

## CHAPTERS IN THE HISTORY OF TERRESTRIAL MAGNETISM

BY A. CRICHTON MITCHELL, D. SC.

### CHAPTER III—THE DISCOVERY OF THE MAGNETIC INCLINATION

1—It has been shown in Chapter II\* that the literature of the fourteenth century contains references to the fact that the direction of a suspended or pivoted magnetic needle is not, always and everywhere, that of the geographical meridian at the place of observation; that for nearly 200 years the existence of this variation was either denied, or doubted, or ascribed to irregularity in the magnetization of the needle or to faulty methods of observation; but that finally, about the middle of the fifteenth century, it was recognized as a general or world-wide phenomenon. Thus was reached the conception of magnetic declination.

The increasing extent to which the magnetic needle was employed in navigation and as an adjustment in sun-dials, together with its observed deviation from the geographical meridian, must have led to greater care in the construction of nautical compasses and to closer observation of their behavior in varying conditions. To these circumstances we may attribute the next discovery in the science, that of the magnetic inclination, or dip.

In its historical aspect, this discovery presents no difficulty to the student. He has no longer, as in earlier phases of development of the science, to contend with documents of more than doubtful origin, with careless transcription or translation, or with deliberate alterations in manuscripts. He has now reached the stage at which the printed word is keenly examined, criticized, and, if need be, answered. There is also the advantage that merely verbal dispute has given place to the consideration of objective fact, and in consequence there is an increased clarity in the expression of physical ideas.

2—The earliest known observation of the phenomenon of magnetic inclination was that reported by Georgius Hartmann, Vicar of St. Sebaldus, Nurnberg, in a letter addressed by him to Count Albrecht of Prussia, and dated March 4, 1544. [1]. He constructed a needle a few inches in length, adjusted it to remain horizontal when balanced at its middle point, magnetized it with the lodestone, and replaced it on the pivot. He then found that it showed an inclination of about  $9^{\circ}$ . Nothing is stated in Hartmann's letter as to the plane in which the needle dipped by this amount. The value of the dip at Nurnberg in 1544 is not *accurately* known, but judging from later observations elsewhere and from the approximately known secular change, it is at once obvious that this observation of Hartmann's is very far below the true value. [2]

The existence of this letter was unknown until it was found in the

\*Terr. Mag., 42, 241-280 (1937).

Königsberg archives in 1831. Apparently, Hartmann had adopted no other means of communicating his discovery to those interested in the subject, nor does he seem to have made any further investigation of the matter. In consequence, as Hellmann pointed out [3], his letter had no influence on the progress of the science.

3—In 1581, Robert Norman, a maker of nautical instruments in London, published his book, *The Newe Attractive*, [4]. In Chapter III, he explains "By what means the rare and strange Declining of the Needle, from the plaine of the Horizon, was first found," and in Chapter IV, "How to finde the greatest Declining of the Needle under the Horizon." It is to be noted that he states explicitly that he had neither "heard nor read of any such matter."

Norman's demonstration of the phenomenon of dip differed from that of Hartmann, and may be described as follows: He had frequently observed that having prepared a (non-magnetized) needle which would balance horizontally on its pivot, its subsequent magnetization by the lodestone was followed by its north-seeking end moving downwards below the horizon. Having discussed this with "certayne learned and expert men acquainted in this matter," he was advised to construct some instrument which would measure the angle through which the needle inclined from the horizon. Accordingly, to a steel needle of six inches' length, he fitted a short axle about which the needle could revolve freely in a vertical plane. He took pains to "perfect" the needle so that when supported by its axle, it would remain either horizontal or in any other position in which it might be placed relative to the horizon. The needle was mounted, with its axle horizontal, at the center of a graduated circle placed in the plane of the magnetic meridian, the needle being free to move in that plane. It was then magnetized by the lodestone, and at once exhibited the dip, which he measured as being  $71^{\circ} 50'$ . It would seem that these experiments were carried out in the year 1576 [5].

4—Hartmann was thus the first to observe the phenomenon of magnetic inclination, but measuring the angle of dip by a faulty method, he estimated its amount at about one-eighth of its real value. Norman, 32 years later, observed it independently, and measured the dip in the plane of the magnetic meridian with tolerable accuracy.

So far, then, as the history of this discovery is concerned, the matter is quite plain. But this has not been the manner of exposition adopted by some commentators. When Voigt [1] first published Hartmann's letter, he confined his statements to the facts as given above. So also Hellmann [1], who added a reference to Norman's work as "an epoch-making book." But Moser [6], and those who have followed him, attempted to belittle Norman's work, by representing it as merely the invention of a particular form of dip-needle, and by calling in question the accuracy of his measurement. It is quite evident that Moser had not studied Norman's book. He does not quote from it, but uses Hansteen [5] as his authority. But Hansteen did not, as Moser represented, question the accuracy of Norman's measurement. What he did do was to investigate generally the conditions of equilibrium of a dip-needle, and to indicate the various sources of error. As Chrystal [7] has pointed out, Moser might by consulting Norman's book, have found that it

gave an account of the matter which showed a much clearer appreciation of the problem than anything said by Hartmann.

5—Two points relating to Norman's observations call for remark. The first is, that he does not explain why the vertical plane chosen for exhibition of the phenomenon, and for measurement of the dip, was that of the magnetic meridian. Most probably it was suggested by the indications of the horizontal needle. Possibly, also, his theory of a "Point Respective" towards which he supposed the needle to be attracted, was formed at an early stage of his inquiry, and as this "Point" could only, according to his theory, lie in the magnetic meridian, he confined his attention to that particular plane. This leads one to observe that he did not notice—or, at least, did not mention—the result obtained in the plane of the magnetic prime vertical. Had he done so, it would almost certainly have led to a revision of his ideas with regard to the "Point Respective." However that may be, he realized that the seat of the attraction lay in the Earth and not in some point in the celestial sphere. He thus prepared the way for the more general ideas of Gilbert. Indeed, it is not too much to say that without Norman's discovery, the *De Magnete* would never have been written.

The second point is that, in Norman's time, the conception of a couple, in mechanics, had not been reached, and for lack of this, his explanation of the magnetic inclination was defective. But he came nearer to this conception, or to one of its results, than did his contemporaries, for he showed by several distinct methods that the Earth's magnetic force has no effect of translation on a magnet. He was the first to prove that magnetization of a steel needle has no effect upon its weight.

#### *Bibliographical references and notes*

[1] Hartmann's letter of 1544 was first published by Voigt in Raumer's *Historisches Taschenbuch*, Leipzig, 1831, II, 328-340; secondly, by Moser in Dove's *Repertorium der Physik*, II, 129-136; again by Voigt in his *Briefwechsel der berühmtesten Gelehrten des Zeitalters der Reformation mit Herzog Albrecht von Preussen*, Königsberg, 1841; and lastly, with a facsimile impression of the original, by Hellmann in *Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus*, Berlin, 1898, No. 10.

The passage referring to the inclination is as follows:

"Zu dem anderen, so finde ich auch diess an dem Magneten, dass er sich nicht allein wendet von der Mitternacht und lenket sich gegen den Aufgang, um 9 Grad mehr oder minder, wie ich es gemeldet habe, sondern er zeucht auch unter sich. Diess ist also zu erweisen. Ich mache ein Züngle eines Fingers lang, das nur fleissig wagrecht oder wasserwagrecht auf einem spitzigen Stift steht, also dass solches nirgends sich zu der Erde neige, sondern an beiden Orten gleich in der Wage stehe, so ich aber der Oerter eins verstreich, sei gleich welches Ort sei, so bleibt das Züngle nicht mehr wagrecht stehen, sondern fällt unter sich etwa um 9 Grad mehr oder minder, Ursach, warum das geschieht, habe ich Königl. Majestät nicht wissen anzuzeigen."

[2] This error is so large that it leads one to speculate as to its probable cause. In the first place, we have absolutely no information either as to the form of the needle he used or as to the manner in which it was supported on its pivot. All that can be said on this point is that, even with the best arrangements then possible, there was room for considerable error in the determination of inclination from a pivoted needle. Another way of looking at it is to suppose that Hartmann measured the inclination with more or less accuracy, but stated the amount wrongly in his letter. For, by some kind of *lapsus calami*, he might have repeated the amount of the declination, to which he referred in an earlier part of the letter, and which was also "9 Grad mehr oder minder."

[3] See [1]. Hellmann's *Neudrucke*, No. 9, Introduction, p. 15.

[4] The different editions of Norman's book are dated 1581, 1585, 1596, 1614.

It was also added as an appendix to the two editions of Whiston's book, *The Longitude and Latitude found by the Inclinator or Dipping Needle*, London, 1719, 1721.

[5] In none of the various editions of Norman's book is any statement made as to the year in which his observations were made. Yet the year 1576 is assigned to them by many recent commentators, but in no case with any authority being quoted. It has not been easy to track this statement up to its source, but it would seem to have originated in a statement by Henry Bond in his book *The Longitude Found*, London, 1676. On p. 11, he says that "some years since" (that is, before 1676), he had seen a dip-needle which had been made by Norman in 1578. Again, on p. 29, he states that Norman discovered the dip in 1576, "or else one year after." Whiston (see [4] above, 1719 edition, pp. 4-5) repeated this statement of Bond's. Its next appearance was in Hansteen's *Magnetismus der Erde*, Copenhagen, 1819, p. 38, and as this had a very much wider circulation than the works of Bond or Whiston, it was taken up by later writers.

[6] See [1], p. 133.

[7] G. Chrystal, *Encyclopaedia Britannica*, 9th ed., XV, 221, note.

*Edinburgh, Scotland,*  
*December 31, 1938*