right ascensions and declinations of the comet are given as deduced from the instrumental readings without any correction. In some instances, when the comet was immediately compared with a star, the position of the star is similarly given. On October 2, the instrumental right ascension and declination of θ *Ceti* are also given; but, in general, the corrected positions of the comet cannot be determined till the stars of comparison have been observed on the meridian, and their positions furnished.

XI. Observations of Altitude and Azimuth of the Great Comet of 1843, made at St. Helena. By G. Brand, Esq.

The observations were made with an altitude and azimuth instrument by Gilbert, and they extend from March 6 to March 23. On March 6, the comet was compared with the moon only; on every other day, the altitudes and azimuths of some of the fundamental stars were observed. The length of the tail is stated to be, on March 6, $42^{\circ}\ 55'$; on March 7, $37^{\circ}\ 23'$; and, on March 17, $32^{\circ}\ 47'$. It is stated also, that on March 8, the colour of the tail had changed, and become more like the rays of the moon: on the 15th it appeared to be much brighter.

XII. Extract from the Translation of a Letter from Professor Bessel, dated Konigsberg, 10th of August, 1844. On the Variations of the Proper Motions of *Procyon* and *Sirius*. Communicated by Sir J. F. W. Herschel.

The subject which I wish to communicate to you, seems to me so important for the whole of practical astronomy, that I think it worthy of having your attention directed to it. I find, namely, that existing observations entitle us without hesitation to affirm that the proper motions, of *Procyon* in declination, and of *Sirius* in right ascension, are not constant; but, on the contrary, that they have, since the year 1755, been very sensibly altered. If this be so, the observations of the place of a star at two epochs are no longer sufficient to express its place for any indefinite time; but, for this purpose, it is necessary to investigate the law of the change. It follows also from this, that we are yet very far off from the correctness we imagined ourselves to have arrived at in the fundamental determinations of astronomy; and, that a new problem presents itself, whose solution will cost much labour and a long period of time, viz. the problem of determining the special motions of a star. For, even if a change of the motion can, up to the present time, be proved only in *two* cases, yet will all other cases be rendered thereby liable to *suspicion;* and it will be equally difficult, by observations, to free other proper motions from the suspicion of change, and to get such a knowledge of the change as to admit of its amount being calculated.

The earliest suspicion of the want of constancy of the proper motion was derived about the year 1834, from the corrections of the clock-time, which, at this observatory, were registered with every observed culmination of a fixed star and its reduction to the meridian. At that time it began to be remarked in a striking manner, that negative clock-corrections derived from *Sirius* were greater, and positive corrections less, than those resulting from the other fundamental stars.

As the right ascensions of *Sirius*, which are given for the beginning of each year in the *Tabulæ Regiomontanæ*, are obtained by comparison of the right ascension in 1755 with that in 1825, their agreement with the observations of the latter years was complete; but, as early as the year 1835, fifty observations shewed, when compared with the three fundamental stars following *Sirius*, viz. *β* and *α Orionis* and *α Canis Minoris*, that $0^{s}{\cdot}188$ must be added to the *Tabulæ Regiomontanæ*, to make the agreement again perfect. This disagreement has been since still increasing. In the year 1843 I found it, from fifty observations by Dr. Busch with the old instrument, $= + 0^{s}{\cdot}318$, and, from forty, which I made myself with the new meridian circle of Repsold, I found it $= + 0^{s}{\cdot}324$.

A second suspicion of the variability of the proper motions of the stars was awakened in me in the year 1840, by the declination of *Procyon*, since a new determination of all the elements of reduction of its declination, and of the declinations of the other fundamental stars, gave the observed declination of *Procyon* more northerly than that of the *Tabulæ Regiomontanæ*, (the tabular result being obtained by comparison of the observations of 1755 with those of 1820), by $1''{\cdot}64$.* This difference has also increased, since, by observations made with the instrument of Repsold, I find it for $1844 = + 3''{\cdot}18$.

What I have brought forward concerning *Sirius* and *Procyon* depends on determinations, whose certainty I esteem as great as can be attained by the present apparatus for observing. At the same time it does not cease to be necessary to subject the important result here given to the strictest scrutiny, by means of all the existing determinations of other observatories, before it can be received as the indisputable result of observation. I would communicate the result which this investigation has produced; but I should go beyond the limits of a letter, should I here give place to the criticism to which some of the numbers must be subjected, before they can be received as valid. Since this is, nevertheless, not the less necessary, I must refer to a paper which will very soon appear on this subject in the *Astronomische Nachrichten*. It is plain that we can obtain for the declination of *Procyon* comparative results from the different catalogues, only by eliminating the constant errors, which, without doubt, affect all the observations up to the present time, and frequently to the amount of several seconds. This I found to be the case by subtracting from the difference between every determination of *Procyon* and the *Tabulæ Regiomontanæ*, the mean of the differences of the results for eight stars, *α Ceti*, *α Orionis*, *β Virginis*, *α Serpentis*, *γ*, *α*, *β Aquilæ*, and *α Aquarii*, the mean of whose declinations is the same as that of *Procyon*, within a very

* *Astr. Nachr.* No. 422.

few minutes. By this means it will be gathered that the following collection of results does not depend upon the absolute declination of this star and the absolute right ascension of *Sirius*, but upon the relative declination and right ascension of each respectively, as founded on the comparison with the above-mentioned eight and three stars.

1. *Relative Declination of Procyon.*

Fundamenta Astron.	1755	″ 0·00	
Maskelyne............	1770	+1·54	For the most part but few observations of the stars; probably not altogether free from errors of observation.
Piazzi	1800	+1·99	New edition of the Catalogue. The results of separate years, which are supplied in the old edition (Supplement), and the determination from the later observations in Libro vi. à Spec. R. di Palermo, give a yet somewhat greater difference.
Bessel	1820	0·00	
Pond I.	1822	−0·03	Mr. Pond's own catalogue for this year.
Pond II.	1822	+0·16	M. Olufsen's computations, *Astr. Nachr.* No. 73.
Struve	1824	−0·15	
Argelander	1830	+0·03	
Airy	1830	+0·47	This is the mean of seven very beautifully accordant results derived from the observations from 1829 to 1835. It agrees tolerably well with the Catalogue of 1112 Stars; but deserves the preference, since the latter in the event of a variable proper motion cannot be correctly reduced to 1830.
Pond	1832	+0·84	
Henderson	1833	+0·89	
Konigsberg Obs. ...	1838	+1·59	*Astr. Nachr.* No. 422.
Ditto, with the Repsold circle	1844	+2·62	

2. *Relative Right Ascension of Sirius.*

Fundamenta Astron.	1755	s 0·000	
Maskelyne	1767	−0·079	Derived from a new reduction of the observations; this result differs by −0·288 from that contained in the Catalogue for 1770; the reason of which I do not know.
Piazzi	1800	+0·033	
Maskelyne	1806	+0·016	Derived from a new computation of the observations of 1803, and agreeing nearly with the Catalogue for 1805.
Bessel	1815	−0·036	
Pond	1819	−0·083	Derived from a new computation of the observations, but agreeing with Mr. Pond's own result.
Bessel	1825	0·000	
Struve	1825	−0·006	
Argelander	1828	−0·003	
Airy	1830	+0·049	This is the mean of seven results derived from the observations from 1829 to 1835; it is, for reasons mentioned above, preferred to the result derived from the Catalogue of 1112 Stars.
Pond	1832	+0·084	
Konigsberg Obs. ...	1835	+0·188	
Ditto, with both instruments ...	1843	+0·321	

These tables shew that the determinations for 1820 and 1825 are fully corroborated by means of nearly contemporaneous observations at other observatories, as indeed the care which has been bestowed on all these observations would lead us to expect. They leave, besides, no doubt of the continual increase of the difference from the *Tabulæ Regiomontanæ*, from 1820 to the present time. This continual increase can be explained on the supposition of an unchanged proper motion only by attributing errors to the determinations for 1755, 1820, and 1825, respectively, of sufficient magnitude to make the *relative* declination of *Procyon* for 1755 to appear to be 7″ in error, and the *relative* right ascension of *Sirius* for 1755 more than a second of time. That so great errors cannot exist is proved by the different checks in the *Fundamenta Astronomiæ*; but Lacaille's and Tobias Mayer's cotemporary results leave no doubt on this point, although they cannot determine it within 2″ in declination and 1-4th of a second in right ascension. I regard also as a result rendered certain by the observations, that the supposition of an *unchanged* proper motion in the case of the relative declination of *Procyon*, and in the case of the relative proper motion of *Sirius* in right ascension, is shewn to be false.

The law of the change of each of the two motions is not yet known with sufficient exactness by the observations given. If Piazzi's determination of the relative declination of *Procyon* is correct (as I believe it to be),* then has the difference, between 1755 and 1820, reached a positive maximum. In the case of the right ascension of *Sirius*, I have sought to obtain a more approximate knowledge of its change, through the following up of the results of the observations with the transit instrument which Pond (vol. for 1811–12) published, and by a new reduction of Maskelyne's observations. This has given a positive maximum of the difference from the *Tabulæ Regiomontanæ* of about $0^s.3$, between 1790 and 1800; but since the pivots of the axis were unluckily proved to have been injured, and were corrected in 1803, which correction produced a significant effect on the subsequent observations of right ascension of *Sirius*, it cannot be *certainly* maintained that the maximum was produced in reality by the motion of *Sirius*, and not, at least in part, through the defect of the instrument. In the meanwhile it follows, from what has been advanced at present, and from the tables, that a period, not very different from that of a half century, would serve in both cases for a sufficient explanation of the observations. I think it, however, expedient in the present state of the subject, to wait for a further developement of the nature of the change, from the observations of the next half century, before pronouncing a judgment thereupon. This has, besides, no real value for the objects of astronomy before the nature of the motion of *all* the stars of the fundamental list are known.

I have investigated the conditions which must be fulfilled, that a sensible change of the proper motion, like that observed, may be capable of explanation by means of a force of gravitation. If the star that exhibits it be represented by S; an attracting mass by

* That of Maskelyne for 1770 has little weight.

m_n, and the corresponding star by S_n; the sun by O; the distance SS_n by r_n; O S_n by r_n'; O S by ϱ; the angle at the star S by s_n; the angle made by the plane O S S_n with the plane of motion of the star S by u_n, we easily find the expression of the second differential co-efficient of the apparent motion of the star, with respect to the time,

$$= \frac{m_n}{r_n r_n} \cdot \frac{1}{\varrho}\left(1 - \frac{r_n^3}{r_n'^3}\right) \sin s_n \cos u_n,$$

and the half of this is the expression for the real motion in a unit of time, answering to a uniform motion with the velocity at the commencement of that unit of time. Take now a century as the unit of time; the sun's mass as the unit of mass; the distance divided by sin $1'' = 206265$ times the mean distance of the earth from the sun as the unit of distance; then this expression becomes

$$= 0{\cdot}00000464 \cdot \frac{m_n}{r_n r_n} \cdot \frac{1}{\varrho}\left(1 - \frac{r_n^3}{r_n'^3}\right) \sin s_n \cos u_n.$$

Since the motion resulting from the foregoing observations is from ten to fifteen millions of times greater than this, it is necessary, for the explanation of it, by means of the attraction of *one* mass,

1. That either m_n be very great; or,
2. That r_n be small, that is, the attracting mass very near to the disturbed star; or,
3. That r_n' be very small, that is, the attracting mass very near the sun. The smallness of ϱ, the distance of the star in motion, from the sun, does not produce this effect, since it can be regarded as a factor of $1 - \frac{r_n}{r_n'}$. But, what *one* mass cannot effect,
4. The joint action of millions of existing stars might produce.

On the supposition that the hypothesis (1) is the true one, but is not connected with (2) or (3), the change of motion which the observations since 1755 have shewn, must have existed during a long space of time with a similar amount and direction; for the relative positions of O, S, S_n, change during this time, by the smallness of the existing motions of the sidereal system, so little, that it does not enter at all into the consideration. The change of motion must also increase proportionally to the square of the time, and much greater values are obtained than are consistent with the numbers of Hipparchus: I find, for example, that this increase of the present observed change of the motion of *Sirius*, would alter its right ascension, in 2000 years, by more than three degrees. Independently of this contradiction of the most ancient observations, there is also very little probability that we should be living precisely at that time when a great proper motion of a fixed star had become changed into a motion in the opposite direction, and again becoming great.

But it would be yet far less likely that this circumstance should take place in two cases independent of each other. One is justified, then, both by observations and probability, to fall back upon the explanation (1), with the exclusion of (2) and (3).

Against the explanation (4) the same objections sufficiently hold.

If (3) were the right supposition, a mass existing so near the sun would produce great irregularity in the motions of the planets, which we do not observe to be the case.

There remains then the explanation (2) alone. Stars, whose motions, since 1755, have shewn remarkable changes, must (if the change cannot be proved to be independent of gravitation) be parts of smaller systems. If we were to regard *Sirius* and *Procyon* as double stars, the change of their motions would not surprise us; we should acknowledge them as necessary, and have only to investigate their amount by observation. But light is no real property of mass. The existence of numberless visible stars can prove nothing against the existence of numberless invisible ones. There have been also stars which seemed to possess the peculiarity of a bright body passing over, and which have again lost it; for example, the star of *Tycho*. The phenomena then of the varying motions of the stars, which are so important for the results of plane astronomy, seem also to possess interest in relation to our knowledge of the physical constitution of the universe.

(Signed) F. W. Bessel.

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