures (especially the two last of them) tangibly demonstrate that (A) simple geometric manipulations including shifts and rotations of source maps that were already made in equidistant cylindrical projections are sufficient building blocks of the *Carta Riccardiana*, and (B) the hypothesised use of a map in *Ptolemy's first projection* as a template is a solution that should not only be accepted at this stage of knowledge, but also analysed in greater detail in the future.
23	806-811	The author states that the main shortcoming of his proposed model (hypothesis) is the poorer longitudinal accuracy as compared to the latitudinal accuracy of Ptolemy's data and suggests

	814-816	this is caused by Ptolemy's underestimation of the earth's circumference, "resulting in exaggerated longitude values". Whilst the latter statement is correct, the author overlooks a far more important difference between Ptolemy's data and the geographic data on a portolan chart: a portolan chart's accuracy is much higher and its geographic detail much richer than Ptolemy's dataset, even if Ptolemy had used the correct earth dimensions. This is in the reviewer's opinion a much more significant shortcoming; he omits to mention the stark contrast between the accuracy of the geographic data in Ptolemy's dataset and portolan charts. How would the correspondingly accurate source data (or field data) have been measured in Antiquity?? How would Marinus (or a contemporary or earlier geographer) have converted this assumed data correctly to latitudes and longitudes? *"Ptolemy ungrudgingly acknowledges that the collection of geographical data presented in the* Geography *is substantially the work of Marinos of Tyre"* (Berggren and Jones, 2000, p. 23). How did portolan charts then obtain their much greater detail and higher geographic accuracy??? Addressing these questions goes way beyond the extent of this research which is based only on the modern approximation (maps) of Marinus's and Ptolemy's maps. The reviewer's critique addresses parts between the lines 806 and 814; however, between lines 814 and 835 the author explains those shortcomings in a wider sense, emphasising how little information from classical antiquity has actually been preserved and calling for future a re-evaluation of the geographical work of Claudius Ptolemy as a continuation of R. R. Newton's critique of his book on astronomy.
24	854	The author focuses exclusively on map projection issues and glosses over the issue of geographic data accuracy. He ignores the question on what observed field data "regional maps with high planimetric accuracy" in Antiquity might have been based and why the top geographers of the period did not use such valuable data. The exclusive focus on the "simpler" equidistant cylindrical projection is misleading. How would geographers (or whoever) in Antiquity have converted high-accuracy field data to a map on this projection? A direct conversion would be impossible, so the antique geographers would have to have computed a very dense dataset in latitudes and longitudes from the field data and then committed those to parchment using the equidistant cylindrical projection. This seems an unlikely scenario, but not mentioning it at all is an omission in the article. The reviewer initially critiques the article for its excessive length and subsequently for its omission of an analysis regarding how classical antiquity experts might have generated a substantial quantity of spherical coordinates for mapping. This article cannot address all those enquiries, and presently, neither can the author, who—if they had undertaken such a study—would likely encapsulate it in a subsequent article (not this one). Such a vast array of results warrants the composition of a scientific monograph rather than the condensation of all material into a single article, as this would be counterproductive for the reader. The main purpose of this study was to investigate the possibility of generating the geometry of a map that corresponds to the *Carta Riccardiana* using modern maps (treated as error-free) whose projections match those known from classical antiquity. Finding out how spatial data (spherical coordinates of locations) were exactly acquired in classical antiquity and how original maps were made based on them was not the goal; this is only partially mentioned in section 7.5, page 34, lines 813-820, where several revisions of maps created by Marinus of Tyre and Agathodaimon as a mapmaker for Ptolemy are cited. Ptolemy's *Geography* comprises approximately 8,000 locations, with 6,300 possessing spherical coordinates, suggesting that he primarily derived them from Marinus of Tyre. Regrettably, we lack knowledge regarding how Marinus obtained the thousands

		of coordinates (let's say at least 4000 of the 6300), as well as how Ptolemy acquired the coordinates for each individual remaining locality. This topic pertains to recent research that, I repeat, exceeds the parameters of this study.