Gauß' Method for Measuring the Terrestrial Magnetic Force in Absolute Measure: Its Invention and Introduction in Geomagnetic Research

by

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Introduction

Gauß' colleague at Göttingen, Wilhelm Weber, wrote the following lines in a letter to Carl August von Steinheil in Munich on November 30, 1832:[1]

> Seine erste magnetische Abhandlung wird nächstens im Druck erscheinen, und Sie werden gewiß bewundern mit welchen einfachen Mitteln er die absolute Declination und deren Variation bestimmt hat. An Feinheit stehen diese Beobachtungen den astronomischen in keiner Weise nach, und können sehr gut mit denselben verbunden werden, so daß gewiß bald an mehreren Sternwarten dieses Verfahren wird nachgeahmt werden.
>
> Am interessantesten scheinen mir aber seine Messungen der absoluten Intensität des Erdmagnetismus zu sein, die ganz unabhängig sind von der Beschaffenheit der dabei angewandten magnetischen Hülfsmittel.

The work referred to by Weber is Gauß' *Intensitas vis magneticae terrestris ad mensuram absolutam revocata,* which was presented to the Royal Society of Sciences in Göttingen on December 15, 1832, and published in the following year.[2] Clemens Schaefer, in his long essay "Über Gauß' physikalische Arbeiten", in the 11th volume of Gauß'

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Werke, has referred to it as "die erste reife Frucht von Gauß' magnetischen Arbeiten".[3] It is important for two main reasons. Firstly, the terrestrial magnetic force is for the first time properly expressed in absolute units, and secondly, as Weber remarks in the letter quoted above, a method is introduced for the measurement of the horizontal component at a given place in absolute units and independently of the magnetic properties of the bar or needle used.

In the ten years following its invention, Gauß' method was extensively applied in geomagnetic research which had become an exciting research field and in which there was extensive international co-operation. Although mathematically sound, Gauß' method presented some unforeseen problems in its practical implementation, the most important of which was the manner of carrying out the elimination of unwanted coefficients in the terms of the expansion which expresses the action of a deflecting magnet on another freely suspended. The solution of this problem required the ingenuity of a number of men working in observational and experimental physics. In this paper the historical origins and discovery of Gauß' method are treated as well as the problems encountered in the implementation of the method in geomagnetic research.

Theory of Gauß' Method

The theory of the method as set out in the *Intensitas vis* is as follows. A freely suspended magnetic needle or bar is made to oscillate in the horizontal plane under the sole influence of the terrestrial force. In 1832 Gauß used bars 30 cm long and weighing about a kilogram; later on much heavier bars were employed. The number of oscillations in a given period is proportional to the intensity of the force (viz. of the horizontal component), to the statical moment of the magnetism of the needle and to its moment of inertia with respect to the axis of rotation. The latter is found experimentally by a method given by Gauß himself (*Intensitas vis,* article 10) and observation of the period of oscillation gives the product of the magnetic moment (M) and the horizontal component of the force (T).

To separate the two quantities a second or auxiliary needle is introduced: the main magnet is removed and the auxiliary one is put in its place and similarly suspended. The latter is then observed under the joint action of the principal magnet and the horizontal component and the quotient of M and T is found. The product and the quotient being known, M is eliminated and T determined.

In the second part of the determination the auxiliary magnet can be observed either in a state of motion or of static equilibrium. In the first method the principal magnet is placed at a suitable distance in the magnetic meridian and in the horizontal plane in which the suspended needle moves. Two positions are possible – with like and opposite poles facing – and two periods of oscillation are observed under the combined action of the horizontal component and the principal magnet. The relationship of the horizontal component to the action of the principal magnet is then obtained from a comparison either of the periods of oscillation for each of the two orientations of the needles or that for one of these with the period of oscillation of the suspended auxiliary needle under the action of the horizontal component alone.

This "modus prior", Gauß admitted in the introduction, was essentially the same as that proposed by Poisson a few years earlier, in 1825 in fact. Furthermore a similar idea is found in Biot's account of the determination of the Biot-Savart law published in 1824. In 1820, shortly after Oersted's discovery of the magnetic field produced by a current, Biot and Savart had determined the relative strength of the field by observing the rate of oscillation of a magnetic needle suspended at various distances from a long straight wire. Where the earth's field could not be neutralized by the field produced by the current they introduced a term to compensate for the remaining field.[4] There is no evidence however that Gauß knew of, or was anyway influenced by, the work of Biot and Savart.

In the second method the principal magnet is placed so that its axis makes an angle (e.g. a right angle) with the meridian and passes through the point of suspension of the auxiliary magnet. The latter is then deflected from the meridian and from the angle of deflection one obtains a relationship between the horizontal component and the action of the deflecting bar. In outlining the method in the introduction to the paper, Gauß admitted an intrinsic fault or defect in the practical application of the method in consequence of the errors arising in the series of observations at various distances which become necessary in order to eliminate the unknowns due to the specific character of the needle. He writes:

> Die eigentliche Schwierigkeit liegt darin, dass aus den in mässigen Entfernungen beobachteten Einwirkungen der Nadel ein Grenzwerth berechnet werden muss, der sich auf eine gewissermassen unbegrenzt grosse Entfernung bezieht, und dass die zu diesem Zwecke nothwendigen Eliminationen um so mehr von den kleinsten Beobachtungsfehlern getrübt, ja sogar völlig unbrauchbar gemacht werden, je mehr Unbekannte, die von der besonderen Beschaffenheit der Nadel abhängen, zu eliminiren sind: auf eine kleine Anzahl von Unbekannten kann aber die Berechnung nur dann gebracht werden, wenn die Einwirkungen in Entfernungen geschehen, welche im Verhältniss zu der Länge der Nadeln ziemlich gross sind und deshalb selbst sehr klein werden.

Gauß saw the solution of this difficulty in developing the practical means for the exact measurement of such small deflections. He entertained high expectations of being able to surmount these difficulties with the assistance of Wilhelm Weber.

In developing the theory of the second method (articles 12–20), Gauß obtained for the deflection v at distance R:

$$\tan v = FR^{-3} + F'R^{-5} + F''R^{-7} + \dots$$

Two methods of placing the deflecting bar are outlined. In the first method its axis and middle point both lie in a line at right angles to the magnetic meridian and passing through the centre of the suspended magnet. In the second method the deflecting magnet is still at right angles to the meridian but its mid point now lies in the meridian, the vertical plane passing through the centre of the suspended magnet, with its poles now on opposite sides of the meridian. In both methods four positions of the deflecting magnet at a given distance are possible (viz. to the east or west and to north or south of the suspended magnet respectively, and with the poles reversed in each position) and v is taken as the mean of four observations, thus eliminating any asymmetry.

The first of the coefficients F is found to be

$$\frac{2\,m\,M}{mT + \theta},$$

using the first method of placing the deflecting magnet, and

$$\frac{m\,M}{mT + \theta},$$

using the second method. M is the magnetic moment of the principal or deflecting magnet, m that of the suspended or auxiliary magnet, T the horizontal component and θ the coefficient of torsion of the suspension. Gauß expressed a preference for the first method of placing the deflecting magnet as it provides a value for F which is twice as large.

He furthermore recommended that only this method be employed in order that the full series of observations necessary to determine the horizontal component could be carried out in a relatively short interval, during which any change in the magnetic conditions of the magnets would be negligible.

The advantage of Gauß' method is that terms containing negative even powers of R are suppressed and do not appear in the expansion. The calculation gains in accuracy as the number of coefficients to be eliminated, and hence the number of observations to be made, is reduced.

Gauß now gave an account of a series of experiments undertaken at Göttingen in June 1832, using the two methods of placing the deflecting magnet outlined. In these experiments he verified that the exponent in Coulomb's Law is 2, i.e. that the attractions and repulsions vary with the inverse square of the distance. Observations of deflections were made at 15 distances ranging from 1.1 to 4.0 metres (Article 21). Treating the results obtained using the method of least squares he obtained the following two equations:

$$\tan v = 0.086870\, R^{-3} - 0.002185\, R^{-5}$$
$$\tan v' = 0.043435\, R^{-3} + 0.002449\, R^{-5}$$

Comparison of the observed values with those calculated using these formulae indicated good agreement, which could be improved by taking further precautions. From the results he concluded that all but two of the terms of the expansion could be ignored, provided the deflecting magnet was placed at distances not less than four times the length of the magnet (Article 22).

By making observations at two distances R and R' (corresponding deflections v and v') the coefficient F is found by elimination:

$$F = \frac{R'^5 \tan v' - R^5 \tan v}{R'R' - RR},$$

and the quotient M/T is found from the following formula (Article 23):

$$\frac{M}{T} = \frac{1}{2} F \left(1 + \frac{\theta}{Tm}\right).$$

Gauß then provided a table of 10 values for T, which he had obtained at Göttingen between May and October 1832, using various needles according to the method described (Article 25). Finally to find the total intensity in absolute measure it was only necessary to multiply T by the secant of the inclination or dip (Article 27).

As will be seen further on, the elimination procedure proposed by Gauß, and in particular the formula for F above, became the subject of an important criticism just a few years after the appearance of Gauß' paper. At this point, however, it is necessary to take a closer look at the history of the "modus prior" referred to by Gauß in the paper and to assess its importance for the development of the principal method.

The "Modus Prior"

This method, as Gauß remarked in the *Intensitas vis,* was first proposed by Poisson in a paper "Solution d'un Problème relatif au magnétisme terrestre", read at the Académie des Sciences on November 28, 1825.[5] Poisson's method was subsequently examined and tried by Ludwig Moser and Peter Rieß who published their results in *Poggendorff's Annalen* in 1830.[6] The following is a resumé of Poisson's method in the notation adopted by Moser and Rieß.

A magnetic needle A, suspended horizontally at its centre of gravity, is oscillated about its position of rest in the magnetic meridian. The theory of the oscillation yields the formula:

$$\varphi h \cos i = \frac{\pi^2 m}{t^2},$$

where φ is the intensity of the terrestrial magnetism,
i the inclination or dip,
m is the moment of inertia of A with respect to its axis of rotation,
t is the period of oscillation, and
h $= \int \mu x dx$, where μdx is the amount of free magnetism in an indefinitely thin section of A, at a distance x from the centre of gravity.

For another needle B, similarly suspended and oscillated, a second equation is obtained:

$$\varphi k \cos i = \frac{\pi^2 m_1}{t_1^2},$$

where m_1 is the moment of inertia of B with respect to its axis of rotation,
t_1 the period of oscillation, and $k = \int \mu_1 x_1 dx_1$.

For the terrestrial magnetic intensity one then obtains:

$$\varphi = \frac{\pi^2 \sqrt{m\, m_1}}{t \cdot t_1 \cdot \cos i \cdot \sqrt{hk}},$$

in which only hk is undetermined.[7]

Both needles are now brought into the meridian in the same horizontal plane, their centres of gravity being separated by a distance r. A is now oscillated with B fixed (period of oscillation θ) and then B with A fixed (period θ_1). The theory of the method then gives the following two equations:

$$fhk + \frac{fa}{r^2} + \frac{fb}{r^4} + \ldots = \frac{m\,\pi^2 r^3\,(t^2 - \theta^2)}{2\,t^2\,\theta^2},$$

$$fhk + \frac{fa_1}{r^2} + \frac{fb_1}{r^4} + \ldots = \frac{m_1\,\pi^2 r^3\,(t_1^2 - \theta_1^2)}{2\,t_1^2\,\theta_1^2},$$

where f is the constant in Coulomb's Law, or that which expresses the interaction of two unit quantities of magnetism at a separation of unit distance. The coefficients a, a_1, b, b_1 etc., have the following values:[8]

$$\begin{aligned} a &= 6\,kh' + 2\,hk' \\ a_1 &= 6\,k'h + 2\,h'k \\ b &= 10\,h''k + 20\,h'k' + 2\,hk'' \\ b_1 &= 15\,hk'' + 30\,h'k' + 3\,h''k, \text{ etc.,} \end{aligned}$$

where

$$\begin{aligned} h &= \int \mu x dx, & k &= \int \mu_1 x_1 dx_1, \\ h' &= \int \mu x^3 dx, & k' &= \int \mu_1 x_1^3 dx_1, \\ h'' &= \int \mu x^5 dx, & k'' &= \int \mu_1 x_1^5 dx_1. \end{aligned}$$

The second set of equations provides the means of determining hk. By observing the oscillation periods of the two needles at different separations a sufficient number of equations is obtained to eliminate the unknowns fa, fb, fa_1, fb_1, etc. The equations are valid only if there is a symmetrical magnetization of both magnets. Otherwise further unknowns, divided by r, r^3, r^5, etc., have to be eliminated.

Moser and Rieß now wrote the second set of Poisson's equations in an equivalent form, introducing and then eliminating a factor which allows the reduction of the period of oscillation to an infinitely small one. In doing so they found they could reduce the number of unknowns to be eliminated from 5 or 6 to 3. The equation thus obtained is:[9]

$$\begin{aligned} &\theta_1^2 fhk + \frac{\theta_1^2}{r^2} fa + \frac{\theta_1^2}{r^4} fb + \ldots \\ &- \theta^2 fhk - \frac{\theta^2}{r^2} fa_1 - \frac{\theta^2}{r^4} fb_1 - \text{etc.} \\ &= \frac{m\,\pi^2 r^2}{2} \left(\frac{\theta^2}{t^2} - \frac{\theta_1^2}{t_1^2} \right). \end{aligned}$$

The observations reported by Moser and Rieß were given only as examples to illustrate the means of applying the method of Poisson and of calculating the horizontal component of the intensity and no great faith was expressed in the values obtained. The lack of a suitable apparatus and of an iron-free house represented impediments to precise determination. The elimination was achieved with 4 equations, each having 3 unknowns. The needles used were cylindrical in form and the moments of inertia were calculated and not determined experimentally.

They also made observations with two needles of other dimensions and the influence of certain defects in the method, e.g. inaccuracy in measuring the distances *r* and the periods of oscillation, were admitted. The achievement of Gauß was therefore not only to establish a method on a sound theoretical foundation, but also to overcome the practical difficulties encountered by Moser and Rieß.

From Gauß' published correspondence in 1832 we know that he knew of the existence of Poisson's memoir and had read that of Moser and Rieß. Writing to Heinrich Christian Schumacher on March 3, he remarks:[10]

> Mit einer andren und wohl an sich nicht viel weniger wichtigen Seite des Gegenstandes habe ich mich in den letzten Wochen viel und wie mir deucht nicht ohne Erfolg beschäftigt, nämlich mit einem Mittel, die Intensität des Erdmagnetismus auf eine absolute Einheit zurückzuführen. Wenn ich nicht irre, hat Poisson zuerst ein Verfahren angegeben, und ich finde auch in Poggendorffs Annalen, einen Versuch, solches zur Anwendung zu bringen. Allein ich finde dabei verschiedenes, was ich durchaus für unzulässig halten muss, und halte mich überzeugt, dass durch solche Behandlung auch nicht einmal ein grob genähertes Resultat erhalten werden kann. Ich habe mehrere Reihen Versuche, aber unter andern Umständen, gemacht, deren schärfere Berechnung, wie ich schon jetzt erkenne, eine ziemliche Annäherung geben wird, deren Resultat aber weit von dem in Poggendorffs Annalen verschieden ist (etwa ½₀ so gross). Allein ich bin auf ein anderes Verfahren gekommen, welches ein viel reineres Resultat geben kann, und ich halte es für möglich, selbst die Genauigkeit des Resultats, wenn man alle nöthigen Vorkehrungen macht, so weit zu treiben, dass sie derjenigen, die durch vergleichende Beobachtungen mit Einer Nadel [erzielt wird], an die Seite gestellt werden kann, oder sie vielleicht noch überbietet. Schon jetzt geben die Versuche, die hauptsächlich Freund Weber nach meinen Angaben gemacht hat, eine Genauigkeit, worin wohl schwerlich mehr, als einige Procent Ungewissheit zurückbleiben; man wird es aber viel weiter treiben können...

In a letter to Christian Ludwig Gerling on June 20, 1832 Gauß once again refers to the paper of Moser and Rieß. The large difference between his and their results is attributed to the mode of calculation they employed. Gauß writes:[11]

> Meine Zurückführung der Intensität auf absolute Einheit, wozu ich schon mehrere, obwohl erst als vorläufig anzusehende Versuche gemacht habe, gelingen ganz unvergleichlich. Aber das von Moser und Rieser [sic] aus den Beobachtungen in Berlin berechnete Resultat ist nur ⅕ des meinigen, also ganz unbrauchbar (mein Resultat bestätigt sich auch durch Versuche an Nadeln von den verschiedensten Dimensionen, obwohl kleine Nadeln wenig Genauigkeit geben können). Jener enorme Fehler hat übrigens seinen Grund hauptsächlich in einer ganz unzulässigen Berechnungsweise: nach richtigen Principien finden sich, so gut es geht, Resultate, die wenigstens Annäherungen sind und sogar mein Resultat zwischen sich haben...

In a letter to Heinrich W. M. Olbers on August 2 we read that Gauß was perfecting his instruments and method of observing the intensity of the force in absolute measure. He refers to trials carried out using the modus prior but declares his intention of restricting his results to those obtained using the second mode. He writes:[12]

> Inzwischen habe ich die Absicht doch gleich eine Anwendung, und zwar die allerwichtigste, in einer Societätsvorlesung bekannt zu machen, nämlich die Bestimmung der absoluten Intensität des Erdmagnetismus. Ich habe schon, so wie meine Apparate sich nach und nach vervollkommneten, eine beträchtliche Anzahl vorläufiger Versuche gemacht, und die letzten werden der Wahrheit, soweit es in meinem Local möglich ist, schon sehr nahe kommen; doch habe ich erst neulich wieder neue Vervollkommnungen hinzugesetzt, nämlich Vorkehrungen, um alle Distanzmessungen dabei mit mikroskopischer Schärfe auszuführen. Auch hierbei ist mir Freund Weber durch Mittheilung seiner Hülfsmittel äusserst hilfreich gewesen.
>
> Jene Vorlesung hoffe ich binnen einigen Monaten ausarbeiten zu können, und einen kleinen Anfang habe ich bereits damit gemacht,...
>
> Nichts desto weniger ist der modus prior... dem zweiten bei weitem nachzusetzen, und zwar deswegen, weil jener eine viel längere Zeit erfordert, wärend welcher die Veränderlichkeit des Erdmagnetismus sich auf das Entschiedenste bemerklich macht. Ich habe zwar auch mehrere Versuche nach dem modus prior gemacht (die nahezu dieselben Resultate geben), werde aber bei denen, die gelten sollen, mich nur auf den zweiten Modus beschränken...

Finally, from a letter to Schumacher written on August 6, 1835 we know that Gauß had not read the paper of Poisson at the time he wrote the *Intensitas vis*.[13] This extract has also been quoted by Clemens Schaefer.[14] Gauß writes:

> Allerdings habe ich meine Methode, die Intensität des Erdmagnetismus zu bestimmen, nicht von Poisson entlehnt, da ich dessen Aufsatz damals (Frühjahr 1832) noch gar nicht gelesen hatte...

It is evident, therefore, that Gauß became acquainted with Poisson's method through the paper of Moser and Rieß. In principle Poisson's method is the same as that devised by Gauß, in that the second part of the determination of the horizontal component involves observation of the oscillation of one of the needles under the joint influence of the other and of the horizontal component. Furthermore, both methods involve the elimination of coefficients. Gauß' method is however superior in several respects. Poisson's method is independent of the magnetic characteristics of the needles employed and is therefore an absolute rather than a relative method for finding the horizontal component. However, as both Dorn[15] and Schaefer[16] have observed, the idea of establishing a unit of magnetism (unit pole), by fixing f, the constant of proportionality in Coulomb's law, as unity did not occur to Poisson. A further drawback of Poisson's method observed by Schaefer is that the moments of inertia of two needles have to be found by calculation.

In Gauß' method only one moment of inertia has to be found and this is obtained experimentally. Gauß also developed a magnetometer and methods of observation which provided a degree of precision not attained using Poisson's method.

Schaefer has observed that Gauß overlooked an important element which leads to a change in the magnetic moment of the needle in consequence of the fact that the main magnet is essentially parallel to the magnetic meridian during the observation of its period of oscillation, whereas it is perpendicular to the meridian during the deflection observations.[17]

This factor was first taken into account by Weber in 1855 after G. Th. Fechner had been the first to take account of the inducing force of the earth on a steel needle oscillating about the meridian in an investigation published in *Poggendorff's Annalen* in 1842.[18]

Perhaps the most important criticism of Gauß' *Intensitas vis* came from the British Isles and in particular from the Astronomer Royal, George Biddell Airy, who questioned the validity of the elimination procedure followed by Gauß. This criticism appeared in unpublished communications to the Royal Society in 1841 and 1842 and has hitherto escaped the notice and comment of historians. It seems therefore appropriate to reproduce Airy's criticism in full.

Airy's Criticism of Gauß' Elimination Formula

When the British colonial observatories were established in 1839 they were equipped with instruments operating on Gaußian principles which were designed by Humphrey Lloyd of Dublin. The same was the case for the Antarctic expedition led by James Clerk Ross (1839–1843). All of the observers were instructed by Lloyd who prepared a set of written instructions based on Gauß' *Intensitas vis*. This then came under the scrutiny of Airy, who on Christmas Day, 1841, wrote to John F. W. Herschel, Chairman of the Physical Committee of the Royal Society, raising objections to the elimination formula of Gauß given by Lloyd in the Instructions. This and the correspondence with Lloyd which ensued was printed as a circular for private circulation.[19] Airy writes:

> I beg leave to address you, as Chairman of the Physical Committee of the Royal Society, the following remarks on one of the paragraphs in the Report containing instructions for the observers in the Antarctic expedition. On page 20 is given the formula by which the absolute intensity of a magnet is to be deduced from two observations of the deflection which it produces in another magnet, at two different distances. The formula is
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> $$\frac{r'^5 \tan u' - r^5 \tan u}{2\,(r'^2 - r^2)} = \frac{m}{x}.$$
>
> Now I beg to remark, that the formula, practically, is useless. It is impossible to avoid small errors in the observation of u'; and these errors are multiplied by so large a factor, that the results are liable to the most enormous discordances. On this point I speak from experience.
>
> I would call attention to the fact that Gauss' theory leads to the result that the tangent of deflection may be expressed by the formula
>
> $$\frac{a}{(\text{distance})^3} + \frac{b}{(\text{distance})^5},$$
>
> where a is the coefficient upon which absolute force depends, and where the term depending upon b is generally much smaller than that depending on a.

I would then lay down, that it is advantageous to begin with determination of the term b, and if it can be well determined, to use it for computation of the small term in any stray observation, in order to deduce a without elimination. For this purpose, distinct series of numerous observations of deflection, at many different distances, ought to be instituted, and the equations

$$\tan d_1 = \frac{a}{r_1^3} + \frac{b}{r_1^5}$$

$$\tan d_2 = \frac{a}{r_2^3} + \frac{b}{r_2^5}$$

$$\tan d_3 = \frac{a}{r_3^3} + \frac{b}{r_3^5}, \text{ etc.,}$$

ought to be formed numerically, and the values a and b determined by the method of minimum squares, or any other method of elimination applicable to numerous equations. Then in any other instance where there is no reason to suppose that the power of the magnet and the earth's horizontal force may have materially altered, the coefficient b may be considered as known; and a may be found by the equation

$$a = r_0^3 \tan d_0 - \frac{b}{r_0^2}.$$

But if from any change of geographical situation, or from any change of magnetism of the bar, the coefficients a and b are changed into A and B, it will be certain in the former case, and may be presumed in the latter, that a and b are changed in the same ratio, so that A = na and B = nb. Then in any observation under the new circumstances, we shall have the equation,

$$\tan D = \frac{A}{R^3} + \frac{B}{R^5}, \text{ or}$$

$$\tan D = \frac{na}{R^3} + \frac{nb}{R^5}, \text{ from which}$$

$$n = \frac{\tan D}{\frac{a}{R^3} + \frac{b}{R^5}}$$ where a and b have the values previously determined.

Then $$A = na = \frac{\tan D}{\frac{1}{R^3} + \frac{b}{aR^5}}$$ is the quantity required.

I should not have presumed to point out these things to the Physical Committee, if in the first place I had not felt the great importance of the determination and, in the next place, if I had not learnt, by sad experience the total inefficiency of the form of solution recommended by the Report.

Airy's letter was passed on to Lloyd, who replied to Herschel on January 11, 1842, expressing some doubts about Airy's proposed solution to the difficulty. He writes:[20]

I cannot quite agree with Mr. Airy as to the *amount* of the discordances in the results thus calculated from good observations. In the earliest observations of the absolute intensity made at the Cape by Mr. Wilmot, which I happen to have had very recently before me, the extreme difference of the deduced intensities (obtained at different periods) only amounted to .005 of the whole; at the same time it is quite true that the errors of observation are much magnified in the result, and it is most important to combine the observational data (if it can be done) according to a method which shall give these errors small influence.

I am too little conversant with problems of this class (as to the best methods of combining the results of observation) to feel any confidence in my own opinion; and I am ready to defer to Mr. Airy's judgement. I think it right, however, to mention, for your consideration, and for his, some doubts which occur to me respecting his solution in the present instance.

The formula in question is that given by Gauss in his memoir "Intensitas vis terrestris, etc.". The method of calculation which Mr. Airy proposes to substitute for it agrees with that given by Mr. Weber (see Scientific Memoires), so far as it relates to the *first* determination of the coefficients a and b. But here lies the difference. Mr. Weber proposes to make observations of deflection on each occasion at three separate distances, and to deduce these coefficients each time by the method of least squares; while Mr. Airy would employ this process only in the *first* determination, and use these values afterwards in calculating the results of observations made at *one* distance only.

Now my own doubts are these. The formulae of elimination employed in the first determination are of a similar form to that objected to; and if these results be thus vitiated, they would, in Mr. Airy's method, vitiate *all* the following.

In the second place – the great value of Gauss' method, it is obvious, is that of obtaining a result which is independent of the magnetism of the bar employed. But the quantity b varies with the bar's change of magnetism, as well as a, and it is necessary, therefore to take account of this variation.

In doing this, Mr. Airy considers that a and b may be assumed to *vary proportionally*. Now on this point I confess I have much doubt. The parts of a and b depending on the magnetism of the bar are the integrals

$$\int_{-l}^{+l} qx\, dx, \qquad \int_{-l}^{+l} dx^3 dx;$$

q being the quantity of free magnetism at any point of the bar, x its distance from the centre, and l half the bars length. Now according to Coulomb and Biot, q may be represented by a function of x of the form

$$q = A\,(\mu^{l-x} - \mu^{l+x}),\ A \text{ and } \mu \text{ being constants.}$$

Substituting this value and integrating, it will be seen that the two integrals do not vary proportionally, if μ varies with the magnetic condition of the bar. I mention these doubts as they occur to me: you and Mr. Airy will best judge whether they are of any, and if so, of what weight.

Lloyd's reference to Weber's "Scientific Memoires" is not precise. The exact reference may be to Weber's article "Ueber Erdmagnetismus und Magnetometer", in *Schumacher's Yearbook* for 1836, which is reproduced in a more mathematical form in the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre 1836.*[21] Here he indicates how the calculus of probabilities can be applied in deflection experiments, making observations at three separate distances of the magnet, in order to calculate the most probable value of M/T in the *Intensitas vis.*[22]

Lloyd's letter of January 11, 1842, was again referred to Airy, who duly replied to Herschel on January 18. Again this letter may be read without explanation.[23]

I received yesterday your letter ... inclosing Professor Lloyd's letter of 11th. I am much obliged by Professor Lloyd's attention to my suggestion, and will now give my further opinion on the points to which he adverts.

1st. As to the amount of discordances. My first magnetic assistant is not in the observatory at the moment (having been on night watch), and I cannot lay my hands on our first results. I have probably exaggerated in saying that the results of one day were double those of another, but the differences were so great that we perceived that to go on in the same manner was but child's play. I think that a consideration of the numbers which occur in the observation will show the same thing. Suppose (using round numbers) that one distance is double the other, and that the greater deviation is 1°; if the deviation is entirely due to the −3 power, the smaller deviation is then 7½′; if the deviation is not at all due to the −3 power, but wholly to the −5 power, then the deviation at the greatest distance is 1⅞′: consequently, the whole determination of the coefficient of the −3 power, on which the absolute measure entirely depends, rests on a measure of 5⅝′. Now Professor Lloyd knows better than I do, that the determination of this to .005 of the whole (or certainly to 1.5″) is a matter of the merest accident. I will go further, and will say that an error of half the ultimate determination is possible.

> 2nd. As to the objection to my proposal for determining b as an independent step. I beg that it may be fully understood that I do not propose this as perfect. I propose it as the better of two bad methods and I have no doubt that it is *fifty times* more accurate than the other (to which I have objected above). As to the failure of the formula of elimination, this may be diminished exceedingly by making trials enough. We are at present trying with a certain magnet placed at every six inches distance from 5 feet to 11 feet (13 distances). In proceeding thus, and afterwards substituting the determined constants in the formula, we have a most severe check on any suspicious observation in the *series*; and in comparing the value of b obtained one day with that obtained on another day, we have another more severe check on the operation in general. As to the variation of b, I fear little about it. I have great confidence of a good magnet of hard steel, and I should without scruple assume that general variation arose from terrestrial variation, and therefore that the special parts varied in the same proportion.
>
> But – fearing that I may have expressed myself ambiguously – I wish expressly to say, that I should only rely on this when there is nothing better to be done. I think that distinct series of observations ought to be made to determine the relative values of a and b from time to time, say twice a year, or oftener if found necessary (just like determining an error of collimation of a transit instrument); but for intermediate observations I should observe a deflection *at one distance only* (namely, the smallest distance), and should reduce it with the proportion of a to b found from occasional series.
>
> You will, however, understand that I have thought it desirable to bring the subject before you, not only because, I do believe the method prescribed for this important determination to be inaccurate, but also because, as now expressed in the instructions, it conveys no trace of the theory upon which it is founded, and therefore puts it out of the power of an observer not previously familiar with the *vis absoluta*, to vary the mode of observing. I certainly would not omit what is given, but I would add a good deal of what is not given.

The discussion between Airy and Lloyd about Gauß' elimination formula was regarded by their intermediary, Herschel, as particularly valuable and instructive; writing to Edward Sabine, he recommended that such communications ought to be printed and circulated among the members of the Royal Society's committee.[24] He writes:

> Do you not think that the written communications, such as this note of Airy's enclosed, or, at least, such as the correspondence of Airy and Lloyd, ought to pass into the hands of *every* member of the Physical Committee? Would the Council authorise us, for example, to print a sufficient number of any such occasional communications, *as circulars addressed to its members*, and thus put every member on a par as to his means of information of what is going on?

Herschel's suggestion was approved and the letters appeared as a circular for private circulation. In a letter to Sir John Lubbock, written on May 4, 1842, and printed in the *Proceedings of the Committee of Physics* for May 12, Lloyd recommended a means of proceeding in deflection experiments so as to minimise errors arising in the observed deflections.

Lloyd writes:[25]

It will be remembered that the ratio of the horizontal component of the earth's magnetic force, to the moment of free magnetism of the bar, is determined by using that bar to *deflect* a second, freely suspended; and that, for getting rid of a term foreign to the result, it is usual to place the deflecting bar at two separate distances, and to infer the ratio in question by elimination between the two equations of condition thus obtained. The difficulty of this method being due to the large influence which errors of the observed deflections have upon the results, its success will manifestly depend on our assuming the distance in such a manner, that the resulting error, corresponding to a given error of deflection, should be the *smallest possible*. The proportion of the distances which satisfy the condition, is a simple result of the calculus of probabilities; and I am more desirous of drawing attention to it now, as it has erroneously been given in the "Instructions" (fortunately, in the personal instructions which I had the pleasure of giving to the officers, I recommended a rule, which gives very nearly the true proportion).

The function sought (the quotient of m and x, the magnetic moment and the horizontal component of the force) is expressed by the formula

$$V = \frac{r'^5 \tan u' - r^5 \tan u}{2(r'^2 - r^2)},$$

in which r and r' are the two distances, and u and u' the corresponding angles of deflection. These angles being always small we may take $\tan u = u \tan I'$, $\tan u' = u' \tan I'$; the angles u and u' being expressed in minutes; and making $r' = qr$, the formula becomes,

$$V = \frac{1}{2} r^3 \tan I' \cdot \frac{q^5 u' - u}{q^2 - 1}.$$

Now, by a well known theorem of the calculus of probabilities, the probable error of V will be expressed in terms of the probable errors of u and u', by the formula

$$(\Delta V)^2 = \left(\frac{\frac{1}{2} r^3 \tan I'}{q^2 - 1} \right)^2 \{q^{10}(\Delta u')^2 + (\Delta u)^2\}$$

or, since the probable error of u and u' are the same,

$$\frac{\Delta V}{\Delta u} = \frac{1}{2} r^3 \tan I' \frac{\sqrt{q^{10} + 1}}{q^2 - 1}$$

Now by the conditions of the question, the ratio must be a *minimum*. Wherefore, making

$$Q = \frac{\sqrt{q^{10} + 1}}{q^2 - 1}, \text{ we must have } \frac{dQ}{dq} = 0.$$

And this gives the following equation for the determination of q:

$$3 q^{10} - 5 q^8 - 2 = 0.$$

In order to solve this equation, we may observe that, q being greater than unity, the last term of the equation may, in a first approximation, be neglected in comparison with the others, so that we have approximately,

$$3 q^2 - 5 = 0, \quad q = \sqrt{\frac{5}{3}} = 1.3, \text{ nearly.}$$

And setting out from this value, we find, by Newton's method of approximating to the roots of equations, q = 1.32 or 1⅓ nearly.

Gauß has shown that the shortest deflection distance is *four times the length of the magnet*, the third term of the series by which the tangent of deflection is expressed becoming sensible within the distance. If this distance be called a, the greater distance should be consequently 1.32 a. I would recommend, however, that the deflection should be made at *three* separate distances in the same proportion, viz. a, 1.32 a, 1.74 a; and thus two values of the quantity sought deduced from the 1st and 2nd, and from the 2nd and 3rd respectively.

Thus in Lloyd's revised "Instructions" to the directors of the British observatories, the Gaußian elimination formula is retained but the deflection distances are so chosen as to minimise the errors in the observed horizontal intensity resulting from errors in the observed deflections.

Contributions of Lloyd and Lamont

Humphrey Lloyd and Johann von Lamont both played an important role in the development of geomagnetic instruments according to Gaußian principles in the period from 1835 to 1850.[26] Whereas Lloyd developed his instruments on the model of the Göttingen instruments devised by Gauß and Weber, modifying the latter only in details of construction, Lamont introduced radical departures in the design of his instruments. Both men also turned their attention to the method for finding the intensity of the force in absolute measure, and both produced publications on the topic.

Lamont's paper (1842), entitled "Bestimmung der Horizontal-Intensität des Erdmagnetismus nach absolutem Maasse", was published in the *Abhandlungen* of the Bavarian *Akademie der Wissenschaften* and as a separate publication.[27] In the first section of his paper, Lamont acknowledges Poisson's contribution without making exact reference to the publication. The "modus prior" is not treated at all and neither Moser and Rieß nor Gauß are referred to. Lamont remarks at the end of the first section:

> Poisson hat selbst seine Methode nicht praktisch ausgeführt, und die Ergebnisse, welche später bei absoluten Intensitätsmessungen erlangt worden sind, liefern den Beweis, dass es keine leichte Aufgabe sey, die Erfahrungsdata, deren man dabei bedarf, mit der nöthigen Schärfe zu gewinnen. Es ist meine Absicht, in der gegenwärtigen Abhandlung, einen Weg anzugeben, auf welchem man mit grösseren Genauigkeit und Leichtigkeit, als bisher geschehen ist, die eben erwähnten Erfahrungsdata praktisch erlangen könne, und zwar werde ich das Produkt ... μX ... durch *Schwingungs-Beobachtungen*, den Quotient dagegen .. μ/X .. durch *Ablenkungs-Beobachtungen* in eigenthümlicher Weise eingerichtet, darstellen.

Here μX and μ/X are the product and quotient respectively of the pole strength of the magnet and of the horizontal force. The magnetic moment M is introduced further on in the paper.

That Lamont knew of Gauß' method is beyond the shadow of a doubt but the reasons for his extreme opposition to Gauß is difficult to understand. The following extract from a long letter to Airy, written on September 12, 1842, describing the Munich observatory, reveals something of Lamont's position.[28] He writes:

> I cannot conclude this letter without remarking that I greatly differ from you with regard to magnetic instruments. You seem to consider the instruments generally used at present, merely as *inconvenient.* On comparing two of Gauß' bars I found them to differ very considerably in the indications. I afterwards discovered the cause and showed the means of preventing it. I showed by experiment that by applying my principles, two instruments of the same kind agreed perfectly and that absolute measures taken at different times of the day agreed with the variations. That Gauß' bars do not agree has been confirmed by Kupffer, and Mr. Lloyd informs me that a series of observations have been made at St.

> Helena proving (when rightly interpreted) the same thing. I therefore consider observations that have hitherto been made subject to periodical errors amounting sometimes to about a few minutes. The same was the case with the bifilar which can never be made to give accurate indications. As for the method of absolute intensity it is not only very inaccurate in practice but defective also in theory, the higher terms in the expansion for m/x and the influence of the induced magnetism of the bar being neglected. It is therefore my opinion that what has been done hitherto is in great part to be considered as lost labour and that in order to obtain the data required by theory a total reform in the arrangement of magnetic observations is absolutely necessary. This will occasion some trouble; the expenses will be inconsiderable.

The references in the above letter are to Adolph Theodor Kupffer, director of the Russian observatories and who co-operated with both the *Göttinger Magnetischer Verein* and with the British colonial observatories, and to the instrument designed by Gauß for monitoring the changes in the horizontal component known as the bifilar magnetometer. The latter instrument was also adopted in the British observatories. Schaefer, commenting on the Gauß-Lamont relationship, has observed that the small bars employed by Lamont were later to find general acceptance.[29]

> Es ist dies einer der wenigen Punkte, wo wir heute – nach dem Vorgange von Lamont – grundsätzlich von Gauß abweichen.

Gauß and Weber were well aware of Lamont's outright opposition to them and there are a number of comments in Weber's correspondence with his friend Steinheil. In a letter, written on June 28, 1842, we find one of the most poignant remarks.[30]

> Lamont hat seine Beschreibung des neu errichteten magnetischen Observatoriums zwar nicht mir oder Gauß, aber der hiesigen Sternwarte geschickt, worin sich durchgängig eine große Abneigung gegen Alles, was von Gauß herrührt, auszusprechen scheint...

In his 1842 paper on the determination of the horizontal intensity in absolute measure, Lamont developed the theory of the method he followed. In all a set of six equations is derived. From the theory of the oscillation of the principal magnet he obtains the equation:

$$MX = \frac{K\pi^2}{T^2} \qquad \text{(IV)},$$

where K is the moment of intertia of the bar, M is its magnetic moment and T is the period of the oscillation.[31] Using the first magnet to deflect a second suspended in its place, the following equation is found:

$$\frac{M}{X} = \frac{1}{2} e^3 \sin\varphi \frac{1}{1 + \frac{p}{e^2} + \frac{q}{e^4} + \ldots} \qquad \text{(V)}.$$

In this equation e is the distance from the centre of the deflecting magnet to that of the suspended magnet, the former being placed as in Gauß' technique in a line at right angles to the meridian and passing through the point of suspension; φ is the angle of deflection from the meridian. Eliminating M in equations (IV) and (V) the sixth equation is found.[32]

$$X^2 = \frac{2\pi^2 K}{T^2 e^3 \sin\varphi} \left(1 + \frac{p}{e^2} + \frac{q}{e^4} + \ldots\right) \qquad \text{(VI)}.$$

Whereas Lamont's equations are in a different form to those of Gauß, the method is in fact identical. Lamont's paper does contain some original features such as the method given for determining the moment of inertia of the small bars employed. To derive the values of the coefficients p and q the deflection is observed at various distances and the method of least squares is applied.[33]

Um den Werth von M/X zu erhalten, reicht es hin, die Ablenkung φ für eine einzige Distanz e zu kennen, vorausgesetzt, dass man den Werth von $1 + p/e^2 + q/e^4$ bestimmt habe. Die Bestimmung dieser Grösse erfordert aber Ablenkungen in verschiedenen Entfernungen, wobei für jede Entfernung eine Gleichung von der Form (V.) erhalten wird. Sind nämlich die Distanzen e, e', e″ ... und die entsprechenden Ablenkungen φ, φ', φ'' ...... so hat man, wenn

$$\log\left(1 + \frac{p}{e^2} + \frac{q}{e^4}\right) = -\frac{p'}{e^2} - \frac{q'}{e^4} \ldots.$$

gesetzt wird

$$\log \frac{M}{X} = \log\left(\frac{1}{2} e^3 \sin \varphi\right) + \frac{p'}{e^2} + \frac{q'}{e^4}$$

$$\log \frac{M}{X} = \log\left(\frac{1}{2} e'^3 \sin \varphi'\right) + \frac{p'}{e'^2} + \frac{q'}{e'^4}$$

$$\log \frac{M}{X} = \log\left(\frac{1}{2} e''^3 \sin \varphi''\right) + \frac{p'}{e''^2} + \frac{q'}{e''^4}$$

. . . .

woraus log M/X eliminirt und durch die Methode der kleinsten Quadrate p' und q', mithin auch p und q abgeleitet werden können.

Lamont's method resembles Airy's proposed method therefore. From the observation of the deflections at a series of distances, the coefficients p and q are to be established. Using these values it is then sufficient to observe the deflection at a single distance and an elimination formula is not required.

Lloyd's paper "On the determination of the intensity of the earth's magnetic force in absolute measure" appeared almost simultaneously with that of Lamont. It was read on January 9, 1843, at the Royal Irish Academy and published in the *Proceedings* and *Transactions* of the Academy.[34] This paper of Lloyd was most probably inspired by his correspondence with Airy in early 1842 about the elimination formula in Gauß' *Intensitas vis*. A method was now proposed which would allow the elimination procedure to be obviated by observing the deflection at a single distance. The idea is explained as follows. In the theory of the deflection one obtains

$$\tan u = \frac{Q}{D^3} + \frac{Q'}{D^5},$$

where u denotes the angle of deflection, D the distance and Q and Q' are unknown coefficients. In Gauß' method the angles of deflection (u and u') are observed at two distances (D and D'), and the coefficient Q is then obtained by elimination between the two resulting equations of condition. Lloyd calculated the amount of probable error using Gauß' method by applying the calculus of probabilities. He found that if the term containing the negative fifth power could be supposed to vanish in the expression for the deflecting force, then the probable error would be reduced in the ratio of 1: 5.563 and the accuracy of the result increased five fold.

The same advantage he found could be gained while retaining the coefficient of the inverse fifth power, provided the ratio of the two coefficients $Q'/Q = h$, be known *a priori.* In this case the above expression could be written

$$\tan u = \frac{Q}{D^3}\left(1 + \frac{h}{D^2}\right), \text{ and } Q = \frac{D^3 \tan u}{1 + hD^{-2}}.$$

Lloyd's idea is therefore essentially the same as Lamont's. By deriving his coefficients p and q, Lamont finds a value for $1 + p/e^2 + q/e^4$, which allows determination of M/X and X by observing at a single distance. Lloyd achieves the same result by finding an *a priori* relationship between the Gaußian coefficients. Lloyd added the following acknowledgement to his published paper.[35]

> When the preceding pages were passing through the press, I received a memoir from Professor Lamont, on the determination of the earth's magnetic force in absolute measure, in which the author has proposed various modifications in the existing method, and has considered with great minuteness of detail, the many corrections which are required in the immediate results of observation. Some of the conclusions of the present paper, I find, thus anticipated, in particular, the form of the equation of equilibrium of the suspended magnet, for the case in which the axes of the two magnets are at right angles. Professor Lamont seems to have considered however, that no approximation to the law of magnetic distribution was possible; and he has accordingly not thought of deducing *a priori* the ratio of the coefficients of the terms in the above mentioned equation (except in the imaginary case in which the whole force is supposed to emanate from the two ends of the bars), or therefore, of employing that ratio, as is proposed in the present paper, to supersede experiment, and thus evade the errors resulting from the process of elimination.
>
> I take this opportunity of stating, that the present paper was, in substance written during last summer; and that several instruments have been constructed on the principle suggested in it. The delay in laying it before the Academy has arisen from the desire of obtaining, previously, experimental confirmation of the accuracy.

Summary and Conclusions

Gauß' method for determining the horizontal component, presented in the *Intensitas vis,* and its subsequent adoption in geomagnetic research was of fundamental importance in the history of the subject. The method had an antecedent in that proposed by Poisson in 1825 and tried by Moser and Rieß in about 1830. Both methods involved the use of two needles or bars; the second part of the procedure in each method involves observation of one of the needles under the joint influence of the second and of the horizontal component. Both methods involve elimination of unwanted coefficients in the final determination. Gauß' method is, however, superior to that of Poisson in several respects. Indeed Gauß appears to have perfected his own method after examining the shortcomings of that published by Moser and Rieß. Gauß set out from a definition of unit pole, setting the constant of proportionality in Coulomb's Law equal to unity; he also verified the inverse square coefficient in elaborating his method. Gauß' elimination procedure is far simpler than that of the method of Moser and Rieß, involving only two unknown coefficients whereas the latter method involved three or more. Lastly, the experimental techniques and instruments devised by Gauß and Weber were far superior to anything previously available; in particular the experimental techniques for finding the moments of inertia of bars, the coefficients of torsion, etc., provided a degree of experimental accuracy previously only attained in astronomy.

Gauß admitted an inherent defect of his method in the introduction of the *Intensitas vis*; he saw the solution of this difficulty, arising from the elimination of coefficients in deflection observations, in the development of the means for the exact measurement of small deflections. He applied the method of least squares to determine the unknown coefficients from a series of observations and made a comparison of calculated and measured values of the angle of deflection using these coefficients. However he did so only with a view to proving the inverse square coefficient in Coulomb's Law and to finding the number of terms in the expansion which have to be taken into consideration. Otherwise, according to Gauß' method, one of the coefficients is to be eliminated and the other substituted in the expression found for M/T, the quotient of the magnetic moment and horizontal intensity. Weber subsequently indicated how the calculus of probabilities is to be applied, taking observations of deflections at three distances, in order to find the most probable value of M/T.

After Gauß' method had been adopted at the British colonial observatories and in the Antarctic expedition in 1839, the elimination formula was criticized by the Astronomer Royal, Airy. He pointed out that errors in the observed deflections are greatly magnified in the end result. Instead of the Gaußian procedure, Airy proposed to establish the unknown coefficients from a preliminary series of observations and to use these values subsequently, making observations at a single distance only.

In response to Airy's objections, Lloyd tried to modify Gauß' method while retaining the elimination formula; he calculated the two distances at which the deflections should be observed in order that the error in the result be minimised.

Lamont, in a paper of 1842, describes a method for finding the horizontal component in absolute measure, which is similar to that of Gauß but he used very much smaller bars. In Lamont's method the coefficients are established in advance by making deflection observations at a series of distances. Subsequently, it is sufficient to observe the deflection at a single distance in order to establish the horizontal component. Likewise Lloyd, in a paper of 1843, tried to establish an *a priori* relationship between the coefficients which would also make observation at a single distance possible.

The fundamental importance of Gauß' method has long been recognised by physicists and historians. It marked the beginning of a development which made the precise determination of the so-called "magnetic elements" (declination, inclination and intensity) possible all around the globe. As the method was improved and became a standard textbook method, its early history was largely forgotten. Historians have for the most part dismissed the corrections involved in retaining the coefficients in deflection observations as of little importance.[36] Perhaps the evidence presented here of the discussion about the adoption of the elimination formula may lead to a reappraisal of the early history of the method. It may also throw a little further light on the role of Gauß as physicist.

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