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Major P. A. MACMAHON, D.Sc., Sc.D., LL.D., F.R.S., PRESIDENT, in the Chair.

Ronald Malcolm, Headley Grove, Headley, Epsom, was balloted for and duly elected a Fellow of the Society.

Sixty-eight presents were announced as having been received since the last Ordinary Meeting, including, amongst others:—

Specola Vaticana, Catalogo Astrografico, vol. 3, Zone + 62°, presented by the Observatory.

T. J. J. See, Electrodynamic Wave-Theory of Physical Forces: Discovery of the Cause of Magnetism, Electrodynamic Action, and Universal Gravitation.

On the Origin of Ptolemy's Catalogue of Stars. Second Paper.

By J. L. E. Dreyer, Ph.D.

In a paper on this subject communicated to the Society last year (*Monthly Notices*, 77, 528) I drew attention to the fact that the methods adopted by Ptolemy for determining the places of standard stars of necessity introduced large systematic errors in the results. Among the sources of error mentioned were refraction, imperfect knowledge of the equation of the centre of the solar orbit, and instrumental errors.

That refraction must have influenced Ptolemy's results very considerably was already noticed by Tycho Brahe.* But I find that I underestimated the error produced by neglecting it. In the only example given by Ptolemy, the difference of longitude of sun and moon was measured on A.D. 139 February 23, "when the sun was just setting," at $5\frac{1}{2}$ equinoctial hours after Noon, while the last degree of Taurus was on the meridian. On that day the equation of time was $+15^{\mathrm{m}}\cdot 7$, so that the observation was made at $5^{\mathrm{h}}\ 45^{\mathrm{m}}\cdot 7$ M.T. Alexandria, the sun's hour angle being $82°\ 30'$. But a point of the ecliptic in $60°\ 0'$ longitude or Right Ascension $57°\ 46'$ was on the meridian. As the Sidereal Time at Mean Noon was $331°\ 31'$, this gives the time of observation as $5^{\mathrm{h}}\ 44^{\mathrm{m}}$ M.T. and the sun's hour angle $82°\ 4'\cdot 5$. Of these two indications of time the former is probably by far the more reliable; we shall assume the hour angle $=82°\ 25'$. The sun's longitude was $333°\ 40'$ and the declination $-10°\ 16'$.† The zenith distance was therefore $88°\ 55'$ and the parallactic angle $q=58°\ 0'$. The refraction in declination was $=23'\cdot 8 \cos q$, and in longitude

$$d\lambda = \operatorname{cosec} \epsilon \sec \lambda \cos \delta \, d\delta = 34'\cdot 5.$$

By this amount, therefore, the difference of longitude of the sun and the moon, and consequently the longitude of the star, was diminished by refraction. In higher declinations of the sun, either north or south, the error would be considerably greater; in declination 20° it would be 60′. But if the observations had been taken at sunrise, the error would have had the opposite sign, as the diminished difference of longitude of sun and moon would have increased the longitude of the star. If Ptolemy divided his observations fairly equally between sunrise and sunset, he would therefore eliminate the effect of refraction.

Similarly the error in the adopted excentricity of the sun's orbit, and consequently in the equation of the centre, may have been avoided or much diminished by distributing the observations of standard stars over the year. We know nothing whatever as to how many standard stars Ptolemy connected with the sun or how many observations of this kind he took.‡ But though errors of this kind or instrumental errors must have diminished the general accuracy of Ptolemy's star-places, the constant error of fully a degree in the longitudes cannot be accounted for in this way. The error in the equinox assumed by him can therefore only be due to an error in the mean longitude of the sun.

As mentioned in my first paper, Ptolemy adopted the elements of the solar orbit found by Hipparchus without any alteration. Whenever he quotes one of his own observations of the planets, *e.g.* of an opposition of one of the outer planets, he gives the

* *Progymnasmata*, p. 145; *Opera Omnia*, T. ii. p. 155.

† Computed from Neugebauer's Tables.

‡ He only mentions (vii. 2, p. 16, Heiberg) that he observed Spica and the brightest stars near the Ecliptic in this manner.

mean longitude of the sun at that moment.* Comparing ten of these with Neugebauer's Tables, the mean difference is $+59' \pm 0'.9$, practically the same as the constant error of Ptolemy's star-places. According to Hipparchus, the tropical year is $365\frac{1}{4} - \frac{1}{300}$ days $= 365^{d}.24666$, which is $0^{d}.00435$ too long. The mean motion of the sun in an Egyptian year of 365 days is therefore $359° \, 45' \, 24''.8$ or $15''.7$ too small. Ptolemy's tables start from the era of Nabonassar -746 February 26, Alexandria Noon, or February 25, 22^{h} G.M.T., for which moment he gives the Mean Longitude $= 0° \, 45'$ ♓. According to Neugebauer's Tables it was $27° \, 56'$ ♒ or $2° \, 49'$ less. The epoch of the star catalogue was 885 of Nabonassar, so that the correction to Ptolemy's mean longitude of the sun was $-2° \, 49' + 15''.7 \times 885 = +1° \, 2'$.

This, then, is the error of Ptolemy's equinox, and as it is (within a minute or two) equal to the average error of Ptolemy's longitudes of stars, it is impossible to doubt that he really founded his catalogue on new observations of stars and the sun, taking the places of the sun from the solar tables in the third book of his *Syntaxis*.

Ptolemy's veneration for his predecessor was so great, that he accepted the solar elements of Hipparchus when a few observations made by himself seemed to agree with them. He gives only the following three determinations of equinoxes made by himself:— †

Autumn Equinox	A.D.	132,	September	25, 2 p.m.,	Alex.	M.T.
Autumn Equinox	,,	139,	,,	26, 7 a.m.,	,,	,,
Spring Equinox	,,	140,	March	22, 1 p.m.,	,,	,,

Computing by means of Schram's *Kalendariographische und Chronologische Tafeln* the time of entry of the sun into the signs of Libra and Aries in those years, the moments indicated by Ptolemy are found to be respectively

h	m	
32	0	too late.
31	7	,,
18	20	,,

The mean is $27^{h} \, 9^{m}$, which corresponds to an error in the mean longitude of $1° \, 7'$. Very probably Ptolemy selected these three determinations because they agreed with the sun's motion according to Hipparchus. But it should also be remembered that such determinations of the time of equinox must have been very difficult to make. Ptolemy estimated that they may be as much as six hours in error, but the uncertainty was probably much

* It should be remembered that a planet was said to be in opposition when it was 180° from the *mean* place of the sun (simplex solis). Kepler was the first to substitute for this the true place of the sun.

† *Syntaxis*, iii. 7 and iii. 1 (ed. Heiberg, p. 256 and pp. 204–205).

greater. The observations were made by means of a large ring suspended in the plane of the equator, and the declination of the sun was supposed to be zero when the shadow of the front part of the ring fell exactly on the back part. Delambre has shown that an error of half a day or more might be caused by refraction, while the error of 13′ or 14′ in the assumed latitude of Alexandria might cause an error of about 14 hours.*

The error of Ptolemy's mean longitude of the sun did more mischief than making his stellar longitudes too small. It spoiled his determination of the constant of precession. In my paper of last year (p. 538), when speaking of the star-places found by Ptolemy for the years 295–283 B.C. and A.D. 92–98, I wrote: "By a curious piece of ill luck the longitudes for the time of Timocharis were all too great and those for the end of the first century too small, which produced the faulty precession of 1° in 100 years." But we see now that this was a necessary consequence of the error of the sun's longitude.

Year.	Mean Error of resulting Star-longitude.	Error of Sun's Mean Longitude.
−288	−38′ (4 obs.)	−50′
+ 96	+44′ (3 obs.)	+50′

The error in the assumed length of the tropical year is therefore responsible for the whole of the error of Ptolemy's value of Precession.

All this was clearly perceived by Laplace. In chapter ii. of the fifth book of his *Exposition du Système du Monde* † he shows that as the assumed annual motion of the sun with regard to the equinoxes was too small, the amount of precession must be increased by the arc described by the sun in an interval of time equal to the error in the length of the year. He then continues: "Ces remarques nous conduisent à examiner si, comme on le pense généralement, le catalogue de Ptolémée est celui d'Hipparque, réduit à son temps, au moyen d'une précession d'un degré dans quatre-vingt-dix ans.‡ On se fonde sur ce que l'erreur constante des longitudes des étoiles de ce catalogue, disparaît quand on le rapporte au temps d'Hipparque; mais l'explication que nous venons de donner de cette erreur, justifie Ptolémée du reproche de s'être approprié l'ouvrage d'Hipparque; et il paraît juste de l'en croire, lorsqu'il dit positivement qu'il a observé les étoiles de ce catalogue, celles même de sixième grandeur."

Ideler § also showed how much Ptolemy's longitude of the sun was in error, and gave a table of the amount of this error. He mentions (without going into details) that Ptolemy's longitudes of stars had to be founded on his places of the sun, and that this

* *Astronomie ancienne*, ii. p. 101.

† 4th edition (1813), p. 383.

‡ That is, a degree equal to one-hundredth part of the quadrant.

§ *Historische Untersuchungen über die astronomischen Beobachtungen der Alten* (1806), p. 299. French translation by Halma (1819), p. 115.

produced the erroneous value of precession; but he does not allude to the star catalogue.

Delambre says that one might explain everything in a manner less favourable but more simple by saying that Ptolemy observed neither stars nor equinoxes but took everything from Hipparchus, starting from the minimum value of precession found by the latter.* He nowhere affirms distinctly that this is the only explanation possible; but, on the other hand, he makes the strange statement, that the error in the mean motion "n'aurait aucun inconvénient pour l'époque où il a vécu." †

In addition to the observations of stars Ptolemy records in the course of his great work more than thirty observations of the moon and the planets, and nobody seems ever to have doubted that these were really made by himself. When describing the different effect of precession in different parts of the heavens, he gives a list of the declinations of eighteen stars according to the observations of Timocharis, Hipparchus, and himself; and he remarks that they agree with the conclusion already drawn, that the stars advance in longitude at the rate of 1° in 100 years.‡ Delambre has calculated the value of the constant of precession resulting from these declinations, and finds §

Timocharis–Hipparchus . . .	$51''.4$
Hipparchus–Ptolemy . . .	47.5
Timocharis–Ptolemy . . .	47.4

Delambre has given equal weight to all the single results, which is absurd, as some of the stars were very close to the solstitial colure (Altair, Sirius, Castor, Pollux) and the results derived from them of no value. It is very curious, that it does not strike him, that his investigation furnishes an excellent proof of the *bona fides* of Ptolemy, since the observations do *not* agree with Ptolemy's preconceived notion of $36''$ a year. As some of the data used by Delambre differ from the figures in Heiberg's edition, I have repeated the calculation. I first determined the declinations by starting from those of Hipparchus, using Ptolemy's longitudes minus $1° 20'$ (λ) and his latitudes unaltered (β), with his value of the obliquity $\epsilon = 23° 51'$, and his precession of $36''$. We should then have

$$\Delta\delta = 160' \sin\epsilon \cos\beta \cos\lambda \sec\delta.$$

Or, we may, like Delambre, from this formula find what annual change of longitude corresponds to the change of declination from Hipparchus to Ptolemy.

* *Astronomie ancienne*, ii. p. 250; see also i. p. 183.
† *Ibid.*, ii. p. 138.
‡ *Syntaxis*, vii. 3 (Heiberg, p. 19 *sq.*).
§ *Astronomie ancienne*, ii. p. 254.

	$\Delta\delta$.	Declination. Computed with 36″.	Ptolemy.	Calc.-Obs.	Precession from $d\delta$ Observed.
	′	° ′	° ′	′	″
α Aquilæ	+ 2·5	+ 5 50	+ 5 50	0	29·2
Alcyone	+57·2	+16 7	+16 15	− 8	41·2
Aldebaran	+49·2	+10 34	+11 0	−26	55·3
Capella	+46·9	+41 11	+41 10	+ 1	35·6
γ Orionis	+37·4	+ 2 25	+ 2 30	− 5	40·6
α Orionis	+30·4	+ 4 50	+ 5 15	−25	65·5
Sirius	+12·3	−15 48	−15 45	− 3	44·1
Castor	+10·6	+33 21	+33 24	− 3	47·7
Pollux	+ 6·1	+30 6	+30 10	− 4	59·9
Regulus	−35·7	+20 4	+19 50	+14	50·7
Spica	−64·5	− 0 29	− 0 30	+ 1	37·1
η Ursæ	−64·8	+59 40	+59 40	0	36·4
ζ Ursæ	−65·0	+65 25	+65 0	+25	50·2
ε Ursæ	−64·5	+66 31	+66 15	+16	45·5
Arcturus	−63·9	+29 56	+29 50	+ 6	39·7
α Libræ	−62·4	− 6 38	− 7 10	+32	55·8
β Libræ	−59·8	− 0 36	− 1 0	+24	50·9
Antares	−51·5	−19 52	−20 15	+23	52·7

Most of the results in the last column differ very little from Delambre's. Omitting the four stars mentioned above, the mean is 46″·9. The second last column also shows that the declinations given by Ptolemy agree very badly with his precessional constant of 36″. They must therefore have been observed, and cannot be those of Hipparchus reduced to A.D. 137. I may add that Ptolemy's knowledge of spherical trigonometry was quite sufficient to enable him to do that, if he had wished.*

If Ptolemy had possessed one of our modern transit-circles, and had allowed for refraction, etc., but had taken the places of the sun from his own tables, his star-places might have represented the relative positions of the stars as accurately as one of the modern catalogues does. But they would certainly all the same have been affected with the large systematic error which we find in his Catalogue. And when in the nineteenth century a faulty determination of the equinox of 1865 produced a value of the constant of precession (that of Nyrén) which differed in an extraordinary manner from those found by other astronomers, it was only a repetition, on a different scale, of what had happened in the second century.

The assertion that Ptolemy's Catalogue is only a reproduction of the catalogue of Hipparchus was (I believe) first made by Tycho

* *Cf.* Braunmühl, *Geschichte der Trigonometrie*, i. p. 23 *sq.*, and Zeuthen's paper, "Note sur la trigonometrie de l'antiquité," in *Bibliotheca Mathematica* I_3 (1900), pp. 20–27.

Brahe.* Repeated by Flamsteed in the introduction to his *Historia Cœlestis,* and further circulated by Delambre, it has been accepted more or less as a self-evident proposition, while the explanation given by Laplace and Ideler has been overlooked. This would hardly have happened if all but one of the writings of Hipparchus had not been lost. The only book that is left, the *Commentary to Aratus,* was written before the discovery of precession was made. The remarkable catalogue of the Right Ascensions of 44 time-stars contained in that book, and the Declinations quoted by Ptolemy, show that he observed these co-ordinates directly, at least for some stars; and it is quite possible, as suggested by Delambre,† that the Catalogue of Hipparchus originally gave the Right Ascension and Declination of Stars, from which the Longitudes and Latitudes were derived by calculation after the discovery of precession. On the other hand, it seems from what Ptolemy says of the observations of the moon by Hipparchus, ‡ that these were made with an instrument which gave longitude and latitude directly, and he may of course also have observed fixed stars with it.

The Atomic Weights of the Elements in Nebulæ. By Professor J. W. Nicholson, F.R.S.

The interpretations, which have been put forward by the writer in various papers in the *Monthly Notices,* of the nebular and coronal spectra in terms of the dynamical vibrations of certain precisely defined atoms about stationary states of steady motion, have hitherto been deficient in one sense at least. A completely defined atom should be capable of an investigation which gives the relative wave-lengths of lines with any desired order of precision, although, on the electromagnetic basis, such an investigation is so complex that the problem, as treated in these papers, was simplified so that the forces acting on the electrons of the atom, during their vibrations, were regarded as purely of the electrostatic type. It remains to be shown that, in the more complete investigation, in which the necessary magnetic and associated mechanical forces on any electron due to its motion are taken into account, the numerical results obtained are a sufficiently close approximation to the truth. The main object of this paper is to obtain a second approximation to the true periods of vibration of a single-ring atom, and thence to deduce, as accurately as possible, the atomic weight of the element designated as nebulium. Bourget, Buisson, and Fabry's experimental investigation, by use of the limiting order of interference which can be obtained from the line λ 5007, has

* *Progymnasmata,* p. 138 (*Opera,* ii. p. 151), and introduction to Catalogue of 1000 Stars, Gassendi Vita Tychonis (Hague edition, p. 248), *Opera,* iii. p. 335.

† *Astronomie ancienne,* i. pp. 117, 172, 184.

‡ *Syntaxis,* v. 5 (Heiberg, pp. 369, 375).