VI. *Appendix to the preceding Paper on the changes which appear to have taken place in the declination of some of the fixed Stars. By* J. Pond, *Esq. Astronomer Royal, F. R. S.*

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The observations which have been made during the last summer, confirm in a very decided manner the results which formed the subject of my last communication; in which I laid before the Society the nature of the differences that exist between the computed places of the principal Stars of the Greenwich Catalogue, and those deduced from actual observation. It is not my present intention to offer any explanation of the cause of these phenomena, although many obvious conjectures present themselves, the value of which it will require perhaps many years to determine. It is now my principal object to consider the force of that explanation of the differences in question, which will most readily occur to every astronomer, namely, that the whole may arise either from error committed by the observer, or from defect in the instruments of observation: this objection being the more weighty from the circumstance, that the observations of three distant periods are employed, and that an error in those of either period (but particularly of the two latter) would materially affect the result now under consideration.

I believe that every person, in proportion to his experience in the use of astronomical instruments, (even of the most unexceptionable construction), will be cautious in admitting the accuracy of any results, with whatever care the observations may have been made, which appear to militate against any received theory of astronomy; and I shall have occasion myself to show, from the great discordances between instruments of the highest reputation, that this distrust is but too well founded. More particularly ought our suspicion to be excited, when such anomalies are found to exist, as bear some direct proportion to the zenith distances of the stars observed. In all such cases we should never hesitate, I think, to ascribe the anomalies to defective observation. If therefore in the present instance, any part of the discordances in question can be shown to depend on polar or zenith distances, I shall willingly admit, as to such part of them at least, that they are no otherwise of importance, than as affording data for leading to the detection of some hitherto undiscovered errors. The anomalies, however, that have led me on to this enquiry, and to which alone I attach any importance, are found to depend rather on the right ascensions, than on the declinations of the stars. Accordingly I found, while collecting observations to form a catalogue for the present period, that I could more nearly predict the deviation of a star from its computed place, by knowing its right ascension, than its declination. Now it is not easy to conceive in what way the error of an instrument for measuring declination, fixed in the meridian, can be occasioned by any circumstance depending on the right ascension of a star to be observed.

The general nature of the deviation of the stars from their computed places will be best understood from the annexed tables; in one of which the principal Stars of the Greenwich Catalogue are arranged according to north polar distance, and in the other, in the order of their right ascensions.

From these tables it will appear, according to my statement in the former part of this paper, that the general tendency of the deviation is towards the south: that in about one-third part of the heavens in right ascension this southern tendency is very inconsiderable, and would hardly have excited attention: for in this part, stars between the zenith and the pole, appear a very small quantity to the northward; whereas in the remaining, and most considerable portion of the heavens, every star appears to be a considerable quantity to the south of its computed place; and with few exceptions, the more southward stars have a greater tendency to deviation than the northern ones.

If we select from the preceding tables, those stars which were least frequently observed, at one or all of the three periods, we shall find that they all tend to confirm the foregoing general results; though they must be regarded as doing so, rather by their united effect, than by their weight of evidence when considered singly. Stars that have been but seldom observed, give results considerably affected by accidental error of observation; which error is quite of a different nature from that produced by permanent defect in the instrument, and which repetition of observation has no tendency to remove.

If the deviations of those stars that have been imperfectly observed, were attributable either to error of observation, or defect in the instruments, the deviation would either follow no law at all, or some law depending upon zenith distance: but the facts we have seen to be at variance with either of these hypotheses. Not however to rest satisfied with these considerations drawn from the general tendency of all the stars without exception, let us select some striking examples of deviation, in particular groups of stars, on which we might be satisfied to rest the issue of this question. Of these groups I have marked *five*, in the table of stars arranged according to north-polar distance, each of which we will take the pains to consider more attentively.

1. There are six stars in my Catalogue north of γ Draconis, of which three are found to the north, and three to the south of their computed places. These inequalities may appear at first sight to be wholly accidental; but if we pay attention to the right ascension, we shall find that the three which appear to the northward, are situated in that part of the heavens as to right ascension where the southern deviation is the least perceptible, and that the three which appear to the southward, are in that part as to right ascension where the southern deviation is the greatest. But of these six stars there are two, α Cassiopeiæ, and γ Ursæ Majoris, which deserve farther consideration. These two stars are within less than one degree of each other in polar distance, and consequently pass over the meridian at nearly the same altitude. The observations of Bradley on the stars north of the zenith are not so numerous as could be wished; but each of the two stars in question was observed by him about five times towards the year 1753; that is 60 years from the date of my catalogue of 1813. I have carefully recomputed the predicted places of these stars, and I find α Cassiopeiæ not less than $1'',5$ to the south of its predicted place, and γ Ursæ Majoris half a second to the north. Now I am quite at a loss to conceive how this difference in so small an arc can arise from error of observation, and I can only attribute it to that cause, whatever it may be, which

seems so generally to depend not on the polar distance, but on the right ascension of the star.

2. The second group which I shall consider, contains the stars α Arietis, Arcturus, and Aldebaran, comprehended within an arc of about six degrees and a half. Of these three, Arcturus alone has yet been observed by reflection; but from the present very perfect state of the Greenwich circle, which the method of reflection has enabled me to ascertain, it cannot be doubted that the places of the two other stars are well determined.* In Arcturus the southern deviation is nearly insensible, but in the two other stars it is very considerable, being in each not less than 1″,5. Now these three stars, but particularly the two latter, are among those that have been most assiduously observed by BRADLEY and myself, at each of the three periods. Let us suppose then, if it be possible, that the whole of these deviations arise from error of observation; or in other words, that no systematic deviation has really taken place in the stars, but that their proper motions are uniform. Then we must admit that the mural quadrant and the mural circle have at each period given the polar distance of Arcturus correct, or at least subject to the same constant error; and as this star has been observed at each period, at all times of the day, and at all seasons of the year, the observations may be considered as perfectly exempt from accidental error. It will I believe be readily conceded that both instruments are so far perfect, that if the error be either nothing, or a given quantity at one point of the arc, the errors must be very nearly indeed the same within a moderate distance, as within 15 degrees, for instance, of that point. Upon this supposition, how can we possibly reconcile the great errors that must

* This has been confirmed by subsequent observation.

have been committed in stars, adjacent as to polar distance, but of opposite right ascensions? I do not wish to press these remarks, in order to obtain greater confidence than they deserve, for observations which can never be regarded with too much suspicion; but the arguments I have used, appear to me to follow logically from the data before us, and strongly to indicate the probability that some cause purely astronomical has, at least, some share in producing these unexpected deviations.

3. The third group, α Herculis, α Pegasi, and Regulus is still more remarkable, being comprehended within two degrees of declination, and two of the stars, α Herculis and α Pegasi* being within half a degree of each other. In this group α Pegasi is at least 3″ south of its predicted place, whereas the other two stars have not deviated much more than 0″,5 to the south.

4. α Orionis, α Serpentis, and Procyon, furnish an example equally striking, they being within less than 2° of declination from each other; α Serpentis is exactly in its predicted place, while α Orionis and Procyon are each of them at least 2″ to the south.

5. Rigel, Spica Virginis, and Sirius, are not contained within so short an arc as the former groups, nor are their places so well determined, on account of their proximity to the horizon; but they afford another instance of the inequality of southern deviation, in stars having nearly the same polar distance, but opposite right ascensions.

But leaving the considerations suggested by these groups of stars, let us examine more minutely the different hypotheses that may be formed on the supposition, that the whole

* The lunar nutation of α Pegasi was nearly a minimum at each period.

of these deviations depends on error of observation caused by some defect in the instruments employed: this investigation becomes the more necessary, as it does not appear that Dr. BRINKLEY, with his instrument at Dublin, has met with similar discordances. Admitting the accuracy of the observations of BRADLEY to form the ground-work of this enquiry, there are then two distinct hypotheses, that may be formed by those, who are inclined to maintain, that the proper motions of the stars are uniform; and that the discordances in question have their source, not in any astronomical cause, but in some erroneous system of observation. Of the observations from which the catalogues of 1813 and of the present year have been computed, we may suppose the one or the other to be erroneous. Let us consider the consequences of each hypothesis.

Let us first suppose the error to be in the observations of 1813. Then the observations of 1756 and 1822 being supposed perfect, a catalogue for the year 1813 may be computed by interpolation; such a catalogue is annexed, and this, (assumed to be correct,) compared with the observed catalogue of 1813, will show the errors of observations at that period. On this assumption the Greenwich circle must, in 1813, have been in a very defective state; and admitting the instrument to be now perfect, this can be only attributed to the insufficiency of the braces which then connected the telescope to the circle; for this is the only difference between the instrument in its former and in its present state. The natural tendency of any such defect would be, I think, continually to increase, and to give results every year more and more distant from the truth: but this is contrary to the known history of the Greenwich observations, which I have found gradually for some time past approaching to those results which are obtained at the present day, and which, according to our present hypothesis are supposed to be nearly perfect. If the catalogue of 1813 were really so erroneous, as our present hypothesis would compel us to regard it, then it would appear that Dr. BRINKLEY'S catalogue for the same period must have been still more erroneous, as may be seen by inspection of the annexed tables. Now admitting for a moment that there were at that time certain imperfections in the Greenwich and Dublin instruments, no person will believe them to have been so imperfect as our present hypothesis would tend to represent them.

Let us now examine the second hypothesis, which presumes the catalogue of 1813 to have been perfect, and consider what confidence is due to the Greenwich observations of the present day. This investigation is to be regarded as important, not merely with a view to the discussion of the nature of the discordances in question, but also from the circumstance, that instruments of well-known celebrity are represented as giving very different results; for which reason I shall be excused for entering into considerable details on this particular question. As the principal reliance I place on the accuracy of the present catalogue, and on the superiority of the Greenwich circle over all other instruments, with the history of which I am acquainted, is derived from the coincidence of the results obtained by the two independent methods; the one of direct measurement of polar distance, the other of observing the angular distance of the direct and reflected image of the stars, it becomes of some importance to consider in what way this coincidence is a proof of the accuracy of either. The source of error the most to be dreaded in every instrument whatever, quadrant or circle, is that which will be caused by the flexure of the materials of which the instrument is made. It is impossible in theory that any instrument can be wholly free from this defect. In the Greenwich circle the number of microscopes placed round its crcumference have an obvious tendency to diminish this error, though they cannot annihilate it; but they have no tendency whatever to diminish the error arising from the flexure of the telescope attached to the circle.

The effect of flexure in any circle will be, in the first instance, to give an erroneous distance from the pole to the zenith: in instruments that turn in azimuth, of the usual construction, the error thus occasioned will be applied to every star under the form of co-latitude, and a star south of the zenith, will be moreover affected by the probably opposite flexure due to that point of the instrument on which the star is observed. This in stars near the equator, or a little to the northward of it, will in our latitude give an error in polar distance, amounting to about double the error committed in determining the co-latitude. On the contrary, the polar distances of stars north of the zenith, being affected only by the difference of two flexures, will be more accurately determined as they approach nearer to the pole, where the errors will wholly vanish. Now, though in the usual mode of employing the Greenwich circle, viz. in measuring directly polar distance, the co-latitude does not become an object of enquiry, yet any flexure of the circle will produce a system of errors of the same nature as those above pointed out. In instruments, like that of Dublin, which turn in azimuth, and with which the observer has to find the place of all the stars by measuring the double of their zenith distances, if he does not find the same zenith point with different stars (provided the instrument be well divided) he may be sure that flexure takes place; but he cannot infer the converse, that flexure does not take place, from his obtaining with all the stars the same error in the line of collimation. For if the flexure be the same on both sides of the zenith, a supposition by no means improbable, the observer will then have no indication of flexure by the usual method of determining the error of collimation by stars of different altitudes. Let us suppose that, with an instrument liable to flexure, it is required to measure by both methods the meridional distance of any two stars. The angular distance of the direct images will (as we have already seen) be affected by the difference, or by the sum of two flexures, according as the stars are placed on the same, or on opposite sides of the zenith. In viewing the reflected images, the instrument receiving two new positions, will be subject to two new flexures, by the sum or difference of which (as it may happen) the angular distance of the reflected images will be affected.

The most probable supposition to be made concerning the flexures is, that at equal inclinations with the horizon, above and below it, they will be the same nearly both in direction and degree, and therefore that the two images below the horizon will approach by nearly the same quantity that the direct images receded, or vice versâ. With an instrument therefore having such a system of flexures, the double altitude of each star will be correctly ascertained; but stars of different altitudes will give different determinations of the horizontal point. From observations thus obtained, a near approximation to the true angular distance might be inferred, by taking a mean between the distances of the direct and of the reflected images. The least probable supposition concerning the flexures is, that at equal inclinations above and below the horizon, they will be equal, but in opposite directions; the consequence of which would be, that the direct and reflected images would approach to or recede from one another by the same quantity: the double altitudes of each star would be incorrectly given, but every star would give the same determination of the horizontal point. To suppose however the existence of such a system of flexures, would be to suppose that gravity produced the same change of form in the instrument, as if its direction were inverted; and since the horizontal line is that, at which according to the supposed system a contrary flexure will take place, the flexure at or near the horizon should be zero, where, however, according to the known laws of mechanics it ought to be the greatest. Such a system therefore must be considered as mechanically next to impossible.

If then an instrument give the angular distances both by reflection and by direct vision the same, and the same determination of the horizontal line from stars of whatever altitude, there are then only two hypotheses that can be formed respecting such an instrument; either that the flexures are insensible, or that they are such as are absolutely inconsistent with the laws of mechanics. Hence I conclude that the coincidence of the results by direct vision and by reflection, and the uniform determination of the horizontal point, will be the strongest proof of the non-flexure of the instrument, and of the accuracy of both results.*

* I must also notice that the method by reflection possesses, in common with

In illustration of the whole of the preceding observations let us examine two catalogues, those of Dr. Brinkley, and Mr. Bessel, which have lately much excited the attention of astronomers. It is obvious, by merely inspecting these catalogues, a comparison of which with the Greenwich catalogue I here subjoin, that one, or both, of the instruments used by these astronomers must be erroneous; and it seems to me, that the source of error is the very flexure, the nature and effects of which we have been considering. For if we attend to the differences between these two catalogues, we shall find, that the six stars near the equator differ 5″ from one another, whereas the stars near the zenith do not differ above 2″,5. In which direction flexure will effect the zenith distances, is a matter quite accidental, depending on the unequal elevation or depression of the object-end or eye-end of the telescope, in consequence of the unequal strength of the materials. If we suppose error to exist in each of the catalogues, this cause must have had an opposite influence in the two cases: if we compare the Greenwich observations with those of Dr. Brinkley, we shall arrive at the same conclusion; namely, that the differences must be caused by flexure in one or both of the instruments; since here also we find that the stars in the neighbourhood of the zenith are affected by only half the difference in polar distance, that is observed in the stars near the equator; and the same conclusions may be drawn from comparing the Greenwich observations with those of Mr. Bessel. The polar distances of all the stars in Mr. Bessel's catalogue exceed the

instruments turning in azimuth, the advantage of measuring the double of the required angle.

polar distances given in the Greenwich catalogue; while those of all the stars in Dr. BRINKLEY's catalogue as regularly fall short of my determinations. It is not from the casual circumstance of my results being nearly a mean between the results of those two astronomers, that I intend to claim a superior weight of authority for my own; for were this the only ground for preference, I should regard the question as yet undetermined, and should think it my duty to recommend the providing of new and more powerful instruments for ascertaining the truth. But it appears to me that from the observations by reflection, which I have lately made, and from their agreement with my observations by direct vision, that I am entitled to determine the share of error to which each of these two catalogues is liable; not only from the general superiority of the Greenwich circle, which I consider to have been thus proved, but from this peculiar circumstance, that whereas in the two catalogues of Mr. BESSEL and Dr. BRINKLEY, the errors cannot fail to be the greatest in stars near the horizon; by my method of reflection those stars, which are nearest the horizon, must be determined the most correctly, from their double altitudes being measured on the smallest arc.

In stars near the equator the catalogue of Mr. BESSEL differs from that of Dr. BRINKLEY five seconds; and from the preceding considerations, I think we may venture to conclude that Mr. BESSEL's polar distances are too great by about three seconds, and Dr. BRINKLEY's too small by about two: and since my catalogue differs from the two former from the zenith to equator in very nearly the same proportion, there can be no reason to doubt that their errors throughout are divided in nearly the same ratio.

With regard to the catalogue for the present period, which accompanies this paper, I beg to state that I consider it only as a very near approximation to the truth, and requiring at least another year's observations, to render it of equal value with that of 1813, which is the result of two years observations with six microscopes, and in four positions of the telescope.

I am persuaded that the more this subject is considered, the more distinctly it will appear, that if any doubt can be entertained, founded on any circumstance arising out of the Dublin observations, that doubt must relate, not to the accuracy of former catalogues, but to the present position of the stars; since it is with respect to their *present* position that the two instruments are really at variance. This circumstance is very fortunate, as time may confirm the present, or suggest some more satisfactory method of investigation, if what I have now advanced be not thought sufficient for the purpose.