CHAPTERS IN THE HISTORY OF TERRESTRIAL MAGNETISM

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CHAPTER II—THE DISCOVERY OF THE MAGNETIC DECLINATION

1—In Chapter I* it was shown that the compass had been brought into use in northwestern Europe for the purposes of navigation by the year 1187, that its first mention in the literature of that year does not refer to it as being of recent introduction, that the discovery of the directive property of a magnet in the Earth's field must have preceded this application by some considerable time, but that of this time it was not possible to frame any reliable estimate.

Until the end of the sixteenth century, and even later, this directive property of the magnet was the subject of much speculation among philosophers, and the ordinary sequence of treatment might lead us to deal next with the results, such as they were, of these enquiries. This, however, will be postponed until we deal with the first step in advance after the introduction of the nautical compass.

2—The four centuries from 1200 to 1600 witnessed the completed transition from the age in which medieval scholasticism was dominant into that when the art and logic of observation and experiment had vindicated their entry on the field of scientific investigation. But we have to wait until the publication, in the latter year, of Gilbert's *De Magnete* before the foundations of the science of terrestrial magnetism were well and truly laid. Meanwhile, one notable advance had been made. For at least three hundred years before Gilbert's time, it had been noticed that the suspended magnet did not, always and everywhere, point to the exact geographical north. At first, this was explained as being due to the lodestone, by which the compass-needle was magnetised, having different properties in different parts; later on, it was attributed to imperfections in the method of magnetising the needle, or to errors in the observation of its direction relative to the geographical meridian. But gradually it came to be recognised that this divergence was a universal phenomenon, and thus was reached the conception of the magnetic declination, the angle at a given place and time between the geographical meridian of the place and the direction of a magnet freely suspended in a horizontal plane.

The question which immediately presents itself is that relating to the origin of this conception—by whom, when, and in what circumstances, this phenomenon of magnetic declination was first observed. But, as in the analogous question of the nautical compass, we have no extant record of any such observation—the earliest references are found to be imperfect and obscure and thus the question cannot be answered in precise terms of persons, dates, and places. In the strict sense of the term, therefore, we cannot speak of the *discovery* of the declination and it would be more correct to refer to the history of the matter as being

*Terr. Mag., 37, 105-146 (1932).

that of the gradual evolution of the conception of declination. This view of the question being adopted, the most suitable method of treatment is indicated. We do not, as in Chapter I, deal with each claimant to the honour of discovery, for this would tend to obscure the historical process, and we proceed to examine the records in their historical order, as far as that can be ascertained.

3—The year 1187 A. D. has been referred to above as the date of the earliest mention in literature of the compass being employed in navigation. For several reasons, the discovery of the directive property of a suspended magnetic needle must have preceded that year by a considerable time. First, because some interval would necessarily elapse before that property was applied to the construction of a primitive compass. Second, it is not referred to, when first mentioned, as a novelty [1]. Third, the compass is found in use in northwestern Europe by 1187, in the Mediterranean by 1204, in the Indian Ocean (or the Persian Gulf) by 1220, and in China at some uncertain, but slightly earlier date. In whatever part of the world this extension originated, the process must have occupied many years. Any estimate of this interval must be conjectural and all that it is possible to say is that the stage was set ready for the recognition or observation of the declination as soon as the directive property of the magnet had been discovered.

There is, however, one claim made for an observation of declination at a date very much earlier than can be easily credited. In 1897, Wylie stated [2] that I-shing, a Buddhist priest and Chinese Imperial astronomer who lived about 700 A. D., was acquainted not only with the directive property of the magnet but also with the fact of its direction in China being eastwards from the geographical meridian. As already shown in Chapter I [3], Wylie gave no exact reference to any original source of his information and although it has been searched for by Hirth [4] and Hashimoto [5], nothing of the kind has been found. One of the many curiosities in the early history of the subject is that while Wylie was exceedingly careful in giving precise references in all his discussions on Chinese literature, he gave none at all in this instance. The principal facts in the life and work of I-shing are given in the *T'ang-shu*, in which Hashimoto [6] found a passage bearing a certain resemblance to the quotation made by Wylie. But this passage has no connection with the magnet and Hashimoto concludes that Wylie translated it incorrectly. Pending further investigation, this supposed mention of the declination must be passed over as having no significance for the present purpose.

4—Another possible reference to magnetic declination also comes from China. It is based on a passage in the Chinese cyclopedia *Mung-khi-py-than*, written by Shonkua, who lived 1030-1093 A. D. It has been translated as follows: "A geomancer rubs the point of a needle with the lodestone to make it point to the south, but it will always deviate a little to the east, and not show the south; that to use the needle, it may be put on water, but it would not be steady; and also it may be put on the nail of a finger or on the lip of a bowl, but it is too apt to drop, because its motion is very brisk; that the best method is to hang it by a thread, and to prepare the contrivance, one has to single out a fine thread from a new skein of floss silk and fix it with a piece of beeswax

on the middle of the needle, the latter to be hung up where there is no wind; that the needle would then always point to the south; that, on rubbing a needle with a lodestone, it may happen by chance to point to the north, and he (the author) owned needles of both sorts, and that no one could as yet find out the principle of it" [7].

This passage, or its substance, also appeared in the supplement [8] to the same cyclopedia, and also in later Chinese works. It is generally on these latter that commentators have based their conclusions [9], its first sentence being quoted, not invariably with its important context, in support of the claim now under reference. But careful consideration of the whole passage proves that the author's attention is concentrated on the transmission to the needle of the directive property and on the demonstration of that property. Further, the deviation from the meridian is not regarded as a separate physical phenomenon but as an accidental result due to imperfect support or suspension of the magnetised needle. The passage cannot, therefore, be accepted as proof of Chinese knowledge, in the eleventh century, of the magnetic declination. In support of this conclusion, the experience of Father Ricci and other Jesuit missionaries in China, about the beginning of the seventeenth century, may be quoted. They were permitted to take part in the proceedings of the Chinese Tribunal of Mathematicians, and had considerable difficulty in persuading the Tribunal that the geographical and magnetic meridians were not coincident. It was only after actual demonstration of the divergence of 2° between them, that the Tribunal accepted the fact of declination. In 1696, Le Comte reports [10] that the Chinese were still firmly persuaded that the magnetic needle always pointed to the south. In 1870, Amiot [11] found that the Chinese still accepted the declination at Pekin as being 2°, thus showing that they obtained their first knowledge of the matter from Father Ricci's demonstration. Another piece of evidence is the fact that when the Chinese used the magnetic needle to determine direction, they did not take the declination into account. Thus, the east and west walls of Pekin, built in the time of the second emperor of the Ming dynasty, do not run exactly north and south, but deviate 2½° from that direction [12].

Hashimoto [13] claims to have found a reference to magnetic declination in a passage in the *Tung-hua-lu*, written towards the end of the twelfth century. The passage is exceedingly obscure and almost untranslatable in modern terms. That it refers to the lodestone or the magnet, or to magnetic declination, is certainly open to question.

From what has been stated above, the conclusion necessarily follows that our knowledge of magnetic declination did not originate in China.

5—From China, therefore, we have to come to Europe. Very soon after its first mention by Neckam in 1187, several writers refer to the compass in one or other of its primitive forms but none of them appears to know of any departure from the rule, then accepted, that the magnetic needle pointed to the north or to the pole star. The earliest reference to any phenomenon of this kind is to be found in the *Opus Minus* of Roger Bacon, written in 1266. The passage in question is printed in full in the appended notes [14]. In explanation of the behaviour of a suspended magnetic needle, Bacon puts forward the theory that a lodestone resembles the Earth in having north, south, east, and west

parts—a curious anticipation of Gilbert's main thesis of 1600—and that if the needle is touched by any one of these parts, it tends to rest in a direction pointing to the corresponding part of the sky. Whether, or in how far, this theory was peculiarly Bacon's own, is not quite clear, but at all events it appears to have persisted down to, or to have reappeared in, the sixteenth century, and we shall meet with it again in dealing with the historical data of that period. Bacon also criticises those philosophers who, by attributing the virtue of the magnet to the pole star, imagine that it must necessarily point to the north, and thereby neglect the possibility of its pointing in any other direction.

The question now arises whether Roger Bacon (or some contemporary) framed this theory to fit the facts of observation, or whether it was merely one of the many speculations of the age. Against the former alternative, it can be urged that no observational fact is quoted in support of the theory. On the other hand, the bare idea of variation of the needle from the north was entirely foreign to the general trend of thought in that age and must have been suggested by some objective phenomenon. That Bacon did not recognise declination as a universal phenomenon is perfectly clear. The curious point is that although he has been referred to as the father of experimental science, he did not, in this case at least, subject his theory to the test of a few simple experiments. Had he done so, they would have led to two results—one, certain, the other possible. The first would have destroyed his theory; the second might have established the fact of magnetic declination.

In connection with Roger Bacon, it was noted in Chapter I, that many writers have been sadly misled by the forgeries of Dupré [**15**] who represented Bacon as demonstrating the properties of the magnet to Brunetto Latini during a visit to Oxford.

6— Contemporaneous with Roger Bacon and his *Opus Minus*, we have the famous *Epistola* of Petrus Peregrinus, written in 1269 [**16**]. For many years it was supposed to have provided the earliest evidence of a knowledge of the magnetic declination. In 1681, Thévenot made the following statement: "On a cru jusques à cette heure, que la déclinaison de l'Ayman n'a commencé d'estre observée que vers le commencement du dernier siècle. Cependant j'ay trouvé qu'elle varioit de 5 degrez l'an 1269, c'est dans un manuscrit qui m'est tombé entre les mains, avec ce titre *Epistola Petri Adsigerii in super rationibus naturae Magnetis*" [**17**]. Thévenot gave no further particulars as to the origin, character, or location of this document, and this, in part, accounts for his assertion remaining unchallenged for nearly one hundred and fifty years. Writers who came after him did little more than repeat his statement, with or without his name as authority. But there is evidence that this was done with reluctance in some cases [**18**]. In 1800, Cavallo published a translation of the more important parts of the manuscript, having meanwhile discovered it in the library of Leyden University. But he added the significant note that the part relating to the value of the declination appeared "as if it were a note or observation not belonging to the text" [**19**]. Finally, in 1835, Wenckebach published an article which explained the matter fully [**20**]. He showed: (1) That the Leyden manuscript is a fifteenth century (possibly later) copy of a genuine manuscript entitled *Epistola Petri Peregrini de Maricourt ad*

Sygerum de Foucancourt militem, de magnete; (2) that the title of the Leyden manuscript, as given by Thévenot, had been derived from that of the genuine original by the omission of three words and the erroneous conjunction of the "ad Sygerum"; (3) that several copies of the genuine *Epistola* existed in various libraries, but that none of them contained the passage on the magnetic declination. He concluded that the passage in question was an interpolation [**21**]. Bertelli also contributed an elaborate bibliographical study of the *Epistola* [**22**].

The sad fact remains that although this error has been exposed for a century, it has been repeated even in recent times and by some most eminent authorities [**23**].

Recently, Winter [**24**] has put forward the proposal that while Peregrinus may not have known of the declination, it was nevertheless known in his time. Winter's argument is somewhat involved, and the present writer has had difficulty in grasping it. It would seem, however, that he seeks to show that inasmuch as the lodestone compass described in Part II, Chapter I, of Peregrinus' *Epistola* was in reality a compass in which the declination was allowed for, after the manner of the Flemish compasses of the early sixteenth century, it follows that the declination must have been noticed by the time Peregrinus wrote. That there are indications pointing to this conclusion being correct, has already been stated above. But I regret being unable to accept Winter's reasons for it, as drawn from the *Epistola*. The text of Peregrinus is, admittedly somewhat obscure and there may be legitimate doubts, as suggested by Winter, as to the authenticity of the figures which accompany some of the translations. But taking the rendering of the relevant passages as given either by Hellmann, or Sylvanus Thompson, or Bertelli (who collated four manuscripts, along with variants in the readings by Gasser, Libri, and D'Avezac) there is to be found no substantial support for this proposal.

7—So far as records bearing on magnetic declination are concerned, the fourteenth century is nearly a complete blank. The only writer in this period who alludes, perhaps distantly, to the subject is Henry of Hesse, otherwise known as Henry of Langenstein, who lived from 1325 to 1397. He graduated at Paris in 1363, was appointed Professor of Theology at Vienna about 1382, and wrote, among other works, a treatise on astrological matters entitled *Contra Coniunctionistas*. The exact date of this work is uncertain, but most probably it appeared soon after 1373. It contains a passage which runs as follows: "Item in partibus Norweie magnes in uno situ trahit ferrum et in alio propinquo (?) non." This has been represented by Thorndike [**25**] as evidence that Henry of Hesse "was acquainted with the variation of the magnetic needle near the north pole." It appears to the present writer that this conclusion is wider than is justified by Henry's remark and that it would be safer to say that what Henry had observed, or had reported to him, was some abnormality due to local attraction of masses of magnetic iron-ore. In any case, it is evidence that even in 1373 such differences had been observed, and this is a matter of some importance.

8—After Henry of Hesse, another gap of possibly half a century or more brings us to evidence of an entirely different kind, and which at one time was widely accepted as being of fundamental importance in

the history of terrestrial magnetism, In 1780, Formaleoni found in the library of St. Mark at Venice, a map or chart prepared by Andrea Bianco and dated 1436 [26]. According to Formaleoni [27], magnetic declination had been observed by Venetian navigators early in the fifteenth century and this knowledge enabled them to correct their charts in so far as these depended on compass-bearings. As evidence of this—but, be it noted, the only evidence—he refers to a figure printed on Bianco's map. It shows one set of lines radiating from the north point of a compass-card, and a corresponding set radiating from a point about 22½° *west* of the north point. He suggested that this angle represented the declination.

Formaleoni's conclusions from Bianco's map, or diagram, were accepted by Humboldt [28] and Libri [29] and also in more recent times by Mottelay [30] and Bauer [31]. But they are no longer credited, especially after Bertelli's criticisms of 1868 [32] and 1892 [33]. It has to be noted that the declination indicated by the figure in Bianco's map is shown as westerly, whereas all available information on the subject shows that the declination over Europe in 1436 was easterly. The matter has again been dealt with recently by Heathcote [34]. The general conclusion is that while the exact purpose of these sets of radiating lines is not perfectly clear, Bianco's map does not show the declination and that the figure in question has no relation thereto.

Another early map is that referred to by Libri [35], and is now in the Bibliothèque Nationale, Paris [36]. It forms part of a manuscript containing the poem *La Sfera* by Goro Dati [37], and is referable to the early part of the fifteenth century. But that it contains, as Libri stated, an indication of the declination is extremely doubtful. It includes a diagram in which an arrow is placed near (but not exactly over) the north point of a compass-card. But it is fairly clear that this unsymmetrical position of the arrow may have been due to carelessness in drawing and not to design. Indeed, cases have been found in which the arrow inclines sometimes to one side, sometimes to the other, of the north point. Hence this map cannot be accepted as bearing any evidence of a knowledge of declination.

9—For a considerable body of valuable evidence drawn from still another source, we are indebted to the labours of Hellmann [38] and Wolkenhauer [39]. This is based on the fact that about the middle of the fifteenth century, if not earlier, portable sun-dials were in use, chiefly by travellers, which had attached to them, in the base of each instrument, a small compass which facilitated the correct adjustment of the instrument with its "noon-line" in the geographical meridian. Some of these compasses show a mark indicating the amount of the declination. As the papers of Hellmann and Wolkenhauer supplement each other, it may be convenient to summarise them together.

After calling attention to the manufacture of these portable sun-dials, chiefly at Nuremberg and Augsburg, Hellmann begins his argument by giving sound reasons for the belief that the word "compass" or "compassus" meant nothing more than a horizontal sun-dial provided with a magnetic needle [40]. At what date the manufacture of these instruments began in Nuremberg and Augsburg is unknown. Peuerbach, one of the early authorities on dialling, lectured at Vienna from 1454 to

1460. Among other writings, he left behind two documents [**41**] bearing on the present subject, namely, a pamphlet entitled *Canones Gnomonis cum nova tabula* and a manuscript entitled *Compositio Compassi cum regula ad omnia climata.* His pupil, Johannes Müller, better known by his latinised name, Regiomontanus, settled in Nuremberg in 1471 and carried on astronomical work until shortly before his death in 1476 [**42**]. He made sun-dials, and either introduced their manufacture to Nuremberg or, more probably, encouraged their manufacture if that had already been begun.

The next step in Hellmann's argument is to suggest that any maker of sun-dials, with their attached compasses, who was acquainted with the ordinary method of fixing the geographical meridian, must, in course of time, have recognised that, with the noon-line of the instrument in that position, the magnetic needle did not point exactly north but (in Germany at that time) to the east of north. To adjust the sun-dial correctly, it would then be necessary to orient it with the noon-line to westwards of the direction of the magnetic needle. The procedure would then be to mark on the compass-dial a point corresponding to the deviation of the needle and to orient the instrument until the needle stood over that point. The north point of the instrument would then indicate the geographical north. In whatever manner the process was gradually evolved, the fact remains that sun-dials have been found which show quite clearly the difference between the geographical and magnetic meridians, generally by a mark on the compass-dial to the east of the north point. A letter written by Hartmann [**43**] in 1544 shows that he had observed the declination at Rome in 1510 and found it to be 6° east, whereas it was 10° at Nuremberg. This observation—the earliest of its kind on land—was very probably made with the compass of a portable sun-dial, for it is known that Hartmann was an expert in the construction of these instruments [**44**].

With regard to these sun-dials, Hellmann referred to the instrument found by Le Monnier in Paris [**45**]. It was constructed by Bellermatus, is stamped with the year 1541, and shows the magnetic meridian making an angle of about 7° east with the north point [**46**]. Presumably, this relates to Paris. Wolkenhauer was able, however, to go much further back [**47**]. He called attention to a sun-compass found by E. Mayer [**48**] in the Spitzer collection of antiques at Paris. It is dated 1453 and shows a deviation of the needle from the noon-line. Other instruments of similar kind found by Wolkenhauer are of dates 1451, 1456, 1470, and 1511, all of them showing an easterly deviation of the magnetic needle [**49**]. Finally, there are good reasons for believing that they had been constructed at Nuremberg.

In confirmation of the conclusions towards which the foregoing evidence points, Wolkenhauer referred to certain early road-maps of Germany which were, either directly or indirectly, the work of Erhard Etzlaub of Nuremberg [**50**]. Of these maps, six copies are still extant. Each has at the side or bottom the figure of a compass, similar to that attached to a portable sun-dial, showing an eastward deviation by about 11¼°. That this deviation was clearly recognised at the time is proved by the instructions, printed at the top of the map, for the use of the compass in travelling by the map. It is generally believed that these maps were issued about 1492.

10—Since the beginning of the nineteenth century, the most general opinion regarding the discovery of the magnetic declination has been that it was first observed by Christopher Columbus during his first voyage to the West Indies in 1492. Until 1790, the only record on which this claim could be based was the biography of Columbus, written by his son Fernando. After that date, Navarette, Humboldt, and, especially, Bertelli, founded it on the Las Casas edition of the *Journal* of Columbus. Later opinion inclined towards the view that the declination was known in western Europe before 1492, and that what Columbus may be credited with is the discovery that its amount diminished, became zero, and then turned westwards, as he sailed westwards across the Atlantic. But even this restricted conclusion has been disputed on the ground that the documents upon which it is based are unreliable.

There is no intention here of embarking on the wide and angry seas of Columbian controversy. But the main results of that discussion have an important relation to the matter now under consideration and it is impossible to avoid their impact upon it. Two other reasons may be adduced in justification of the detailed examination of a claim which would, at this stage, appear to have been disposed of by what has already been stated. The first is that the Columbus claim has been put forward so strongly and in such detail by Bertelli, backed by all the resources of an extensive and profound knowledge of the literature of the subject, that his argument cannot be ignored. The second is that a fresh examination of the original documents, which are reproduced below, throws new light on some aspects of the question. At the same time, an attempt has been made to condense the discussion by acceptance of decisions upon extraneous matters when made by competent and reliable scholars. It will also be noted that, as far as possible, the question at issue is examined independently of any historical evidence already given.

11—The original sources on which the Columbus claim is ultimately founded are: First, the biography [51] of Columbus written by, or ascribed to, his son Fernando; second, the *Journal* of Columbus during his first voyage of 1492, edited or summarised by Bartolemé de Las Casas [52]. But before proceeding to any examination of these sources, two very important questions must be considered. The first is, as to what extent Columbus was, by education and training, of such attainments as would qualify him to understand the various cosmographic and geophysical questions involved in his project or raised in the course of his voyage. The second is, how far the authors or editors of the original sources mentioned had opportunity of acquiring exact information as to the doings of Columbus, and whether they were qualified to set out that information in a manner at once precise, accurate, and intelligible.

First, then, as to Christopher Columbus himself. The information available regarding his early life is remarkably scanty; most conclusions on such matters must be the result of inference and seem destined to remain in dispute. But it has been established with tolerable certainty that he was born at Genoa [53], probably in 1446 [54], and was the son of an artisan in very humble circumstances [55]. There is no evidence that in his boyhood he received any better education than that usually provided for the children of Genoese artisans, if indeed any institution

for the purpose were then in existence. The statements made by his son and by Las Casas that he attended the University of Pavia cannot be accepted [**56**]. At fourteen years of age, he went to sea, possibly as a sailor, or more probably in some capacity as a trader. How he was employed between that date (1460) and the year 1471, when he arrived in Portugal, is either unknown or at best uncertain. But his own statement that he was engaged as a naval commander under King Rene of Provence is demonstrably untrue [**57**]. Nor can the story of his presence at the sea-fight off Cape St. Vincent be accepted, for it has been proved that this took place ten years after he had settled in Portugal [**58**]. With regard to his educational attainments after reaching manhood, there has been wide divergence of opinion [**59**]. But it can be definitely proved that his knowledge of Latin was, at best, rudimentary [**60**] and in consequence he could not have made that profound study of the science of his time, such as it was, that he claims to have made. It is also probable that he could not write Italian [**61**]. There is no evidence that he was in a position to form any reasoned or independent judgment on cosmographical or geophysical questions. When he did express opinions on such matters, his views had no scientific basis and must have been regarded as absurd, even by the limited knowledge of his own day. His science, if it can be so called, was strongly blended with a religious mysticism which only served to confuse important issues and to lead him into speculations of an extravagant kind [**62**]. The most just estimate we can frame of his general capacity is to represent him as a man, probably of much natural intelligence of an untutored kind, capable of persistent effort, but certainly of no special skill or merit in such matters as navigation, astronomy, or cosmography [**63**]. Without stressing the value of negative evidence, it is probable that with regard to terrestrial magnetism, as then understood, he had no closer acquaintance than the average pilot of his time. If this view be correct, he was not in a position to place any intelligent interpretation on the variations exhibited by compass-needles during the crossing of the Atlantic. Lastly, the character of Columbus has been the subject of widely varying opinion. On the one hand, volumes have been written in support of his canonisation as a saint [**64**]; on the other, he has been denounced as a mendacious impostor [**65**]. As is usual in such cases, the truth lies in neither of these extreme directions. But it has to be admitted, and with regret, that Columbus was prepared at times to represent himself as something other than he was in reality [**66**], to resort to stratagem of a dubious kind in order to secure his end [**67**], and to sacrifice truth in the interests of expediency [**68**]. The venial sin in the world of political chicane has no place in the pursuit of scientific knowledge, and the fact that such charges can be laid to his account must tend to lower the credit of Columbus as an observer and recorder of natural phenomena, and must demand the utmost caution in accepting his statements [**69**].

12—Second, as to Fernando Columbus, his son. He was born in 1488, attended school at Cordova, and acted as page to Prince Juan of Spain. When fourteen years of age, he accompanied his father on the fourth voyage (May, 1502, to November, 1504) [**70**]. In 1509, he accompanied his brother Diego to San Domingo, returning in about six months. By the year 1511, he had settled at Seville and had begun the collection of his famous library, which is said to have included 20,000 volumes.

and of which he prepared more than one descriptive catalogue [71]. In later years, he travelled extensively throughout Europe. In 1526, he acted as president of a commission of cosmographers and navigators appointed to undertake the correction of marine charts and had also to deal with the examination and licensing of pilots. From all this we may conclude that he was a man of studious habits and that he was in touch with the nautical science of his country and his time. But whether he was conversant with that science as studied or practised in other countries is a point on which we have no information. He had access to his father's papers and records, and perhaps was as capable as most men of his time of understanding their contents. But his manner of giving these expression leaves much to be desired by the modern reader of his *Historie*, for it is frequently vague and occasionally confused. This may be attributed, in part, to the fact that he had to deal with new phenomena which had not as yet found their systematic explanation. It may also be due to later interpolation or to faulty translation, but of this we cannot judge, as will be explained below.

13—Third, with regard to Bartolomé de las Casas [72]. He was born at Seville in 1474. His father accompanied Columbus on the second voyage (September, 1493, to June, 1496). In 1492, he obtained the degree of licentiate at Salamanca University. Several writers state that he sailed with Columbus on the third voyage (May, 1498, to November, 1500), but this is incorrect. His first visit to the West Indies was probably in 1502, when he accompanied Ovando, who had been appointed governor. While in Cuba, where he entered the priesthood, he was a witness of the brutal atrocities committed by the Spanish colonists on the natives and he became convinced of the utter injustice of the *repartimiento* system by which the latter were enslaved. The rest of his life was almost wholly directed towards the amelioration of these conditions, in the face of continual opposition by interested officials and in spite of the detestation of many of his countrymen. Retiring in 1547 from his bishopric of Chiapa, which he only held for three years, he spent the remaining years of his life at Valladolid in the completion of his *Historia de las Indias* [73] It had been begun in 1527 but he was still enaged on it in 1561, his eighty-seventh year. He left the manuscript to the college of San Gregorio, Valladolid, with instructions that it was not to be published for forty-five years. But it was not until 1875 that it saw the light. The *Historia* contains ample evidence that he had access to many original sources of information, that he must have had before him some written account of the first voyage of Columbus, and that he had borrowed largely from the biography by Fernando Columbus. It must be emphasised, however, that the *Historia* is a special plea in support of Las Casas' main thesis—the ill-treatment of the West Indian natives by the Spanish adventurers. The author is not concerned with, and was probably unable to understand, problems connected with cosmography and geophysics.

14—We have now to turn to the literary history and character of the original sources upon which we have to depend, and these are sufficiently curious. In 1571, seventy-nine years after Columbus made his first voyage, and sixty-five years after his death, there appeared in Venice an Italian translation by Alphonse Ulloa of Fernando Columbus' biography of his father [74]. Of the original from which this translation was

made, absolutely nothing is now known, for it has never been found, either in manuscript or in print. The authenticity of this work has been severely attacked by Harrisse [75] and has been defended by D'Avezac [76] and others [77]. Whatever be the ultimate decision as to authorship and date, it is clear that the writer had before him some more or less systematic account of the voyages of Columbus. But up to the time of publication of Ulloa's translation, and indeed for more than two hundred years afterwards, no such account was known. And this leads to the discovery of the *Journal* of Columbus. In 1789, Charles IV of Spain conceived the idea of establishing at Cadiz a library devoted to naval and maritime affairs, and commissioned Don Martin Fernandez de Navarette to examine public and private libraries in order to secure copies of books and manuscripts bearing on such matters. While thus engaged, Navarette found [78] in the archives of the Duke of Infantado two manuscript copies of the *Journal* maintained by Columbus during his first and third voyages. One of these manuscripts is in the handwriting of Las Casas, the other is in a later hand. Otherwise, they are identical. They were not published until 1825 [79].

The real difficulties involved in these original sources now begin. With regard to the *Journal*, the Las Casas manuscripts are not simple transcripts of a single original. In some passages, they profess to give the exact words of Columbus. The remainder consists of an abstract or précis, that is, Las Casas' own edition of what happened or of what he supposed did happen. The first fundamental difficulty therefore is that we are entirely unable to prove that what Las Casas left behind is an accurate reproduction, either in the exact words or in the true sense of the original. With regard to that original, it has never been found. There is reason to believe that Columbus sent it, or at least the earlier part of it, to the Spanish sovereigns soon after returning from his first voyage; that the original was retained and a copy returned to Columbus; that this copy was afterwards amplified or edited with certain corrections, and that this corrected copy was retained by Columbus [80]. But the copies used by Las Casas in the compilation of his *Historia* must have been different from those used by Fernando in his *Historie*, for these two works disagree in certain particulars. Hence the best we can say for the Las Casas manuscripts is that they consist partly of a transcript and partly of an abstract of a copy of a copy of the original. Indeed, the derivation may have been, and most probably was, even more remote. There has therefore been abundant room for errors in transcription, and for alteration and interpolation. They are certainly not documents which can be accepted without the most serious reservation. Hence, to say with Thacher [81] that every word of the *Journal* "was in the Admiral's proper hand," or with Markham [82] that Columbus "diligently wrote his *Journal* until the day of his return to Palos," is to represent matters incorrectly and to assert what cannot be proved. Lastly, the *Journal*, as we have it from Las Casas, is exceedingly unsatisfactory from the point of view of the scientific navigator, as Lord Dunraven has shown [83].

Second, it will be found that the early authorities—the Las Casas manuscripts, Fernando Columbus' *Historie*, and Las Casas' *Historia*—are not always in agreement as to essential matters. This will be dealt with in detail in what follows.

Third, the Las Casas manuscripts have suffered at the hands of transcribers and translators. When he published these manuscripts in 1825 [84], Navarette, for some reason now unknown, made alterations in the text and these have been accepted without question, except in one instance, down to the present time. Some of these alterations make radical changes in the record of observations made by Columbus, and have given rise to much discussion and to a certain amount of futile speculation. It is now necessary to go back to the original manuscripts of Las Casas in order to ascertain the exact character of the statements made therein; to assist the reader in forming his own conclusions, photographs of the relative portions of these documents, now [85] in the National Library, Madrid, have been obtained and are published here for the first time [86]. The necessary corrections in the generally accepted version will be noted in their appropriate place below. It will also be found that the *Historie* of Fernando Columbus has suffered in translation.

15—With this preliminary sketch of the materials at disposal, and of the authors or editors connected with them, the passages which refer, or are supposed to refer, to observations made by Columbus on magnetic declination may now be considered. They consist of four from the Las Casas manuscript of the *Journal*, five from the *Historie* of Fernando Columbus, and two from a letter of Columbus. In each case, the language of the original is given, along with a parallel translation into English. The passages headed *A*, *B*, *C*, *D*, are from Las Casas' manuscripts; *E*, *F*, *G*, *H*, *I*, are from the *Historie* of Fernando Columbus; and *J*, *K*, are extracts from the letter of Christopher Columbus.

A

Domingo 9 de Setiembre

Los marineros gobernaban mal, decayendo sobre la cuarta del norueste [87], y, aun a la partida, sobre lo cual les riñó el Almirante muchas veces.

Sunday, 9th September (1492)

The sailors steered badly, falling off to the northwest quarter, and even to one-half of the quarter, for which the Admiral reprimanded them on many occasions.

B

Jueves 13 de Setiembre

En este día, al comienzo de la noche, las agujas noruesteaban, y, a la mañana nordesteaban [88] algun tanto.

Thursday, 13th September (1492)

On this day, at the beginning of the night, the needles declined to the northwest, and in the morning they declined a trifle to the northeast.

C

Lunes 17 de Setiembre

. . . tomaron los pilotos el Norte marcándolo, y hallaron que las agujas noruesteaban una gran cuarta, y temían los marineros, y estaban peñados y no decían de que. Conociólo el Almirante, mandó que tornasen a marcar el Norte en amaneciendo, y hallaron que estaban buenas las agujas; la causa fué porque la estrella que parece hace movimiento y no las agujas [89].

Monday, 17th September (1492)

. . . the pilots took the position of the North Star, marking it, and they found the needles declined to the northwest a good quarter, and the sailors were afraid and were troubled and did not say for what reason. The Admiral took cognisance of this fact and ordered them to take the position of the North Star again at dawn, and they found the needles were good. This was because the står which appears, moves, and the needles do not.

D

Domingo 30 de Setiembre

Nota: que las estrellas que se llaman las guardias, cuando anochece, están junto al brazo de la parte del Poniente, y cuando amanece están en la línea debajo del brazo al Nordeste, que parece que en toda la noche no andan salvo tres líneas, que son nueve horas, y esto cada noche; esto dice aquí el Almirante. También en anocheciendo las agujas noruestean [90] una cuarta, y en amaneciendo están con la estrella justo; por lo cual parece que la estrella hace movimiento como las otras estrellas, y las agujas piden siempre la verdad [91]

Sunday 30th September (1492)

Note: that "the stars which are called the guards, when night falls, are near the arm in the west, and at dawn they are on the line below the arm to the northeast, as it appears that during the night they do not go more than three lines, which are nine hours, and this each night." The Admiral says this here. Also at nightfall the needles decline to the northwest one quarter and at dawn they are exactly in the direction of the North Star; by which it appears that the North Star moves as do the other stars and the needles always demand the truth.

The five passages from the *Historie* of Fernando Columbus are as follows. The text is taken from Caddeo's edition [92].

E

Capitolo XVII

Ma, essendo poi corsi altre cinquanta leghe verso Ponente, a XIII di Settembre trovò che da prime notte norvesteavano le calamita d' bussoli per mezza quarta, e l'alba norvesteava poco più d'altra mezza; da che connobe che l'agucchia non andava a ferire la stella che chiamiam Tramontana, ma un altro punto fisso e invisible. La qual varietà fino allora mai non aveva conosciuto alcuno; e però ebbe giusta causa di maravigliarsi di ciò. Ma molto più si maraviglio il terzo dì, nel quale era già corso quasi cento leghe più avanti pur per quel paraggio; perciocche le agucchie da prima notte norvesteavano già con la quarta; e la mattina tornavano a percuotere nella medisima stella.

Chapter XVII

On 13th September (1492), he found that at nightfall the needles of the compass varied half a quarter to the northwestwards, and at break of day half a quarter more; by which he understood that the needle did not point at the North Star but at some other fixed and invisible point. This variation no man had observed before and therefore he had occasion to be surprised at it. But he was more amazed the third day after, when he was almost 100 leagues further, for at night the needles varied about a quarter to the northwest, and in the morning they pointed to the star [93].

F

Capitolo XIX

Nè però, quantunque l'Ammiraglio ponesse mente a tutti questi segni, si scordava di quelli del cielo, nè i corsi delle stelle. Laonde in quel paraggio notò con grande ammirazione che di notte le guardie stavano giustamente nel bracchio dell'occidente; e, quando aggiornava, si ritrovavano nella linea sotto il braccio a Nordeste, da che comprendeva che in tutta la notte non camminavano se non tre linee, che sono nove ore; e questo provava egli ogni notte. Parmiente notò, che da prima notte le agucchie Norvesteavano per tutta una quarta, e, quando aggiornava, stavano giustamente con la stella. Per le quali cose i piloti erano in grande affamo e confusione, fiu che egli loro disse di ciò essere cagione il cerchio, che

Chapter XIX

Although the Admiral took note of all these signs, he did not forget the signs of the sky or the course of the stars. He therefore noted with great admiration that at night the "pointers" stood exactly on the arm of the west, and when it was dawn they were found in the line under the arm to the northeast, from which he understood that during the night they only moved three lines which are nine hours, and this he proved every night. He likewise noted that at the early evening (30 September) the needles northwested a whole quarter, and at dawn they were exactly with the star. At this, the pilots were in great anxiety and confusion, until he told them that the reason was the revolution of the Pole Star round the

la stella Tramontana fa, circondando il Polo; il quale avvertimento diede lor qualche conforto; perciocchè in fatti per cotai differenze temevano di pericolo nel cammino, in tanta distanza e diversità di paesi.

Pole; which news gave them some comfort, because such differences made them afraid of danger in such unknown regions.

G

Capitolo LXIII

Questa mattina le aguglie fiammiglie norvestavano, come sogliano, una quarta; e le genovesi, che solevano conformarsi con quelle, non norvestavano so non poco; e per l'avvenire hanno a norvestare andando il Leste, che è segno che ci ritroviamo cento legha alquanto più all'occidente delle isole de gli Astori; perciocche, quando furono appunto cento, allora era in mare poca ciba di ramuscelli sparsi, e le aguglie fiammiglie norvestavano una quarta, e le genovesi percotevano la Tramontana; e, quando saremo più al Leste nordeste, faramo alcuna cosa. Il che si verificò subito la Domenica seguente a XXII di Maggio. Dal quale indicio, e dalla certezza del suo punto conobbe allora che si ritrovava cento leghe lontano dalle isole de gli Astori; di che egli si maraviglia, e attribuisce la cagione alla differenza della calamita, con che si temperano le aguglie; perciocche fino a quella linea tutte norvestano una quarta; e quivi le une perseverano, e le altre, che sono le genovesi, percuotono giustamenta la stella. E ancor si verificò il medismo il seguente giorna a XXIIII di Maggio.

Chapter LXIII

This morning (20 May 1496) the Flemish compass-needles northwested a quarter as usual, and those of Genoa, which used to agree with them, did not except slightly, but after sailing eastwards they northwest more, which is a sign that we are 100 leagues or somewhat more to the west of the Azores; for when we were just 100, there were but few scattered weeds in the sea; and the Flemish needles northwested a quarter, those of Genoa pointing to the North Star; and when we were somewhat further east-northeast, they will alter again, which was verified on the Sunday following, 22 May; by which, and the certainty of his position, he found he was 100 leagues from the islands of the Azores, which he was surprised at, and assigned this difference to the several kinds of lodestone by which the needles are made; for till they reached that line, they all northwested a quarter, and there some held it, those of Genoa cutting the north point exactly. This was most clearly verified on the following day, the 24th of May.

II

Capitolo LXVI

E, quanto al norvestare, io credo che la stella abbia la proprietà dei quattre venti, come l'ha ancora la calamita; che, se toccano col Levante, dimostrerà il Levante e altresì il Ponente, o il Settentrione, o l'Ostro; e però colui che fa le aguglie copie con panno la calamita in modo che non resti di fuori, eccetto che la parte settentrionale, cioè quella che ha virtù di condurre l'acciaio a percotere la Tramontana.

Chapter LXVI

As to the northwesting, I believe that the star has the properties of the four winds, as has the lodestone; that when it touches the east, it will point to the east, and in like manner the west, north, and south; and for that reason, he who makes the compass-needle covers the lodestone with a cloth, all but the north point of it; namely, that which has the virtue of making the steel point to the north.

I

Capitolo LXII

Medisimamenta dice, che quella stessa notte, che fu il Giovedì a XVI di Agosto non avendo fino allora norvestato, le aguglia norvestearono in fretta più d'una quarta e mezza, e alcune mezzo vento, senza che in ciò vi potesse essere errore, perdre sempre erano stati molto vigilanti per notar ciò.

Chapter LXII

He (Christoper Columbus) also says that this same night, being Thursday 16th August (1498), the compasses, which till now had not northwested, did so at this time, at least a quarter and a half, and some of them two quarters; wherein there could be no mistake, because several persons had always watched to observe it.

The two remaining passages from the Columbian documents which must be quoted are from the letter written by Columbus from Haiti on October 18, 1498, and addressed to the Spanish sovereigns [94]. It gives his description of the third voyage, and includes the following.

J

Quando yo navegué d'España á las Indias, falló luego, en passando çient leguas á poniente de los Açores, grandissimo mudamiento en el cielo y en las estrellas y en la temperatura del ayre y en las aguas de la mar; y en ésto e tenido mucha diligençia en la experiençia, fallo que de septentrión en austro, passando las dichas çient leguas de las dichas islas, que, luego, en las agujas de marear que fasta entonçes nordesteavan, noruestean una quarta de viente todo entero.

When I navigated from Spain to the Indies, I found that, immediately after passing a hundred leagues to the west of the Azores, there was a very great change in the sky and in the stars and in the temperature of the air and in the waters of the sea. I have used much care in verifying this. I found that from north to south, passing there the said 100 leagues from the said islands, immediately the needle of the compass, which up to then had turned to the northeast, turned a full quarter of the wind to the northwest.

K

Yo siempre leý qu'el mundo, tierra y agua, era espérico, y las auctoridades y esperiençias que Ptolemeo y todos los otros qu'escrivieron d'este sitio davan y amostraban para ello así por ecclipses de la luna y otras demonstraçiones que hazen de oriente fasta occidente, come de la elevaçión del polo de septentrión en austro, agora ví tanta disformidad, como yo dixe; y por ésto me puse á tener esto del mundo, y fallé que no era redondo en la forma qu'escriven, salvo que es de la forma de una pera que sea toda muy redonda, salvo allí donde tiene el peçón, que allí tiene más alto, ó como quien tiene una pelota muy redonda, y en un lugar d'ella fuesse como una teta de muger allí puesta, y qu'esta parta d'este peçón sea la más alta y más propinca al çielo, y sea debaxo la línea equinoçial, ye en esta mar Occíana, en fin del oriente (llamo yo fin de oriente adonde acaba toda la tierra y islas). y para ésto allego todas las razones sobre escriptas de la raya que passa al occidente de las islas de los Açores çient leguas de septentrión en austro, que, en passando de allí al poniente, ya van los navíos alçándose hazia el çielo suavamente, y entonçes se goza de más suave temperançia, y se muda el aguja del marear, por causa de la suavidad, d'esa quarta de viento, y quanto más va adelante y alçándóse, más norvestea, y esta altura causa el desvariar del çírculo que escrive la estrella del norte con las guardas. y quanto más passare junto con la línea equinoçial, más se subirán en alto y más differencia avrá en las dichas estrellas y en los çírculos d'ellas.

I have always read that the world, land and water, was spherical, and authoritative accounts and the experiments which Ptolemy and all the others have recorded concerning this matter, so describe it and hold it to be, by the eclipses of the moon and by other demonstrations made from east to west, as well as from the elevation of the Pole Star from north to south. Now, as I have already said, I have seen so great irregularity that, as a result, I have been led to hold this concerning the world, and I find that it is not round as they describe it, but that it is the shape of a pear which is everywhere very round except where the stalk is, for there it is very prominent [95], or that it is like a very round ball, and on one part of it is placed something like a woman's nipple, and that this part, where this protuberance is found, is the highest and nearest to the sky, and it is beneath the equinoctial line and in this Ocean sea at the end of the East. (I call that "the end of the East," where end all the land and islands.) And in support of this, I urge all the arguments given above, concerning the line which passes from north to south a hundred leagues west of the Azores. For in passing hence to the westward, the ships went rising gently towards the sky, and then the mildest weather was enjoyed, and the needle shifted a quarter of the wind on account of this mildness, and the further we went, the more the needle shifted towards the north west, and this elevation causes the variation of the circle which the North Star describes with the guards, and the nearer I approached to the equinoctial line the more they rose and the greater difference there was in the said stars and in their orbits [96].

16—Before dealing with these passages, it is necessary to explain some of the terms employed, and to assign, as nearly as possible, the position at sea to which each passage refers.

In Columbus' day, the compass-card was divided into eight "winds," separated from one another by the directions north, northeast, east, southeast, south, southwest, west, and northwest. Each of these "winds" of 45° each, was subdivided into four "quarters," each of 11¼°. Hence a "quarter" corresponded to what is now known as a "point". Whether further subdivisions were marked on the card is uncertain. Probably they were estimated by eye.

In the account, by Fernando Colombus, of the second voyage (*G* above), there are references to "Flemish" and "Genoese" compasses. In the Genoese compass, the north-pointing end of the magnetic needle was fixed under the compass-card so as to correspond with the north point of the card. The direction of this latter point would then be affected by declination, and allowance would have to be made for this in laying a ship's course. In the Flemish compass, however, a different arrangement was adopted. The sailors of northwestern Europe had found that the needle did not point exactly to the geographical north; that it made an angle of about a point, or 11¼°, eastwards, with that direction. In order to correct for this deviation, or declination, they fixed the north-pointing end of the needle 11¼° to the east of the north point of the compass-card. The latter then gave them the true north [97]. Obviously, a compass so arranged would only give correct indications if used in a region where the declination was 11¼° east, and so long as it remained at this amount and of this sign. It should also be noted that the amount by which the Flemish compasses used by Columbus were corrected is uncertain, but it does not appear to have exceeded 11¼°. Gilbert writing in 1600, mentions corrections varying from one-half to two-thirds of that amount. There is no information as to the type of compass used on the first voyage of Columbus in 1492. Most probably it was a Genoese compass. It will be found later on that the fact of both kinds of compass being used on the second voyage is of considerable importance.

The passages quoted also make reference to the stars then known as the "Guards." These are now known as the "Pointers," and are the stars α and β of the constellation Ursa Major. A line through these stars passes close to the Pole Star (α Ursa Minor). Columbus makes several references to this latter star, and had some very peculiar ideas as to its diurnal motion on the celestial sphere [98]. In 1492, its distance from the celestial pole was 3° 26′.7 and its right ascension 2° 50′.0. Hence, at "nightfall," which, for the date and latitude concerned, may be taken as being soon after 6 P. M. local mean time, the Pole Star would be seen in the northern sky at an altitude of 28° 09′, and nearly due east of the celestial pole. At dawn, it would be at an altitude 28° 26′, and nearly due west of the celestial pole [99]. The two positions, at nightfall and dawn, would be separated by an arc of nearly 8°.

The question arises—and will be fully dealt with later—as to the fiducial point from which Columbus reckoned the easting or westing of his compass-needles. Nothing of a positive character is stated in any of the original documents, the *Journal* or the *Historie*. But, as will be shown below, there is a possibility that, in his earlier observations, for

example, on September 13 and 17, 1492, he used the azimuth of the star Polaris as being that of true north and reckoned the variation therefrom. He would thereby ignore the fact that this star was nearly 3½° from the celestial pole.

It only remains to add that the quotations given above relate to voyages, dates, and positions at sea, as follows:

Quotation	Voyage	Date	Latitude north ° ′	Longtiude west ° ′
A	First, outward	September 9, 1492	28 20	20 21
B	First, outward	September 13, 1492	28 21	29 16
E (1st part)	First, outward	September 13, 1492	28 21	29 16
C	First, outward	September 17, 1492	27 38	36 30
E (2nd part)	First, outward	September 17, 1492	27 38	36 30
D	First, outward	September 30, 1492	25 58	50 55
F	First, outward	September 30, 1492	25 58	50 55
G	Second, homeward	May 20, 1496	About 20 leagues west of Azores	
H	Third, homeward	July, 1498	6	40
I	Third, homeward	August 16, 1498	11	63
J	Relate to observations made when 100 leagues west of the Azores but to no particular date.			
K	Relate to observations made when 100 leagues west of the Azores but to no particular date.			

17—The quotations from the original sources have now to be examined in order to ascertain their meaning and to deduce therefrom what Columbus actually observed. This is a task of no slight difficulty, for the most cursory perusal of these quotations will show their exceeding obscurity. They have been examined in some detail by Bertelli [**100**], Wolkenhauer [**101**], Errera [**102**], and Magnaghi [**103**], but the conclusions reached by these eminent critics are by no means concordant.

The first passage, *A*, has apparently little connection with the question of magnetic declination. The reason for its inclusion is that it was brought forward by Bertelli [**104**] as indicating the result of steering courses uncorrected for declination.

As given by Navarette, and as used by Bertelli, it represents the steersman as turning from the prescribed westerly course into another lying somewhere between north and northeast; that is, through an angle between 90° and 135°. But the meaning of the phrase "aun a la media partida" is very far from clear, if we accept Navarette's transcription and Bertelli's argument founded thereon.

Bertelli's explanation of the matter forms part of his more general hypothesis that Columbus knew nothing of the fact of magnetic declination until September 13, four days afterwards. His argument may be stated thus. On September 9, the ships were in a region of easterly declination, although neither Columbus nor any of his pilots knew it. The course actually steered would thus lie to the north of the westerly course prescribed by Columbus. But while the steersman went entirely by compass, Columbus navigated by astronomical observations, and was thus able to check errors in steering.

Bertelli's view cannot be accepted for several reasons. First, while it is true that the ships were then in a region of easterly declination, it is highly probable that the amount of the declination was too small to be detected by the rudely divided compass-cards in use at that time. About 40 years afterwards, De Castro [**105**] found the declination at Lisbon to be 7½° east, and in 1538, at Las Palmas, it was 5½° east.

We may therefore take it that, at the position reached on September 9, 1492, the easterly declination did not exceed 3°. Schott [**106**] has made another estimate and finds it 2°. Thus the amount of the declination was altogether too small to have an immediately appreciable effect on the steering. Second, even supposing the error in steering, due to supposed ignorance of the fact of declination, were large, it could in no circumstances account for such an enormous departure, exceeding 90°, from the course laid down. Third, it was impossible for Columbus to check the steering of the ship from one moment to another by astronomical observations. Indeed, there is no mention in the records of his having attempted anything of the kind.

PLATE I

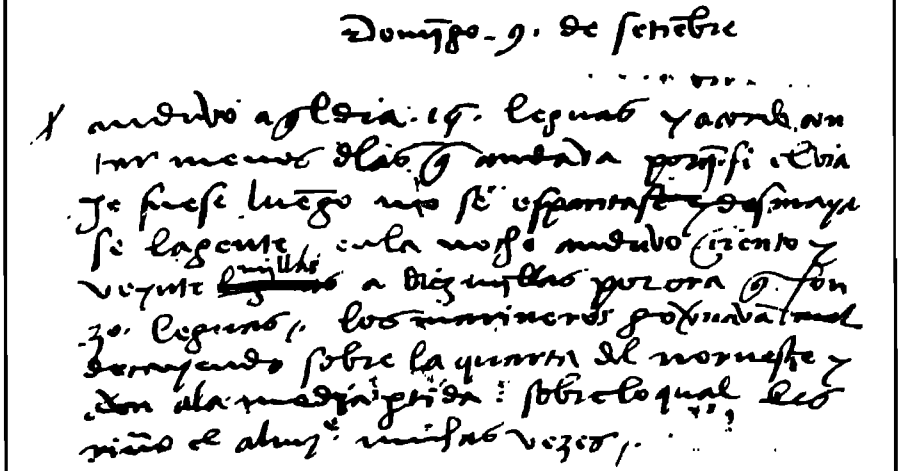
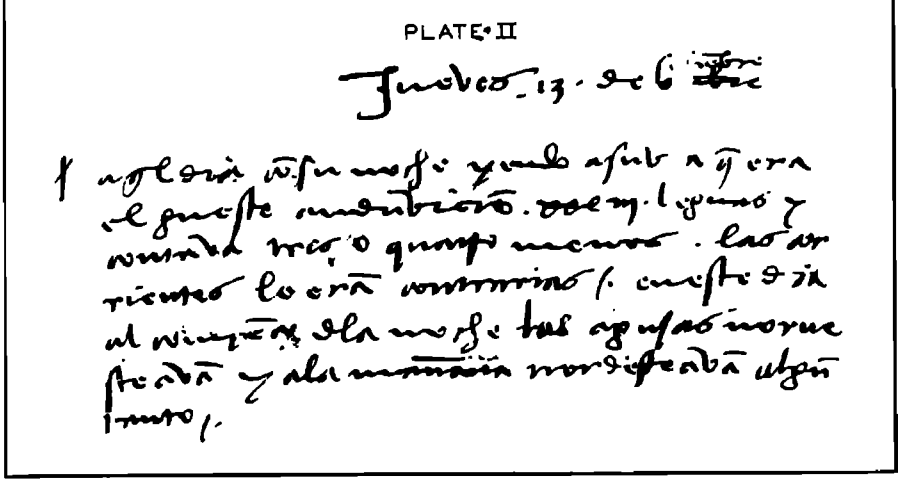


PLATE II



Magnaghi [**107**] has suggested that the steersman, in turning northwestwards from the prescribed westerly course, was attempting to allow for leeway due to currents. But it is improbable that a steersman would make any such attempt without specific instructions.

Since a departure from west to a course nearly northeast would have been practically a return in the homeward direction, Lord Dunraven [**108**] suggested a mutiny of the crew and an attempt to return to Spain. But this is not supported by any available evidence.

The whole matter assumes a totally different aspect if the original documents are examined. The transcription of the passage *A*, in the *Colección* [**109**] of Navarette gives northeast as the direction to which the ship fell off. But from the photographed copy, Plate I, of the Las Casas manuscripts it will be seen that the word used is not "nordeste," as transcribed by Navarette, but "norueste." This is supported by what Las Casas stated in his *Historia* [**110**], where he gives northwest as the new direction, not northeast. This explains the passage quite sufficiently, including the phrase "aun a la media partida." What it meant to convey was that the steersman allowed the ship to fall away from west *towards* northwest, and even to the extent of half that departure, that is, by two points, or 22½°. This would be quite possible as an example of careless steering in such ships and would not require astronomical observations for its detection, because it would be immediately noticeable.

The passage *A* has thus no bearing on the question of declination, or on that of Columbus' knowledge, or ignorance, of the existence of that quantity.

18—We therefore pass on to consider the quotation *B* from Las Casas; and along with this, as dealing with the same event, we take the first sentence of the passage *E* from the *Historie* of Fernando Columbus.

The first matter which calls for attention is a serious discrepancy between these two accounts. The Las Casas manuscript (see Plate II) states that the compass-needles declined northwestwards on the evening of September 13, 1492, and on the following morning they declined a trifle towards northeast. In Navarette's transcription [**111**] the latter is given as northwest, an error or substitution, which is obvious on reference to the original. On the other hand, the *Historie* of Fernando Columbus, as quoted in passage *E*, makes the morning deviation to the northwest. Not only so, this quotation gives the evening deviation as half a quarter and that of the morning half a quarter more —that is, the difference between the evening and morning directions of the compass-needle was nearly 5½°. Turning to Las Casas' *Historia de las Indias* [**112**], we find the morning direction given as "*nordesteaban, que es decir, que se acostaba la flor de lis a la mano derecha del Norte*," thus confirming what we find in the Las Casas manuscript.

There is thus distinct and irreconcilable conflict between the two statements, but how this arose is far from easy to explain. Several possible solutions present themselves. Las Casas and Fernando Columbus may have been quoting from different copies of the *Journal;* there may have been errors of transcription by one or other or both authors; there may have been errors in translation by Ulloa, the editor or translator of the *Historie*. In the absence of the originals, both of the *Journal* and of the *Historie*, it is impossible to settle the point def-

initely. But, after the important correction has been made in Navarette's transcription of the Las Casas manuscript, it will be seen that Las Casas' is the more acceptable version of what took place.

First, assuming that Columbus used the direction of the Pole Star as being that of true north, it has to be remembered that his fiducial point moved nearly 8° across the sky in a westward direction during the night of September 13-14. Hence, if we make the perfectly reasonable supposition that there was little or no change of declination during the night's sail (about 50 miles), a deviation slightly to westwards of the Pole Star at nightfall would be equivalent to a nearly equal deviation to eastward of the Pole Star in the morning. Regarded in this way, the statement made in Las Casas' edition of the *Journal* under September 13, 1492 is consistent with itself and with what we know of the conditions.

Second, on the same assumption, Fernando Columbus' statement would make the total motion of the compass needle during the night equal to "half a quarter," that is, nearly 5½°, *plus* the westwards motion of the Pole Star—in all, about 13½°. This, to put it mildly, is so exceedingly improbable within a distance of 50 or 60 miles, that it must be entirely rejected.

Third, discarding the foregoing assumption and supposing that Columbus, recognising the fact of the Pole Star having an appreciable N. P. D., measured compass-deviation from the true north, it is impossible to account for the compass-needle varying westward at night *and* eastward next morning. The ships were not, at the time, in a region in which the distribution of declination was highly irregular, and the only possible explanation—on the above hypothesis—would be either careless determination of the direction or the intervention of some extraneous disturbing agency.

Fourth, if we again assume that Columbus measured deviation from the true north, Fernando Columbus' statement is conceivably true as regards the *manner* of the variation recorded but, as to its amount, namely, 5½°, it cannot, for reasons given above, be accepted.

Thus the balance of argument is in favor of the statement made in Las Casas' edition of the *Journal* and is in support of the hypothesis that Columbus measured the variation from the Pole Star. This latter point will be dealt with again.

Two additional comments have to be made on these entries relating to September 13, 1492. The first is that Fernando Columbus wrote his *Historie* nearly forty years after the events which he undertook to describe. By that time, the fact of the magnetic declination was well known, and possibly that of its variation in space. Looking back on what he personally knew of the history of the matter and believing that a continuous change in declination from east to west took place in the westward crossing of the Atlantic, he might conclude that on this westward track the westerly variation on the morning of September 14 should be still greater than that of the previous evening. Accordingly, he made it so, without examining the question as to the point from which his father had reckoned the declination. In the second sentence of the passage *E*, he states that no one had observed such variations before and that his father was surprised at them. This would appear to be a reflection of his own, rather than an extract from any record. For it is to be noted that there is nothing said by Las Casas,

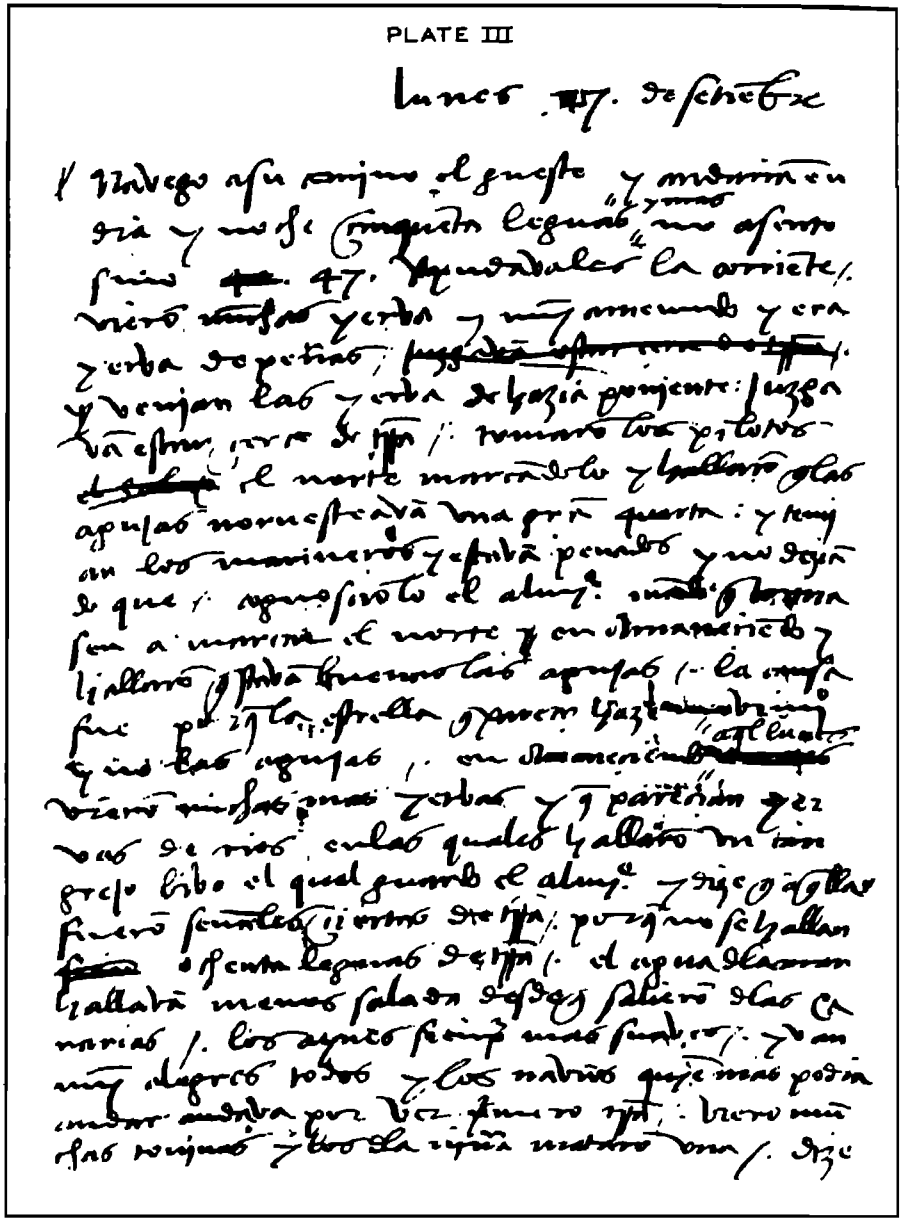
either in his edition of the *Journal* or in his *Historia*, which corresponds to this statement.

The second comment is that we may profitably enquire into the possible reasons which may have led Columbus to make observations on compass-variations during his voyage. Following Navarette [**113**] and Humboldt [**114**], Bertelli [**115**] has claimed for Columbus the discovery, not only of the space-variation of the declination, but of the declination itself. Let us suppose for the moment that this view is correct, and therefore that Columbus sailed from Palos in the fixed belief that, no matter where it might be observed, the compass-needle always pointed exactly north. Why, then, these observations, night and morning, of the direction of the compass-needle? In the belief referred to, they were unnecessary for the navigation of the ship. It is true that there is no record of any observations of the kind during the earlier portion of the voyage. If they were made, it could only have been because the fact of the declination was already known and because it was necessary for correct navigation to ascertain its amount and make allowance for it. If they were not made, Bertelli's claim on behalf of Columbus is founded upon a casual determination of declination whose amount was within the limits of observational error with the instruments then employed. Again, if such earlier determinations were made and if the declination, as asserted by Bertelli, were then unknown, how was Columbus able to detect a small westerly declination on September 13, and yet fail to note the larger easterly declination off the Spanish coast? The inevitable conclusion is that Columbus (or, rather, his pilots) must have been aware of an easterly declination and this would account for there being no record of it in his *Journal*. It would be an accepted fact. Later on, it will be found that this conclusion is supported, if not positively expressed, by his own statements.

Summarising the foregoing examination of the records relating to September 13-14, 1492, it may be stated: (1) That the two accounts by Las Casas and Fernando Columbus disagree as to details, but that the former appears to be the more reliable; (2) that in all probability, Columbus reckoned compass-direction from the azimuth of the star Polaris as giving true north; (3) that on this assumption and taking into account the diurnal change in position of the fiducial point, the observations are consistent with a westerly declination of 1°, or possibly 2°, but the method of observation was much too rough to determine this with any approach to accuracy; (4) that general considerations are all in favor of the view that Columbus, or his pilots, were already acquainted with an easterly declination. If this is correct, Columbus did not discover the magnetic declination. He made observations which indicate that on September 13-14 he was close to the agonic line, but formed no conception of any general kind as to the existence of any such line.

19—The observations made on September 17, 1492, have next to be considered. They are contained in the passage *C* from the Las Casas manuscript (see Plate III), and in the latter part of *E* from Fernando Columbus' *Historie*. They agree in stating that at nightfall the deviation was westerly by a full point, that is, nearly 12°, but that on the following morning the needle pointed to the Pole Star.

PLATE III



The questions we have to answer, if possible, are two. What are we to understand by these observations? How did Columbus interpret them? With regard to the first, we have here again a case which can be best explained on the hypothesis that, up to this date, Columbus measured compass-directions from the Pole Star as representing the true

north, and not from the celestial pole. If he were measuring direction from the latter point, the difference between the evening and morning directions of the compass-needle was fully 11¼°. This, of course, is an impossible result and we therefore take the first alternative. In this case the evening observations made the compass-needle point 7¼° west of true north (that is, allowing nearly 4° for the N. P. D. of Polaris). The morning observation made it point 4° west of true north These two observations differed by 3¼°, an amount too small for measurement by the rough methods [**116**] then in vogue and which was, as a matter of fact, neglected. The true meaning of these observations therefore is that the compass-needle continued to point nearly true north. So far, then, nothing had been actually observed which proved the existence of a magnetic declination.

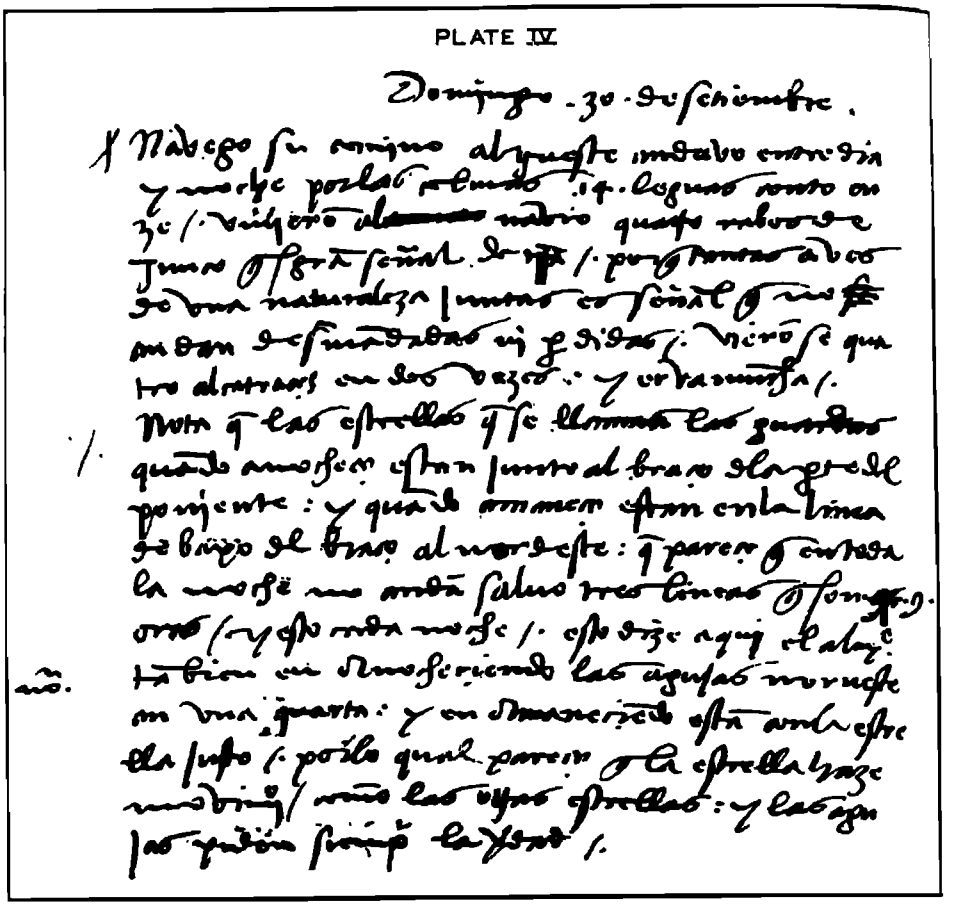
With regard to the second question, as to what construction was put upon these observations by Columbus, we have very little information. The exact extent of his knowledge of the behavior of a compass-needle is unknown to us, and such comment as is made in his *Journal* on these observations is not in agreement with his later statements. If we suppose that Columbus regarded the position of Polaris as being a fixed point on the celestial sphere, the observations ought certainly to have appeared to him as being peculiar. But there are indications in the passages quoted, especially if taken along with the last sentence of passage *D*, that it was only about this time that Columbus began to realise that the star Polaris was not fixed in position, that it had an appreciable N. P. D., and that it revolved round the celestial pole as do the other stars. It is thus possible, though not probable, that the correct interpretation of the phenomenon may have occurred to Columbus. The *Journal*, or Las Casas' edition of it, states that the sailors became afraid. Fernando Columbus says his father was surprised. The cause of this fear or surprise could not have been the discovery of any new and inexplicable fact such as magnetic declination, for such a result the observations did not indicate. The only reasonable hypothesis is, that whereas they had known, and been accustomed to, an easterly declination off the coasts of western Europe, they now found that this would appear to have nearly vanished, and possibly become a westerly declination.

20—The quotations *D* (Plate IV) and *F*, relating to observations on September 30, 1492, do not add anything to what had been recorded already. But the passage *F* again suggests that observations of the direction of the compass-needle were measured from the Pole Star, and not from the celestial pole.

21—We have now to deal with the passage *G* from the *Historie* of Fernando Columbus, and it contains important matter for the present purpose. It deals with observations made on May 20, 1496, while on the homeward run of the second voyage. The position of the ship on that date was somewhere about 120 leagues west of the Azores [**117**].

The passage begins with a reference to "Flemish" and "Genoese" compasses, the arrangements in which have already been explained. Nothing is on record as to the kind of compass used by Columbus on the first voyage, but the passage now under consideration shows that on the second voyage both kinds were used. The first voyage lasted

PLATE IV



from August 3, 1492, until March 15, 1493, and Columbus embarked on the second voyage on September 20, 1493. According to Bertelli, the fact of magnetic declination was unknown to any person before September 13, 1492, on which date, he avers, Columbus discovered it. If this be the case, his discovery could not have been published to the world before his return on March 15, 1493. Now, in the first place, we have no evidence of any such publication. Yet in six months we have the Flemish compasses in use, with their device making allowance for the declination. It is altogether inconceivable that, in this short period of six months, a knowledge of Columbus' supposed discovery could have travelled as far as Holland and could have allowed navigators to confirm it, to devise means of making allowance for it, to introduce the new form of compass, and for this new form to reach Spain in time to be included in the equipment for Columbus' second voyage. The only possible conclusion is that the magnetic declination was known to the navigators of northwestern Europe independently of Columbus. And we may go so far as to regard it as highly probable that Columbus knew of it him-

self when he left Palos on his first voyage. The single fact of the use of both kinds of compass on the second voyage is fatal to Bertelli's claim on behalf of Columbus [**118**].

The same passage, *G*, also refers to the indications given by the two forms of compass. Beyond the fact that the directions of their north points on the compass-card differed by a "quarter," that is, 11¼°—which shows that the Flemish instrument had been fitted for an easterly declination of that amount—the information given is scanty and its meaning is obscure. Apparently, there was a belief on the part of Columbus—confirmed by later documents still to be cited—that in the region 100 leagues west of the Azores, the compass behaved in a peculiar manner. But in what the "difference" consisted, at which Columbus was surprised, is far from clear. In any case, his explanation that it was due to the needles being magnetised by different kinds of lodestone is unacceptable [**119**]. It is in some degree a reflection of the opinion held at the time by some navigators [**120**].

22—In the passage *H*, which relates to July 1496, when Columbus was sailing westwards on his third voyage, and was in a position about latitude 6° north and longitude 40° west, Fernando Columbus gives his own explanation of the "northwesting" of the compass-needles which had been observed on previous occasions. He believes that there is, in each piece of lodestone, some particular part or point which, when rubbed on a steel needle, causes the needle to point to the north. Here he is only repeating what was common knowledge of the time. But he goes on to state that the lodestone has other parts or points which, when so applied, cause the needle to point to the east, the west, or to the south. In support of this statement, he quotes the practice of compass-needle makers, who cover the lodestone with a cloth, "all but the north point of it," when magnetising a needle. It has already been shown, in paragraph 5, that a similar idea was current in Roger Bacon's time.

The curious feature here, however, is that there is no mention of declination, although its existence was common knowledge among navigators when Fernando Columbus was writing his book. It also suggests the conclusion that his father had not formed any clear conception as to magnetic declination and this we find to be confirmed by later documents.

The passage *I*, from Fernando Columbus' *Historie* relates to observations made near the Gulf of Paria on the third voyage westwards. Its substance is so extraordinary that it has generally been rejected entirely by commentators. That the compasses should show no variation from true north during the voyage, and then suddenly change by nearly 17°, or even by 22°, is quite unbelievable. We can only conclude that such effects must have been produced by some extraneous disturbance.

23—Finally, we have the two extracts, *J*, *K*, from the Haiti letter of Columbus, dated October 18, 1498.

In the first, we have Columbus' own statement that until he passed to westwards of a point 100 leagues west of the Azores, the compass-needle pointed towards northeast, and that when he had passed this point, it immediately turned to a direction fully 11° west of north. This is the first time, among the somewhat obscure records available, that we have a tolerably clear statement of what Columbus believed he had found. Yet it presents two difficulties in the way of its unqualified

acceptance. The first is, that a sudden change in direction, of this amount, is altogether improbable. The second comes from the historical point of view. For his statement gives no indication as to the exact stage, in his previous voyages, at which he had framed this conception. It might be quoted as proof that he knew of the easterly magnetic declination before he left, or very soon after leaving, Spain on his first voyage, but the letter does not say so explicitly. All that can be said is that it is not inconsistent with, and even strengthens that conclusion, which, on grounds already stated, has been shown to be highly probable.

In the second passage, *K*, Columbus gives expression to his own peculiar views regarding the changes met with in crossing the meridian 100 leagues west of the Azores. These have no bearing on the question of priority in discovery of the magnetic declination and the passage is only quoted on account of the second explanation which Columbus now gives for the westerly variation of the compass-needle, namely, that it is due to the mildness of the climate, which, in its turn, he attributes to a protuberance of the terrestrial surface in that region.

24—The observations made by Columbus, or credited to him by his editors, biographers, translators, or commentators, have thus been dealt with in detail. But before summarising such conclusions as can be drawn from them, it is well to ascertain whether these observations, or their results, were referred to by any writers between 1592 and 1789, the date of discovery by Navarette of the Las Casas manuscripts.

Bertelli [**121**] has brought forward several witnesses of this kind, the first being a letter [**122**], dated from Cadiz, January 2, 1498, and written by Simon del Verde to Mateo Cini. It contains the following passage: ". . . *lo admirante ha havuto grande animo et ingiegno havere discoperto l'altro mondo opposito al nostro con tante fatiche et sudori et visto la mutatione che fa la tramontana per essere ito di la linea del equinoctiale.* . . ." But this does not credit Columbus with the discovery either of the declination or of its change in sailing westwards. It only refers to the declination and its change as possible difficulties in laying a correct course across the Atlantic. Indeed, one might almost gather from the letter that both of these quantities were already known.

Bertelli [**123**] also quotes a letter, dated January 1519, from Piero di Giovanni di Dino [**124**], in which observations of declination on the Guinea Coast and at the Cape of Good Hope are given. They are among the earliest of their kind. There is full recognition of the declination and its change with position but there is no mention of any observations by Columbus in connection with either. Since the observations by Columbus had not, by that time, been published to the world generally, it must be concluded that both facts must have been reached by others independently of, and even possibly before, Columbus.

The last two references given by Bertelli [**125**] are those to a letter, dated March 6, 1582, by Filipo Sassetti [**126**], and a passage written by Giovanni Maria Sagri in the preface to a book published in 1574 by his brother Nicola [**127**]. Both refer to the declination—a matter of common knowledge by either date—but only in a general manner. Neither makes any mention of Columbus.

Such absence of any reference to Columbus is all the more sharply marked by the fact that none of the writers, in whose works we might expect such reference, mentions his name in this connection. Oviedo

published his history of the Indies [**128**] in 1535, but although he deals at some length with the northwesting and northeasting of the needle, he does not ascribe the discovery to Columbus. His account, it is true, is very far from clear—so much so, that his French and Italian translators have made sad havoc of the passage [**129**]. And although Las Casas said of the book that it contained as many lies as it had pages, it is to be remembered that Oviedo was the Spanish Historiographer-Royal, and in this capacity would have access to all official records bearing on the voyages of Columbus. After Oviedo, we have Pedro de Medina (1555) and Pedro Nunes (1537). The former even denied the existence of declination, ascribing the supposed results to defective instruments or faulty observations [**130**]. The latter gives directions for finding the declination [**131**]. But neither mentions Columbus.

Thus, apart from the question whether Columbus did or did not discover the declination, three centuries elapsed during which, if we except Ulloa's translation of Fernando Columbus' *Historie* and the writers [**132**] who copied therefrom, the name of Christopher Columbus is never associated with the discovery of magnetic declination, and during which, as will be shown later, other claims were brought forward. Surprise has been expressed [**133**] that such should have been the case. But, apart from the merits of the Columbus claim, the explanation is not far to seek. To begin with, nothing seems to have been publicly known with regard to the observations made by Columbus until the issue of Ulloa's translation in 1571. By that time, other interests had been created; the question of the position of the agonic line had led to attempts at finding the longitude by magnetic methods; attention was drawn off to schemes of exploration and conquest; and, it might be held, Spain, as a land of scholarship and research, had begun to sink. Ulloa's translation had, probably, but a limited circulation, for it was not translated into French until 1681, nor into English until 1744, while it was only in 1748 that it was retranslated into its original Spanish. Gilbert's *De Magnete* appeared in 1600 and focussed enquiry into the wider aspects of the subject. The magnetic declination was now an acknowledged fact; its origin must have been regarded as one of many things imbedded in the distant past. These conditions or others of similar tendency apparently led to neglect of the early history of the science and the earlier records upon which it was based. It was not until Navarette found the Las Casas manuscripts that the name of Columbus was again brought into prominence.

The first distinct reference, apart from Ulloa's translation, to the observations is that by Formaleoni in 1783 [**134**], a few years before Navarette's discovery. He makes two statements of importance. The first is that it is wrong to ascribe the discovery of declination to George Hartmann in 1538, "for it had been known before him, and known for a long time." The second—which may possibly be a quotation from Ulloa's translation—contains the following: "In the history of voyages, I found express mention of the observations made by Columbus of the deviation of the needle; that in these new seas he found it to vary very much from that observed in the Mediterranean, which embarrassed him and was regarded by the Spaniards as a new phenomenon." The important point here is that Formaleoni does not ascribe the discovery of the declination to Columbus and refers to its value in the Mediterranean

as known before Columbus. Further, it is doubtful whether he ascribes the discovery of the space-variation of the declination to Columbus, for it is a moot point whether his phrase "by the Spaniards" is to be construed as meaning that other nations had previous knowledge of the phenomenon.

25—We have now to summarise the conclusions reached with regard to the claim advanced on behalf of Columbus, that he was the first to observe the magnetic declination, or at least that he discovered the space-variation of the declination. These claims have been considered on their own merits, without prejudice from the previous history of the matter. With reference to the first of these two questions, the following conclusions appear to be reasonably established by the available evidence:

(*a*) That the faulty steering of the ships on September 9, 1492, has no bearing on the question. In his transcription of the Las Casas manuscripts, Navarette made one serious mistake, after correction of which the whole matter appears in a simple light and unconnected with questions of declination.

(*b*) That it is almost entirely certain that an easterly declination had been observed in northwestern Europe before Columbus sailed on his first voyage. The proof is meanwhile inferred from the Columbian records themselves. It therefore follows that the magnetic declination was not discovered by Columbus.

With regard to the second question, the conclusion is:

(c) That the observations made by Columbus in his first westward voyage in 1492 are, in a rough manner, consistent with his having crossed the agonic line, but that he attributed the results to entirely erroneous causes, and thereby failed to recognise them as evidence of a general space-variation of magnetic declination. There are also slight indications that before this first voyage the fact that declination was not of equal amount at all places was known to several navigators.

26—In the literature of the present subject, reference is frequently made to the possibility of the magnetic declination having been discovered by John Cabot or by his son Sebastian Cabot. Neither left behind him any writing or other document in which this claim is definitely put forward and its general foundation is practically confined to statements made by third parties. These have now to be considered.

In dealing with this matter, certain dates have to be kept in view. The years of birth and death of both the Cabots are not exactly known, but it is probable [**135**] that the father was born not later than 1451, and the son before March 1474. John Cabot's first voyage of discovery, really inspired by the earlier enterprise of Columbus, was carried out in 1497, and his second voyage was in the following year. Whether Sebastian, who afterwards became the more famous man, actually accompanied his father on either of these voyages seems to be doubtful. For several years, little is heard of Sebastian, until in 1514 he entered the service of the Spanish Government, was appointed a pilot, and in 1518 became Pilot-Major to His Catholic Majesty. In this capacity, and as

a member of the Junta, he must have been closely acquainted with many of the results of the enterprise of Columbus, particularly in their bearing on oceanic navigation. There is, however, no direct evidence on this point. In 1544, he published his famous planisphere, on which he indicated the agonic line passing north and south in 25° west longitude [**136**]. In the following year, we find him engaged with others at Seville in examining and reporting on Pedro de Medina's treatise on navigation [**137**].

The earliest document connected with Sebastian Cabot and his possible discovery is to be found in the correspondence between the Senate of Venice and their ambassador, Contarini, at Valladolid. During an interview with the latter in 1522, Sebastian Cabot stated that he knew of a method by which the distance between two places, east and west, could be found by the compass, and that it had never been observed by any one else [**138**]. This communication to the ambassador, who forwarded it to the Senate, could have had no influence at the time, for it was hidden in the archives of the celebrated Council of Ten—a body eminently well qualified to keep its own secrets—and was unknown until 1864. Cabot's was probably an early form of the attempt to determine longitude from the declination, supposed known along a circle of latitude. The sixteenth century saw several attempts of the kind [**139**]. But Cabot's statement to the ambassador has no bearing on the question of priority, for the existence of declination had been proved long before 1522.

The first writer who advanced any express claim on behalf of Sebastian Cabot was Livio Sanuto. His book [**140**] was published in 1588, but was written before 1553, while Sebastian was still alive. Sanuto stated that a friend had informed him some years before—when, he does not say—that the magnetic needle does not always point to the geographical meridian of the observer; that Sebastian Cabot had discovered this fact and, in the presence of the friend referred to, had communicated it to the King of England, and that he also showed that the divergence between the two meridians was not the same at all places. This statement by Sanuto was accepted and given wide currency by several influential writers, among whom may be mentioned Gilbert [**141**], Kircher [**142**], and Fournier [**143**]. A similar statement was made by Fontenelle [**144**] in 1714, but, as pointed out by Harrisse [**145**] who has investigated the matter with his usual thoroughness, this was probably derived from an inscription on one of Cabot's maps. Another possible source for such statements was an inscription on Ruysch's *Mappamundi* of 1509. This inscription was placed on the map in a position about latitude 85° north and longitude 25° west and said that "*Hic compassus navium non tenet, nec naves qui ferrum tenent revertere valent*" [**146**]. This, of course, was only a reflection of the opinion, then widely held, that the directional property of the magnet was due to the presence, in the far north, of powerfully magnetic islands. Writers who based their accounts on this source may have been misled by the commentary on Ruysch's map by Marcus Beneventanus, which referred to discoveries made by the English in these regions, and may have connected them with one or other of Cabot's voyages under the English flag. Still later writers, for example, Lelewel [**147**], improve upon the matter by dating Cabot's discovery in 1497 but for this there is absolutely no evidence.

Sebastian Cabot cannot, however, be upheld as the discoverer, either of the declination or of its space-variation, and this for several reasons. First, and principally, the declination was known before his father's first voyage. Second, had Cabot deserved such honor, some at least of the writers on nautical matters of the time, who knew both the man and his achievements, would have mentioned his name in this connection. But these, including Oviedo [**148**], Pedro de Medina [**149**], Cortes [**150**], and Nunes [**151**], are silent. Lastly, at an important meeting of pilots held at Seville in 1536, at which Sebastian Cabot was most probably present in his capacity of Pilot-Major, a map showing declination, prepared by Alonzo de Santa Cruz, was exhibited [**152**]. Yet nothing was then advanced by or on behalf of Sebastian Cabot as an earlier discoverer of what was thus represented. Other names are connected with the study of such questions, but that of Cabot receives no mention.

27—During the sixteenth century, we have the earliest recorded values of the magnetic declination at many places over a wide area. These cannot be regarded as evidences of the origin of the conception of declination and, especially as they have an interest of their own in the history of the subject, they require separate and later treatment. We therefore conclude our investigation into all known or supposed origins with the examination of the claim to priority put forward on behalf of Sebastian Cabot. The way is therefore clear to draw such conclusions as the records would appear to warrant.

The discovery of the directional property of a suspended magnet was reached in an age which was but poorly equipped to apply accurate tests to such a property. Instruments for such purposes were very few and very crude. Add to this the fact that it was a time in which discussion tended towards the assignment of quality, rather than the determination of physical magnitude, and we need not be surprised that progress towards the second fundamental in terrestrial magnetism was uncertain and slow. But the recognition of magnetic declination was only a matter of time, depending as it did upon improvement in instrumental means and the consequent increase in accuracy of observation.

From the earliest records quoted, one derives the conclusion that, long before the regular observation of declination as an accepted fact, there must have been a vague impression that the magnet did not coincide in direction with the geographical meridian. Even in 1266, when Roger Bacon wrote, the possibility of its pointing in other directions had been mooted and it is difficult to conceive of Bacon writing as he did without some observational data at his disposal. It is legitimate—without adopting the facile process of raising conjecture to the level of historical fact—to suppose that cases would occur irregularly in which attention would be directed to the fact of such divergence between the two meridians. But these would not form the subject of record, although they might be common knowledge among those who had actual contact with the matter—that is, among sailors and makers of compasses, sundials, maps, and charts.

It is here, then, that the beginnings of the conception of magnetic declination are to be found—among those whose business in life was to take cognisance of such matters and deal with them as actual affairs. Unfortunately for the historian of science there is no record of the process and we are only permitted to know its outcome. This took the

practical form of making an allowance for the declination in the construction of sun-dials and nautical compasses. The date at which this adjustment was first effected cannot be fixed with certainty. As the records stand, the earliest indication is given by the Nuremberg sun-dials dating from about 1450. The first report of the Flemish compass with its adjusted needle is nearly half a century later, but the fact that it then appears in fairly general use would argue that it had been employed for many years. Lastly, the records do not justify the association of any particular name with either the first recognition of declination or with the practical measures adopted to allow for it in sun-dials or compasses. None of the claims put forward, whether on behalf of the Chinese, or of Peregrinus, Christopher Columbus, or Sebastian Cabot can be substantiated.

The general conclusion reached, therefore, may be stated as follows. Within a century after the first mention in literature of the nautical compass, we find allusions in the writings of Roger Bacon to the fact that the direction of the magnetic needle does not coincide with the geographical meridian. When, where, and by whom this was first observed is now unknown. At first, this deviation was ascribed to irregularities in the process of magnetisation of the compass-needle, or to faulty methods of observation. But gradually it came to be recognised as a general and world-wide phenomenon and it is probable, though there is no extant record to establish the fact, that this stage was reached in the earlier years of the fifteenth century. Very soon thereafter and certainly not later than 1450, the makers of sun-dials in Germany introduced improvements in their instruments in order to avoid the effects of deviation of the needle. Possibly about the same time, though more probably later, the compass-makers of Holland adjusted their compass needles with the same object. The Mediterranean navigators continued to use the compass without this adjustment. Finally, no original observation, discovery, or instrumental improvement, as made or effected down to the end of the historical period concerned, can be definitely or exclusively associated with the name of any particular person.

Bibliographical Notes and References

[References which have not been examined and verified are marked by an asterisk.]

[1] In the case of the compass referred to by the Persian Awfi (Chapter I, p. 119), it would seem to have been regarded as something new. But this may have been due to the writer's limited acquaintance with nautical matters.

[2] Wylie, *Chinese researches*, Shanghai, 1897, Part III, 155.

[3] *Terr. Mag.*, XXXVII (1932), 109-110. Wylie's statements have been repeated by Chu-Co-Ching, *Geog. Rev.*, V (1918), 137-138, but without giving any further information.

[4] Hirth, *Ancient history of China*, New York, 1911; Chronological summary, under 700 A. D.

[5] Hashimoto, *Memoirs of the Research Department of the Toyo Bunko*, Tokyo, 1926, No. I, p. 85.

[6] See [5], p. 85.

[7] See [4], p. 132.

[8] **Mung-khi-pu-py-than. Ch. III.*

[9] Klaproth, *Lettre à M. le Baron A. de Humboldt sur l'invention de la boussole*, Paris, 1834, p. 68, quotes from the *Pen-thsao-yan-i*, compiled 1117 A. D. The same source was used by Gaubil, *Observations mathématiques, astronomiques, géographiques,*

chronologiques, et physiques, tirées des anciens livres chinois, ed. by Souchet, Paris 1729, 3 vols; Hager, *Memoria sulla bussola orientale*, Pavia 1809 and 1810; Biot, *C. R. Acad. sci.*, XIX, 822-829. The passage also appeared in the twelfth century dictionary *Poei-wen-yun-fou* and in the medical and zoological work, entitled *Pen-thsao-kung-muh*, completed about 1580. Bertelli, *Mem. Acc. Nuovi Lincei*, IX, Part I, reports it as occurring in the great Sino-Japanese encyclopedia *Wa-kau-san-sai-dzu-ye*, Bk. XVI, fol. 19. Translations by different commentators have not always been in agreement. See Bertelli, *Bull. Soc. geogr. ital.*, III, and De Moidrey, *Terr. Mag.*, IX (1904), 29.

[10] Le Comte, *Nouveaux mémoires sur l'état présent de la Chine*, Paris 1696, I, lettre VIII; in the English edition, London, 1699, p. 229.

[11] Amiot, *Mémoires concernant le Chinois*, Paris, 1788, 13 vols., IX, 2.

[12] Gaubil, *Description de ville de Peking*, Paris, 1763, p. 8.

[13] See [5], p. 88.

[14] Fr. Rogeri Bacon, *Opera quaedam hactenus inedita*, ed. by Brewer, vol. I, Rolls Series, 15, p. 383-384. The passage is as follows: "Et hoc est miraculum naturae in parte notum; scilicet, quod ferrum sequitur partem magnetis quae tetigit ipsum, et alteram partem fugit ejusdem magnetis. Et convertit se post motum ad partem coeli conformem parti magnetis, quae ferrum tetigit. Nam pro certo quatuor partes mundi distinguuntur in magnete, scilicet oriens, occidens, septentrio, et meridies; et possunt per experientiam cognosci, secundum quod bene exformiter ad quam partem coeli quaelibet pars tendit. Et nunc si a parte septentrionali magnetis tangatur ferrum sequetur illam partem qualitercumque meatur; scilicet sursum aut retrorsum, dextrorsum, sinistrorsum; et secundum omneno differentiam positionis. Et in tantum rapitur, quod si ferrum ponatur in vase pleno aquae, et manus ponatur sub vase, tacta pars demergit se in aqua in directum magnetis. Et si deferatur undique magnes extra nos, ferrum super partem tactam erectam currit in directum cujuslibet loci, ad quem defertur magnes. Et si ima pars magnetis objiciatur parti ferri tactae fugaret eam sicut inimicam; sicut agnus lupum (MS, *vapum*). Et ablato magnete pars tacta dirigit se ad locum coeli similem parti magnetis.

Vulgus philosophantium nexit causam experientiae vulgatae in hac parte, et credit quod stella Nautica (MS. *Naucita*) facit ad hoc. Sed stella non facit ad hoc sed pars coeli. Et ita bene operantur tres aliae mundi partes; scilicet meridies, oriens, et occidens, sicut septentrio. Similiter non considerant quod quatuor imae partes mundi distinguantur in magnete. Sed tot attribuunt uni parti, quae cum stella convenit Nautica (MS, *Naucita*) in naturali proprietate. Et aliae sunt hujusmodi experientiae et meliores non de ferro solum et magnete, sed de auro, et omnibus metallis respectu diversarum specierum magnetis, sicut docetur in libro *De Proprietatibus*. Et non solum in his sed in omnibus rebus mundi necesse est quod partes quatuor signentur, secundum quatuor partes coeli, et maxime in his rebus, quae fixum habent locum, ut in rebus inanimatis et plantis; non habent continue partem determinatam ad oriens, et aliam ad occidens, et ceteras duas ad septentrionem et meridiem, et continuas et perpetuas recipiant influentias a partibus coeli; respectu quarum habent situm quod possumus experiri in rebus infinitis."

[15] *Terr. Mag.*, XXXVII (1932), 126-127, and notes [265], [266].

[16] See [15], notes [242], [243]. By an unfortunate misprint, the date of the Peregrinus letter was given as 1209, instead of 1269, in Chapter I, [15] p. 125.

[17] Thévenot, *Recueil des voyages*. Paris, 1681, p. 29; also *Journal des Scavans*, Amsterdam, 1688, T. 15, p. 561.

[18] For example, Van Swinden, *Mém. de mathem. et de phys. présentés à l'Acad. Roy. des Sci.*, VIII, 5-6, note; Gehler, *Physikalisches Wörterbuch*, Leipsic, 1787, I, 16; also in the 1825 edition, IA, 136; Barlow, *Phil. Trans. R. Soc.*, 1833, II, 670.

[19] Cavallo, *Treatise on magnetism*, 3rd ed. London, 1800, 299-320. The first edition, 1787, does not refer to "Adsiger."

[20] Wenckebach, Mulder's **Natur en Scheikundig Archief*, Rotterdam, 1836, 275-297; French trans. by Hooiberg in *Annali di Matem.*, VII, 159-168.

[21] Following Wenckebach, Libri, *Histoire des sciences mathématiques en Italie*, Paris, 1838, I, 383, II, 70-72, with notes, dealt with the matter at some length, giving translation (I, 487) of the genuine *Epistola* from the copy in the Bibliothèque Nationale, Paris (MS. Latin, No. 7378A). He acknowledged the value of Wenckebach's researches. About the same time, Humboldt took a hand in the business, but only succeeded in darkening counsel. In his *Examen critique de l'histoire de la géographie du Nouveau*

Continent, Paris, 1837, I, 240, he says "En Europe, cette déclinaison avait déjà été trouvée par Peregrini en 1269." But in vol. III, 30, he contradicts himself by referring to this as "une pretendue observation." He then proceeds to quote Libri's opinion on the Leyden manuscript, without acknowledging Wenckebach, whose paper he had, apparently, never seen. He represents Libri as saying that the passage relating to the declination is *not* in the Leyden manuscript. Libri did not say this. What he did say was, that the passage was not in the genuine copies of the Peregrinus letter.

It is interesting to note that, shortly before this, Humboldt had been criticising Columbus as being the victim of disordered ideas, of which his writings carry the imprint! !

[22] Bertelli; see notes [**103**, *a*, *d*, *h*], of Chapter I.

[23] It would be tedious and unprofitable to add a list of writers who have accepted or reproduced Thévenot's original assertion. But the reader who wishes examples of an error repeated half a century and more after its exposure can find them in Justin Winsor's *Christopher Columbus*, London, 1890, p. 199 in Hutchinson's *Advanced textbook of electricity and magnetism*, London, 1917, I, p. 4, and—*quandoque bonus dormitat Homerus*—in Poynting and Thomson's *Electricity and magnetism*, London, 1914, p. 169.

That this interpolation in the Leyden manuscript should have remained so long without notice was most probably due to the limited circulation of the journal in which Wenckebach's paper first appeared. For example, Brewster, writing in 1857 (*Encyclopedia Britannica*, 8th ed., XIV, 2) does not refer to it, although it would have cleared up what seemed to him doubtful.

[24] Winter, *Ann. Hydrogr.*, LXIII (1935), 352-363.

[25] Thorndike, *History of magic and experimental science*, Washington, 1934, vol. III, 499, 501. Also contains details regarding the life and writings of Henry of Hesse. The *Contra coniunctionistas* is in British Museum manuscripts, Sloane, 2156, the passage being in I, 15. The context is obscure and appears to have little or no bearing on the subject, except that it might have been introduced as an example illustrative of principles which are put forward with regard to matters of a totally different kind. Professor Thorndike informs me that the text of *Contra coniunctionistas* has been edited by Pruckner in his *Studien zu den Astrologischen Schriften des Heinrich von Langenstein*, Berlin and Leipsic, 1933, 139-206.

It is stated in the text that the contribution to the subject by Henry of Hesse is the only one of its kind known in the fourteenth century. There is, however, another possible reference which must be mentioned. In his *Parlement of Foules*, line 117, Chaucer refers to the planet Venus "as wisly (surely) as I see thee in the north-northwest." That the poet should have supposed the planet to have been visible in this direction from, say, London, is surprising. Skeat's note on the passage (*Works of Geoffrey Chaucer*, London 1894. Vol. I, p. 509) is not very illuminating. Professor Hugo Lange and Dr. A. Nippoldt are of opinion that the direction is correctly given if we suppose that Chaucer was using an azimuth *taken from a compass-direction*, and that the magnetic declination at London in 1380 was 23½° west. That Chaucer was acquainted with the compass, and even used it for such purposes, is perfectly possible. That he was also aware of the magnetic declination, especially when of large amount, is also possible and even probable. But it is not an inevitable deduction from the passage and I do not gather that the writers mentioned put it forward as such. The whole matter seems to depend rather upon the view which may be taken as to the nature of the secular change in declination, and should therefore be dealt with in connection with that part of the subject. See Lange, *Anglia, Zeitschrift für englische Philologie*, Bd. XLVIII, Heft 3/4, and *Forschungen und Fortschritte*, 20 April, 1935. Professor Lange has very kindly sent me (July 27, 1936) a paper, in typescript, giving his own and Dr. Nippoldt's views on the matter, but I am not aware of this having been published as yet.

[26] Toaldo, *Saggi di studi veneti*, Venice 1782, p. 61. Bianco's map has been reproduced by Peschel, *Facsimile dell'atlante di Andrea Bianco*, Venice, 1869, German ed. of same year. Also by Mayer, *Die Entwickelung der Seekarten*, Vienna, 1877.

[27] Formaleoni, *Saggi sulla nautica antica dei Veneziani*, Venice, 1783.

[28] Humboldt, *Cosmos*, English trans., London, 1849, IV, 53.

[29] Libri, [**21**], II, 72.

[30] Mottelay, *Bibliographical history of electricity and magnetism*, London, 1922, p. 62.

[31] Bauer, *Principal facts of the Earth's magnetism*, U. S. Coast Geod. Surv., Washington, 1909. His conclusions were drawn rather from the map itself than from the attached diagram.

[32] Bertelli, *Bull. bibliogr. st. sci. mat. fis.*, I, 411.

[33] Bertelli, *Raccolta di documenti e studi pubblicata della Reale Commissione Columbiana nel quarto centenario della scoperta di America*, Rome, 1892. Part IV, 88.

[34] Heathcote, (a) *Sci. Prog.*, XXVII (1932), 89; (b) *Annals of Science*, I (1936). Apart from other points of value, these papers give many references to early charts, and their interpretation by Gelcich, Kretschner, Peschel, and Breusing.

[35] Libri, [21] II, 71. Libri also refers to a German map in the Paris Bibliothèque Nationale, which shows the declination, but no particulars are given.

[36] MSS. Italiens, histoire et géographique, No. 42. Bertelli [32], Tav. IV, has reproduced the compass-card forming part of the map.

[37] Molini, *Documenti di storia italiana*, Florence, 1836, I, lxix.

[38] Hellmann, *Zs. Ges. Erdk., Berlin*, XXXII, 112-136.

[39] Wolkenhauer, *Mitt. Geogr. Ges., München*, I, 161-260.

[40] Hellmann quotes from (*a*) Doppelmayer, *Historische Nachricht von den Nürnbergischen Mathematicis und Künstlern*, Nuremberg 1730; (*b*) Grimm's *German Dictionary*, Leipsic, 1873, V, 1685; (*c*) Sebastian Munster, *Horologiographia*, Basle, 1533, p. 7; but chiefly relies on (*d*) a passage in a letter addressed to Prince Albrecht of Prussia by George Hartmann, dated March 4, 1544. Reference may be made to the *Oxford English Dictionary* for different meanings of the word "compass."

Hartmann's letter, which is of considerable importance in the history of terrestrial magnetism, was found in the archives of Königsberg, and was first published by Voigt in Raumer's **Historisches Taschenbuch*, vol. II; later by Moser in *Repertorium der Physik*, vol. II; again by Voigt in his *Briefwechsel der berühmtesten Gelehrten des Zeitalters der Reformation mit Hertzog Albrecht von Preussen*, Königsberg, 1841; and lastly by Hellmann in his *Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus*, Berlin, 1898, No. 10. In this letter, Hartmann refers to observations of magnetic declination made while he was in Rome, and their date can be almost certainly fixed (see *a*, above, p. 57) as in the year 1510. For errors arising from ascribing the observations to the year of the letter, see Hellmann [38], p. 128, and Wolkenhauer [39], p. 183.

[41] I quote from Hellmann [38], p. 113, not having seen either document. Nor have I been able to ascertain their present location.

[42] A complete list of the works of Regiomontanus will be found in Ersch and Gruber's *Cyclopedia*, vol. III. The instruments used, and observations made by him in Nuremberg are detailed in a posthumous work, **Scripta clarissimi mathematici Joh. Regiomontani*, Nuremberg, 1544. Details regarding his life and work may be gathered from Gassendi's biography, *Tychonis Brahei equitis Dani astronomorum coryphaei vita*, Paris, 1659.

Thorndike, *Science and thought in the XV century*, New York, 1929, ch. VIII (with some valuable references), is of opinion with regard to Peuerbach and Regiomontanus, that "their importance has been exaggerated at the expense of the preceding period and their own contemporaries." This criticism is intended to apply to their astronomical and mathematical work, and whether well-founded or not—a point upon which I am not qualified to offer an opinion—has no bearing on that aspect of their work with which we have to deal here.

[43] See [40].

[44] See Doppelmayer [40, *a*].

[45] Le Monnier, *Hist. Acad. sci.*, Paris, 1771, p. 29.

[46] In Hellmann's original paper [38], this is given as 10°, but was corrected in the translation which appeared in *Terr. Mag.*, IV, 79. With regard to the value 7°, see Fritsche, *Die Elemente des Erdmagnetismus und ihre saecularen Aenderungen während des Zeitraumes 1550-1915*, St. Petersburg, 1900.

[47] See [39], 257.

[48] Mayer, *Mitt. aus dem Gebiete des Seewesens*, 1878, Bd. 7, p. 331. See Spitzer, *La Collection Spitzer*, Paris, 1890. This collection has since been dispersed by auction, and the present whereabouts of the instruments appears to be unknown.

[49] The sun-dial of 1451 has been separately described by Hellmann in *Met. Zs.*, XXIII, 145-149. He paid particular attention to the question as to whether the

mark indicating the magnetic north had been made at the time of construction of the instrument. This matter had been raised by some critics, who suggested that it might have been added later. But Hellmann was perfectly satisfied that the mark had been made on the instrument as originally constructed.

[50] Wolkenhauer [39], 193-199. Fuller details are given in his later paper, *Deutsche geographische Blätter*, XXVI, 120-138. The maps have been republished by Wolkenhauer in *Erhard Etzlaubs Reisekarte durch Deutschland 1501*, Berlin, 1919, which contains many references to the literature of the subject. Etzlaub's year of birth is unknown, but is believed to be about 1460. In 1484 he was enrolled as a burgher of Nuremberg, and died there in 1533.

[51] *Historie del Signor Don Fernando Colombo nelle quali s'ha particolare e vera relatione della vita e de' fatti dell' Ammiraglio D. Christoforo Colombo suo padre. Tradotte nell' Italiana dal S. Alfonso Ulloa.* Venice 1571. Second edition, Milan 1614. The *French edition, Paris, 1681, is said by Thacher to have been badly translated. The English translation, included in Churchill's *Voyages and travels*, London, 1744, II, 479-604, contains several errors, some of which are referred to later. A retranslation into Spanish was given by Barcia in his *Historiadores primitivos de las Indias Occidentales*, Madrid, 1748, Italian edition, by Dulau, London, 1867. The best modern edition is that of Rinaldo Caddeo, Milan, 1930, 2 vols. It gives in the Introduction a life of Fernando Columbus (xii-xvii), deals with the question of authenticity (xix-xl), its truth and authority (xli-lviii), a list of editions (lix-lxviii), a bibliography of the *Historie* (lxxi-lxxv), and a list of works relating to Christopher and Fernando Columbus (lxxvi-lxxxvii). The edition of the *Historie* follows, and is fully annotated.

[52] The Las Casas MS of the *Journal* of Columbus was first published by Navarette in his *Colección de los viajes y descubrimientos que hicieron por mar los Españoles desde fines del siglo XV*. Madrid, 1825, 2 vols. I, 1-166.

Navarette's edition, with or without corrections, has been reproduced several times. (*a*) *Personal narrative of the first voyage of Columbus to America, from a manuscript recently discovered in Spain*, Trans. by S. Kettell, Boston, 1827. In some respects, this is incomplete, and it was scarcely accurate to announce the discovery as "recent" when it had been known to scholars for nearly forty years. (*b*) *Relations des quatre voyages entrepris par Christophe Colomb*, Trans. by Verneuil and de la Roquette, Paris, 1828, 3 vols. II, 1-345. (*c*) Italian translations by *Marmocchi, 1840, and *Torre, 1864. (*d*) Varnhagen, *La verdadera Guanihani*, Santiago de Chili, 1885. (*e*) Los Rios, *La parte de los Montaneros en el descubrimiento de America*, Santander 1892. (*f*) The best edition, with valuable notes and references, is that by Lollis in the monumental Italian publication, [33], vol. I, 1-119. (*g*) Markham, *The journal of Christopher Columbus during his first voyage*, 1492-93, Hakluyt Soc., London, 1893. (*h*) Thacher, in his *Christopher Columbus*, New York, 1903, 3 vols., I, 512-570. (*i*) Brooks, *Christopher Columbus, his first voyage to America*, London, 1925; this is a close copy, even to the notes, of (*a*) above. The latest version is Duff's *The truth about Columbus*, London, 1936. The author states in his preface that he has "relied upon original sources," and that he "found that without exception English translations of the *Journal*, letters and documents, were either incredibly inaccurate or incredibly wooden, thus providing at source a basis for misinterpretation or misrepresentation." The original sources of the *Journal* are the two Las Casas manuscripts, and neither Navarette's, nor any other, edition or version of them. Had the author relied on this original, he would have avoided adding to the list of incredible inaccuracies, for it will be found that under September 9 and 13 he repeats Navarette's mistakes in transcription.

Las Casas gave, with some differences in detail, a copy of his edition of the *Journal* in his *Historia de las Indias*, first published at Madrid, 1875, 5 vols., I, 261-469. This was used, in abbreviated form, by Herrera in his *Historia general de los hechos de los Castellanos en las islas i tierra firme del Mar Oceano*, Madrid, 1730, 3 vols., Dec. I, lib. I, c. IX-XX; lib. II, c. I-III.

[53] For discussions as to the birthplace of Columbus, see Washington Irving, *History of the life and voyages of Christopher Columbus*, Paris, 1828; Harrisse, *Christophe Colomb, son origine, sa vie, ses voyages, sa famille, et ses descendants*, Paris, 1884; Thacher [52, *h*], I, 230-263; Vignaud, *Etudes critiques sur la vie de Colomb*, Paris, 1905; *Christopher Columbus: Documents and proofs of his Genoese origin, Published by the City of Genoa*, Genoa, 1932.

[54] As to date, see authorities quoted in [52], especially Thacher, I, 264-285.

[55] The controversy on this point, the social position of the Columbus family, began by Bishop Agostino Giustiniano asserting in his *Psalterium Hebraeum, Graecum*,

Arabicum, et Chaldaicum, Genoa, 1516 (note to Psalm XIX), that Columbus was of common parentage, *ortus vilibus parentibus*. This was rather vehemently contradicted by Fernando Columbus in his *Historie* [51], ch. II. Further discussion by Thacher [52, *h*], Vignaud [53], and other critics has settled the matter. See also [75].

[56] As may be conclusively proved by a comparison of the earlier chapters of the *Historie* [51] with those of the *Historia* [52], the two authorities are not really independent. The conclusion under reference is only inferential, but is reached after consideration of all the relevant evidence. See the publication of the city of Genoa [53], p. xx. Markham, *Life of Christopher Columbus*, London, 1892, Ch. I, note 3, gives the best reasons for rejection of the statement by Fernando Columbus.

[57] Markham [56], Ch. I, note 3; see also references quoted by Jane, *Select documents illustrating the four voyages of Columbus*, London, 1930, Introduction, xxxvii.

[58] The sea-fight referred to took place on August 22, 1485; see Markham [56], p. 16, note.

[59] The best summary on this point will be found in Jane [57] Intr. xxxiii-xxxvi. Thacher [52, *h*], and Markham [56] formed a higher estimate of the educational attainments of Columbus than appears to be warranted. For the opposite view, see Vignaud [53] and Harrisse [53].

[60] See Jane [57] xxxvi.

[61] See Jane [57] xxxvi. Harrisse [53] points out that of all the writings we have of Columbus, there is none in Italian. When he wrote to the Pope, he used neither Latin, the usual medium, nor Italian, his mother-tongue, but Spanish.

[62] This is abundantly shown in his letter to the Spanish sovereigns, written from Haiti, October 18, 1498; the letter is printed in *Select documents* [57], II.

[63] See *Select documents* [57], II, Intr. lxxxi-lxxxiii.

[64] For example, Belloy, *Christophe Colomb et la découverte du Nouveau Monde*, Paris, 1865; Roselly de Lorgues, *L'ambassadeur de Dieu et le Pape Pie XI*, Paris, 1874, and his **Christophe Colomb*, Paris, 1856.

[65] Goodrich, **History of the character and achievements of the so-called Christopher Columbus*, New York, 1874.

[66] For example, Fernando Columbus, *Historie* [51], Ch. II. "Io non sono il primo Ammiraglio della mia famiglia." Jane [57] xx, points out that this statement is given by Fernando Columbus as coming from the letter to the nurse of Prince Juan; but the letter, as preserved, contains no passage of the kind.

[67] The story told by Columbus, and repeated by his son, of his having altered the compass in order to delude his crew, is entirely improbable.

[68] For example, see his *Journal* under September 9, 1492, when he entered the day's run as less than the actual. Also note by Jane, *Voyages of Christopher Columbus*, London, 1930, p. 331.

[69] See Jane [57], and [68] 58-60, where he deals with the reliability of the *Journal*. For a vigorous, yet temperate, expression of the need for reserve in accepting the statements by, or accredited to, Columbus, see the letter of Vignaud, in Filson Young's *Christopher Columbus and the New World of his discovery*, London, 1906, II, 373-380.

[70] Bertelli [52], (*f*), Part IV, II, p. 37, note, states that Fernando accompanied his father on the voyage to America in 1496. This is incorrect. No voyage was *begun* in 1496. The second voyage ended in June, 1496.

[71] See *Catalogue of the library of Ferdinand Columbus: Facsimile of a manuscript in Columbine Library, Seville*, by A. M. Huntington, New York, 1905.

[72] The earliest original source of information regarding the life and work of Las Casas is Remesal, *Historia de la Provincia de S. Vincent de Chyapa*, Madrid. 1619. A useful summary is given by Helps, *Life of Las Casas*, London, 1868. See also Thacher [52], (*h*), I, 113-159.

[73] *Historia de las Indias, escrita por Fray Bartolomé de Las Casas, Obispo de Chiapa*, Madrid, 1875, 5 vols.

[74] See [51].

[75] Harrisse, *Fernand Colombo, sa vie, ses oeuvres*, Paris, 1872. The grounds upon which Harrisse assailed the genuineness of Fernando's *Historie* are of interest. To begin with, Ulloa's translation has a dedication by Moleto to the Italian patrician Fornari. According to this dedication, the manuscript of the *Historie* was given to Fornari by Don Luigi, grandson of Columbus. Fornari handed it to Giambattista

de Marini, who took it to Venice and arranged for its translation into Italian by Ulloa. Now in writing an introduction to the Italian *Codice Diplomatico Colombo-Americano*, Genoa, 1823 (English ed., London, 1823), Spotorno stated, p. lxii, that Don Luigi, "persona di vita dissoluta," brought the manuscript to Genoa in 1568. Harrisse begins by pointing out that Don Luigi was arrested at Valladolid in 1558 on a charge of polygamy, was tried in 1563, and was exiled to Morocco until 1572. He could not, therefore, have been in Genoa in 1568. Secondly Martini was dead before 1567. Hence Spotorno's statement is materially impossible. Thirdly, although Fernando Columbus bequeathed his library to Don Luigi, the latter never took possession of it. The library was under seal in the house of Fernando from his death in 1539 until 1544. From 1544 to 1552, it was in charge of the monks of St. Paul's Convent, Seville, and from the latter year onwards it was in the Cathedral of Seville. Assuming that the manuscript was in the library, Don Luigi could never have had it in his possession. But beyond this, there is grave doubt as to whether the library contained the manuscript of the *Historie*. Fernando prepared several detailed catalogues of his library, but none of these mentions it. For instance, there is no entry of the kind in the facsimile printed by Huntington [71]. Nor is there any mention of it by any contemporary. None of the 450 works catalogued in Harrisse's *Bibliotheca Americana Vetustissima*, New York, 1866-72, refers to it. The earliest reference of a reliable kind is to Ulloa's translation and not to any original thereof. Harrisse ventures the hypothesis that the *Historie* was really written by Fernand Perez de Oliva, whose manuscript certainly existed in the Library of Fernando Columbus. Another interesting point regarding the *Historie* and its authorship is its reference to Bishop Agostino, who is mentioned in Chapter II as the author of a "chronicle." This unfortunate prelate (he was drowned at sea) wrote two works. The first, his Psalterium [55], was published in 1516 and contained some reflections on the parentage of Columbus. The second was his *Castigatissimi annali della eccelsa et illustrissima Republica di Genoa, da fideli et approvati scrittori*, published at Genoa, in March 1537, and in which were repeated the statements made in the *Psalterium*. The question is, to which does the *Historie* refer? The term "chronicle" could scarcely apply to the *Psalterium*, but would be accurately descriptive of the *Annali*. But if to the latter, the difficulty arises that Fernando Columbus was ill for a long time before his death in 1539 and, if able for any work at all, was engaged in other matters. Hence either the reference to Agostino's work must be an interpolation or the *Historie* must have been written after the death of Fernando Columbus.

[76] D'Avezac, *Bull. soc. géog., Paris*, 1872, 1873. Harrisse's answer is in same Journal, 1874.

[77] Perogallo, *L'authenticità della historie di Fernand Colomb*, Genoa, 1884. *Duro, *R. Acad. de la Hist., Mem.* X, Madrid, 1896.

[78] See [52] (b), I, 143.

[79] See [52].

[80] This point is discussed by Vignaud, *Histoire critique de la grande entreprise de Christophe Colomb*, 2 vols., Paris, 1911, I, 18-20. Also by Jane [68], Introd. 59-60.

[81] See [52] (*h*), I, 513. Thacher also states, p. 512, that Las Casas "had in his possession, among many other of Christopher Columbus' papers, his original holograph Journal." A simple statement this, but it comes perilously near to that method in historical writing so severely denounced by F. C. Baur, namely, drawing conclusions regarding the uncertain from the unknown (*Life and works of St. Paul*, Part I, ch. VII). Evidence in support is entirely lacking.

[82] Markham [52], (*g*). Introd. p. v.

[83] Filson Young [69]. Lord Dunraven contributed a note, I, 291-322, on the navigation of Columbus. He states that, according to the *Journal*, Columbus manipulated the compass so as to make it point to the Pole Star. There is no passage in the *Journal* which bears this out. By some writers it has been suspected that he may have done so, but there is no proof. The idea may have its origin in the story told by Columbus himself, and referred to in [67].

[84] Navarette [52], (*b*).

[85] This sentence was drafted early in 1936. In view of the present unhappy state of affairs, it would be more accurate to say "formerly" instead of "now."

[86] For permission to obtain photostat copies of the relative portions of the original Las Casas manuscripts, I have to thank the Director of the National Library, Madrid, and also Mr. A. R. Hinks, Secretary, Royal Geographical Society, London, through whose good offices these copies were obtained. For a careful scrutiny of these

records, and for notes on their translation, I am much indebted to Mr. L. B. Walton, Lecturer in Spanish, University of Edinburgh.

[87] The original, Plate I, gives "norueste." Navarette gave "nordeste," which reading was accepted by those who had not examined the original, or a facsimile thereof. It also had a decisive influence on most discussions of the subject. The Italian *Raccolta* edition [52] (*f*) gives the correct reading, but strange to say, Bertelli, in the same publication, adopted Navarette's incorrect reading.

[88] The original, Plate II, gives "nordesteaban." The *Raccolta* edition gives the same, and notes Navarette's reading as a variant.

[89] Reproduction of original in Plate III.

[90] Navarette also gave "noruestean," whereas in the parallel passage in Las Casas' *Histôria*, p. 281, we have "nordesteaban."

[91] Reproduction of original in Plate IV.

[92] See [51].

[93] Churchill's translation [51], p. 524, of this passage contains three errors: "Northwestward" in the first and third sentences is translated "northeast," and "invisible" is put as "visible."

[94] Copied from original Spanish and English translation given in *Select documents* [57], II, 56, which also gives details as to the provenance of the letter.

[95] These speculations regarding a protuberant portion of the oceanic surface were current in the times of Ristoro d'Arezzo in 1282, and of Paul of Venice about 1420. But it is highly improbable that Columbus was acquainted with either work.

[96] For remarks on the cosmographical speculations of Columbus as contained in this letter, see Jane [57].

[97] The earliest reference, after that of Fernando Columbus, to the practice of adjusting the needle under the compass-card, as in the Flemish compass, is that by Norman, *The newe attractive*, London, 1581 (reprinted in Hellmann's *Neudrucke* [40], No. 10). In Ch. X, he refers to different amounts of declination for which the adjustment is made. As an interesting indication of the accuracy aimed at, in Norman's time, in laying courses at sea, he recommends that the needle be set at half a point eastwards, as the maximum error will then be only a quarter point. A fuller description of these Flemish compasses is given by *Barentz, *Description de la Mer Méditerranée*, La Haye, 1599. The principal passage is given, in English, by Heathcote [34], p. 92. In Barentz's time, the Flemish compasses allowed for a declination of 6° east. Gilbert, *De Magnete*, London, 1600, Bk. IV, Ch. viii, also refers to the matter, and gives details as to the different declinations for which allowance was made. Gellibrand, *Discourse mathematicall on the variation of the magnetic needle*, London, 1635, p. 5, condemns the use of compasses adjusted in this manner, "considering the variation is perpetually variable, according to the shipp's motion."

[98] See the letter of Columbus, in passages *J*, *K*.

[99] For these details, I am indebted to Prof. R. A. Sampson, Astronomer-Royal for Scotland.

[100] Bertelli [52], (*f*), Part IV.

[101] Wolkenhauer, *Mitt. Geogr. Ges., München*, I, Heft 2.

[102] Errera, *Sulla scoperta della declinazione magnetica e sulla storia della bussola nautica nei secoli XV-XVI*, in *Riv. fis. mat. sc. nat.*, Pavia, 1907, 85.

[103] Magnaghi, *Boll. Soc. geogr. ital.*, Ser. IV, vol. x, 595-641.

[104] Bertelli [52], (*f*), Part IV.

[105] De Castro, *Roteiro de Lisbon a Goa por De Joao de Castro*, Ed. by Corvo, Lisbon, 1882.

[106] Schott, *U. S.-Coast and Geodetic Survey report for 1880*, p. 416.

[107] See [103].

[108] See [83].

[109] See [52].

[110] See [52].

[111] See [52].

[112] See [52].

[113] See [52].

[114] See [21].

[115] See [52], (*f*), Part IV.

[116] Fox, *U. S. Coast and Geodetic Survey report for 1880*, p. 405, states that compass-sights only came into use in the following century.

[117] There is some doubt as to the value of the league used by Columbus. See Fox [116], p. 401.

[118] The view presented in this paragraph was first put forward by D'Avezac, *Bull. soc. geog., Paris*, Series IV, XIX, 356; also *C. R. Acad. sci.*, LXX, 1080.

[119] Heathcote [34] gives a different explanation, with which I am unable to agree.

[120] For example, Pedro de Medina, *Arte de navegar*, Valladolid, 1545, lib. vi. Many years later than Columbus, a belief was current that different degrees of magnetisation of the compass-needle produced different amounts of deviation from the geographical meridian. This was finally disposed of by Sellers, *Phil. Trans. R. Soc.*, 1666, p. 473.

[121] See [52], (*f*), Part IV.

[122] The letter is printed by Harrisse [75], 470-471. Bertelli's notes [52], (*f*), Part IV, ch. II, p. 19, on the letter should be consulted along with it.

[123] See [52], (*f*), Part IV.

[124] Bertelli's reference is to *Pubblicata da Guglielmo Brenna*, Florence, 1886, p. 5, but I have not had an opportunity of verifying this. The passage is as follows: "Maraviglia me fe assai el variare della bussola, non solamente la nostra, ma di tutte l'altre dell'armata, che la fiamma della tramontana, passando noi di Ginea, convincio a inclinare, secondo el parere de piloti, una quarta verso libeco, e alsi passando al campo di Buona Speranza per la inclinazione a scirocco; non ho tanto discorso ch'i'sappia ritrare se dalla calamita o dal sole o dalla regione procede." See Bertelli's notes [122].

[125] Bertelli [122].

[126] **Lettere edite ed inedite di Filipo Sassetti*, Florence, 1855, 206, 207.

[127] **Regionamenti sopra la varietà dei flussi et riflussi del mar Oceano occidentale*, Venice, 1574. Reference is on second page, unnumbered.

[128] Oviedo y Valdes, *Historia general y natural de las Indias*, first published, Madrid, 1535. It was preceded in 1525 (or 1526) by his *Summario de la natural hystoria de las Indias*. The best edition of the *Historia general* is that published in 1851 by the Royal Academy of History, Madrid (see lib. II, cap. 5, 11). In the latter chapter he refers to the needles being unreliable.

[129] Heathcote [34], p. 85, who points out that this may account for Fournier, *Hydrographie*, Paris, 1643, Bk. XI, 541, ascribing the discovery of declination to Oviedo. Most probably, Fournier consulted the French translation, which renders Oviedo's word for needles (aguajes) as "agitations des eaues."

[130] See [120].

[131] Pedro Nunes, *Tratado da sphera com a theorica do Sol e da Lua.*, Lisbon, 1537.

[132] For example, Herrera, *Historia general de los hechos de los Castellanos en las islas y tierra firme del Mar Oceano*, 4 vols., Madrid, 1601-15. Decad. I, Bk. I, ch. ix. English trans. by Stevens, London, 1740, I, p. 35. The quotation by Herrera is almost exactly as in Ulloa's translation.

[133] See Heathcote [129].

[134] Formaleoni [27], p. 53; see also Bertelli [32], 411-413.

[135] Harrisse, (a) *Jean et Sebastian Cabot: leur origine et leurs voyages*, Paris, 1882, Ch. I. Copies of all original documents are given in appendices. (b) *John Cabot, the discoverer of North America, and Sebastian Cabot his son*, London, 1896, pp. 29, 37. Reference may also be made to Biddle, *Memoir of Sebastian Cabot*, London, 1831, but with due weight assigned to Harrisse's criticism (*v. s.* p. 372).

[136] The only known copy of this map is now in the Bibliothèque Nationale, Paris. Full details, along with a reproduction of the map, are given by Harrisse [135], (*a*). Several writers refer to another copy having been found in Germany, but I have not been able to trace these to their source. See Harrisse, *v. s.* p. 153, note 1.

[137] See [120]. This book created much interest throughout western Europe. Later editions, Lyons, 1553, Venice, 1555, Rouen, 1573.

[138] The original letter of Contarini, dated 31 December 1522, is in the Biblio-

theca Marciana, Venice, and was published by Rawdon Brown, *Calendar of state papers and manuscripts relating to English affairs, existing in the archives and collections of Venice, and in other libraries of Northern Italy*, London, 1864-69, III, No. 558; republished by Harrisse [135], (*a*), 347-351.

[139] Bertelli [52], (*f*), Part IV, ch. xi, 63, gives a list of writers who dealt with this question. To this should be added, Besson, *La cosmolabe*, Paris, 1567, and Bourne, *Regiment of the sea*, London, 1577. That the idea occurred to Columbus was asserted by Humboldt [21], III, 38-40, but this cannot be sustained. The first to call the method in question was Cardan, *Practica arithmetica*, Milan, 1539 (*Opera*, III, 474). Gilbert [97] devotes Ch. IX, of Bk. IV to the question. He evidently thought there was more hope of finding the latitude from the dip (Bk. V., Ch. VIII).

[140] Livio Sanuto, *Geographia distinta*, Venice, 1588. Lib. I, cap. 2A.

[141] Gilbert [97], p. 4.

[142] Kircher, *Magnes, sive de magnetica arte*, Rome, 1641, p. 33.

[143] Fournier [129], 541, 545.

[144] Fontenelle, *Hist. Acad. Sci.*, 1712, Paris, 1714.

[145] Harrisse [135], (*b*), 290. Fontenelle evidently consulted the edition of Cabot's map published by Kochaff, *Variorum itinerarium delicia*, Harborn, 1594, p. 773. See Foscarini, *Della letteratura veneziana*, Venice, 1752, I, 439.

[146] For bibliographical and other details regarding Ruysch's map, see Harrisse [135], (*a*), 164-165. The map has been reproduced in Nordenskiöld's *Facsimile atlas*, Stockholm, 1889.

[147] Lelewel, *Géographie du moyen âge*, Brussels, 1852.

[148] See [128].

[149] See [120].

[150] Martin Cortés, *Breve compendio de la sphera y de la arte de navegar*, Seville, 1556; reproduced in Hellmann's *Neudrucke* [40].

[151] Nunes, see [131].

[152] Navarette, *Disertación sobre la historia de la náutica, y de las ciencias matemáticas*, Madrid, 1846, p. 179. In 1536, when this meeting was held, the pilots could only agree on three points at which the declination had been satisfactorily determined. "En Santo Domingo noruestaba dos cuartas el aguja, en la Habana dos y media, y tres en la Nueva España." But perhaps they were only confining attention to the West Indies.

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